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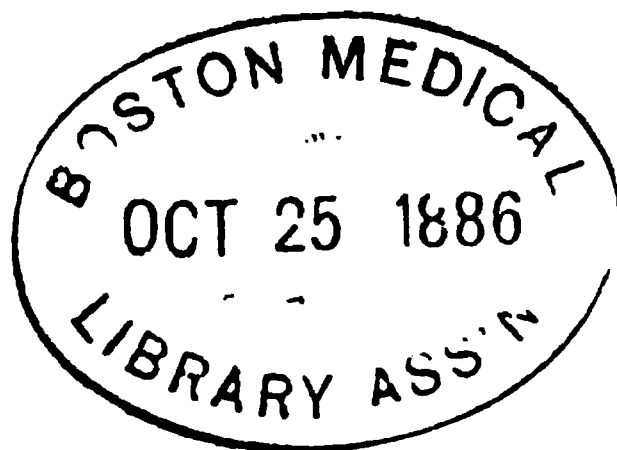
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THE  
MONTHLY MEDICAL JOURNAL



Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Contributions to the Pathology of the Heart and Arteries.—Injury of the Valves of the Heart from violent muscular exertion.* By THOMAS B. PEACOCK, M.D., Assistant-Physician to St Thomas's Hospital, London.

It has been frequently remarked that, though the valves of the heart would appear to be peculiarly exposed to sustain injury during violent muscular exertion, such accidents rarely occur, and indeed it is remarkable how few cases of the kind have been placed on record. Corvisart, after alluding to several cases quoted by Senac, in which the latter supposes disease of the heart may have so originated, states that he was not aware of any well-ascertained examples of injury of the valves from muscular effort which had been published before those which he has related. Of the cases which he has recorded, the first<sup>1</sup> is that of a man, thirty years of age, who, after performing a long journey on horseback, was suddenly seized, while passing from Dover to Calais, with difficulty of breathing and spitting of blood, and died with symptoms of cardiac disease in about nine days. A recent rupture of one of the fleshy columns of the mitral valve was found on examination after death, and there does not appear to have been any other disease of the heart. The second case,<sup>2</sup> related by M. Corvisart, is one which had been previously reported by M. Marat.<sup>3</sup> The patient was a turner, forty-four years of age, who, while rolling a heavy cask, experienced a severe strain in the back, with sense of suffocation and pain between the shoulders, followed by palpitation and other symptoms of cardiac disease, under which he sank in twenty months from the

<sup>1</sup> Sur les Maladies du Cœur, 3me edit., 1818, Obs. 40, p. 274.

<sup>2</sup> Ibid., Obs. 41, p. 278.

<sup>3</sup> Journal de Med. cont., T. 6, p. 587.

date of the accident. One of the fleshy columns to which the cordæ tendineæ of the mitral valve are attached, proved after death to have been torn across;—the date of the injury apparently corresponding with that of the strain. An aneurism of the aorta also existed in this case, but there had been no evidence of disease of the circulatory organs before the accident. In 1824, Bertin<sup>1</sup> related the case of a young female, who died in the third stage of phthisis, after having suffered from severe fits of coughing, in whom one of the muscular columns of the tricuspid valve was found lacerated across; and Dr Townsend,<sup>2</sup> referring to this observation, states that he has seen three instances of a similar description, though he does not state whether the mitral or tricuspid valve was the seat of injury. In 1839, M. Legendre<sup>3</sup> exhibited to the Société Anatomique of Paris an example of rupture of one of the fleshy columns to which the posterior fold of the mitral valve is attached. The patient had sustained a fracture of the ribs from pressure, and this was followed by symptoms of disease of the heart, of which he died; but the period which elapsed between the occurrence of the accident and his death is not stated. In 1846, Dr Latham published, in the second volume of his valuable clinical lectures on diseases of the heart, the particulars of two cases of injury of the valves from muscular effort. The first of these occurred in the practice of Dr Bence Jones. A stableman, twenty-eight years of age, while running a horse in the yard to show off his paces, suddenly experienced severe palpitation, followed by symptoms of imperfection of the aortic valves, which proved fatal in thirteen months from his first seizure. On examination after death, the attachment of two of the aortic valves was found broken down, so that the two corresponding sinuses of Valsalva were converted into one. The subject of Dr Latham's second case was a gentleman, in the prime of life, who, while rowing, experienced a sudden shock in the region of the heart, followed by symptoms of excitement, for which he was actively treated. When seen by Dr Latham, about twelve months after the occurrence of the injury, there were evidences of hypertrophy of the heart, and a loud systolic murmur was audible at the præcordia but not in the arteries, and was regarded as dependent on incompetency of the mitral valve. The patient still survived after an interval of two years from that time. In addition to these cases, Dr Latham refers to several others, in which symptoms of cardiac disease followed violent efforts, and were probably dependent on injury of some portion of the structure of the heart, though of what nature could not be ascertained. About the same period at which Dr Latham's notice of this subject appeared, Dr R. Quain published a memoir on injuries to which the aortic valves are liable during muscular efforts,<sup>4</sup> which had been read at

<sup>1</sup> *Traité des Maladies du Cœur*, Obs. 31, p. 52.

<sup>2</sup> *Cyclop. of Practical Medicine*, Art. Rupture of the Heart.

<sup>3</sup> *Bullet. de la Société Anatomique*, 14me année. 1839.

<sup>4</sup> *Edinburgh Monthly Journal of Medical Science*. 1846,

the first meeting of the Pathological Society, in the month of October 1846. In this paper, Dr Quain relates three cases, in addition to that of Dr Bence Jones, just referred to. The first case is that of a music smith, who, after making a few vigorous blows with a sledge, was seized with a distressing sensation in the region of the heart, which immediately arrested his exertions. He was seen by Dr Quain a few days after, and then presented evidences of imperfection of the aortic valves, which gradually became more severe, till he died in August 1843, two years after the accident. He had never previously had any symptoms of cardiac disease, nor had he suffered from rheumatism. On examination after death, the heart was found very greatly hypertrophied, weighing  $22\frac{1}{2}$  oz. avoirdupois; and the attachment of two of the valves had been torn from its point of union with the coats of the aorta, and allowed two of the curtains to drop below the level of the third. The second case related by Dr Quain was one communicated by Dr Jones Quain. A porter, resident in Paris, while in good health, endeavoured, in a state of excitement, to force open a door, and suddenly felt a sense of oppression at the chest, and when seen by a medical man, presented the usual signs of incompetency of the aortic valves. He died in eighteen months, and the convex margin of one of the valves was found to have been torn from its attachment. The third case is that of a carpenter, fifty-four years of age, who, like the others, laboured under symptoms of imperfection of the aortic valves, and stated that his illness had commenced five months before with sense of oppression, pain in the region of the heart, and palpitation, which came on suddenly while he was stooping to lay down a load of timber which he was carrying. He continued to survive when Dr Quain's memoir was published. Since this period, Dr Rawson<sup>1</sup> of Liverpool has related the case of a poacher, who, while leaping over a fence, felt something give way in the chest, and died in twenty-one days, having presented symptoms sufficiently indicative of injury of the aortic valves, though no post-mortem examination was made.

These few cases are all which, on a somewhat extended research, I have been able to find recorded of injury of the valvular apparatus of the heart from severe muscular effort.<sup>2</sup> It is true that most systematic writers enumerate violent exertion as among the causes of cardiac disease, and there can be little doubt that it frequently operates as a predisposing cause. In the absence, however, of a larger number of recorded cases, it may be safely inferred, that immediate injury of the valves from muscular exertion is by no means of frequent occurrence. Dr Ormerod, in his elaborate statistical investigation of the causes of valvular disease of the heart, while he alludes to accident as an ordinarily assigned cause, does not include any such case in his table; and of sixty-eight fatal cases of which I possess records, that which I am about to narrate

<sup>1</sup> Medical Times, vol. ii., 1850, p. 415.

<sup>2</sup> I of course exclude from this enumeration cases of direct violence, where, coincidentally with other severe injuries, the valves have been found ruptured.

is the only instance in which disease of the heart had its origin in injury. In speaking, however, of the rareness of rupture of the valves during muscular exertion, I must be understood to refer only to the occurrence of the accident in persons previously in sound health; for, since the time of Sandifort and Meckel, pathologists have been familiar with the frequent occurrence, during active exertion, of laceration or rupture of diseased valves, and the sudden aggravation of the symptoms so occasioned. Baillie<sup>1</sup> has described and figured a specimen of ruptured aortic valve, existing in Dr Hunter's museum, which was probably of this description. An interesting case is related by Mr Allen Williams:<sup>2</sup>—"A man, forty-one years of age, who had presented signs of hypertrophy of the heart since an attack of rheumatism in early life, while engaged in violent exertion, felt something give way in the chest, became faint, and suffered from palpitation, etc., under which symptoms he sank in 110 hours. The heart was double its natural size, and the columnæ carneæ of the mitral valve were cartilaginous, and the aortic valves thickened and ossified; but the immediate cause of death was found to be seated in the tricuspid valve, which, though otherwise healthy, had sustained a laceration of one of its folds to the extent of half an inch in length." Mr Adams<sup>3</sup> has related the case of a musician, thirty-four years of age, under the care of Dr Cheyne, who was suddenly seized with aggravated symptoms of disease of the heart, and died in about six weeks. In addition to extensive hypertrophy and valvular disease, especially on the left side of the heart, "the cordæ tendineæ, which connect the larger portion of the mitral valve, were torn off just at their point of insertion into the valve." M. Nicod<sup>4</sup> has related the case of a lady, who had been previously liable to palpitation, and suddenly experienced a sense of suffocation while walking, which, though relieved by bleeding, recurred in a few days, and proved fatal. On examination after death, in addition to old disease of the mitral valve, two of the fleshy columns were found torn across, and the appearance corresponded with the periods at which the symptoms had become aggravated. It is, however, needless to enumerate cases of this description, as most practical physicians have observed similar instances in their own practice. It occasionally happens that symptoms of valvular disease follow the occurrence of injuries of the chest, giving rise to endocarditis, as in the cases related by Dr Abercrombie,<sup>5</sup> and more recently by Mr Marson. All such cases are, however, very different from those before referred to, in which the valvular injury is the immediate result of violent exertion.<sup>6</sup> After this brief allusion to

<sup>1</sup> Morbid Anatomy, Pl. 2, fig. 2.      <sup>2</sup> Lond. Med. Gaz., vol. iv., 1829, p. 78.

<sup>3</sup> Dublin Hospital Reports, vol. iv., 1827, p. 404.

<sup>4</sup> Journ. Hebdom. de Med., tom. iv., 1834, p. 42.

<sup>5</sup> Med.-Chir. Trans. of Edin., vol. i., 1824, p. 50.

<sup>6</sup> There is frequently found a peculiar condition of the semilunar valves at the aortic orifice, which has been supposed to depend on rupture of the angle of attachment of two of the segments, and which, if really so produced, would show that accident to be of frequent occurrence. I allude to the cases in which the



the literature of the subject, I shall proceed to narrate the particulars of a case of injury of the valves from muscular effort, which has recently fallen under my notice.

William Childs, aged thirty-three, a sailor, was admitted into St Thomas's Hospital, on the 12th of March 1851. He had previously been under my care in November 1849, and then gave the following account of his illness:—About ten months before he left Bona, on the west coast of Africa, where he had been for seven months, and had suffered from the usual endemic remittent fever. Shortly after sailing, he was ordered to go aloft to secure the main-royal, and ran up the rigging, racing against another man. When he had nearly reached the main cross-trees, he was suddenly seized with violent pain in the region of the heart, became faint, and gasped for breath, as if he was dying. In a few minutes these symptoms subsided and he was able to do what was required, and to return to the deck. He still, however, felt faint, and had difficulty of breathing and pain in the left side of the chest. During the remainder of the voyage he was never able to go aloft. He reached home on the 10th of September, but had not had any medical assistance before he applied to me. He stated that he had never had rheumatic fever or inflammation of the chest, and, with the exception of the fever which he had at Bona, had enjoyed good health till the occurrence of the accident.

When he first came under my care, he complained of difficulty of breathing, pain in the region of the heart and palpitation, which were aggravated by exertion, so that when walking he was compelled to stop frequently; but while quiet he breathed more freely, and suffered but little pain. His complexion was sallow, and he had become much thinner since the commencement of his illness. The pulse was sharp, and vanished rapidly under the finger, having a regurgitant character; occasionally it was slightly irregular in force and frequency, and it was equal at the two wrists. A to-and-fro murmur was audible over the whole præcordia, but most distinctly at the level of the nipple, and midway between that body and the sternum. It was also very loud along the upper part of the sternum and at the top of that bone, but was very imperfectly heard at the apex. From these observations it was inferred, that when he was first seized the aortic valves had sustained injury, which had rendered them incapable of completely closing the orifice. He was placed upon a mild alterative course, and this was followed by tonic treatment, under which he improved remarkably. In the spring of

aortic orifice is found to be provided with only two valves, one of the segments being generally much larger than the other, and partially divided on its aortic side by a fœnum or ridge, and on its ventricular side by a shallow sulcus or groove. This condition of the valves obtains, however, at the pulmonic, as well as at the aortic, orifice; and it is frequently found in infants who have lived only a few days after birth. The most natural explanation of its production is, that it is due to the fusion of the contiguous sides of two valves, and the gradual atrophy of the septum so produced. It frequently, indeed, co-exists with undoubted malformation of other parts of the heart, and is most probably an intra-uterine change.

1850 I lost sight of him, and subsequently learnt that he had gone to sea. He again came to me in March 1851, and stated that he had just returned from a voyage to the East. He was much worse than when I had last seen him, and was accordingly admitted into St Thomas's Hospital.

The following notes were taken on the 12th of March :—During his last voyage he has been to Calcutta, Mauritius, and Australia. On first sailing he took cold, and, being unfit for hard work, was allowed to act as cook ; but, on the return voyage from Australia, he did full duty as an able seaman. He has been several times ill while away, and had a slight attack of dysentery at Calcutta. He is now thinner and paler than before, and there is a decided icteroid tinging of the conjunctivæ. He complains chiefly of difficulty of breathing and cough, and expectorates a considerable quantity of frothy mucus, mixed with dark blood. He does not complain of palpitation. The tongue is clean ; the pulse but little accelerated ; the bowels are torpid ; and there is some oedema of the lower extremities. The chest is throughout less resonant than natural, and this deficiency is most marked above and on the right side. The respiratory murmur is feebly heard, especially at the right apex ; and there is increased resonance of the cough and voice. Sonorous rhonchus is heard in all parts of the chest on forced inspiration. The præcordial dulness commences above, at the level of the third cartilage. Laterally it extends from the left side of the sternum to the line of the nipple. The apex of the heart is perceived to pulsate in the fifth interspace in the line of the nipple. There is a double murmur heard over the whole præcordia, and along the course of the sternum. Midway between the nipple and sternum the two murmurs are heard very distinctly, but the first or systolic murmur is short, the second or diastolic more prolonged. At the right side of the upper part of the sternum, the first murmur is heard more intensely and is more prolonged than elsewhere ; the second murmur is most distinctly heard at the left side of the middle portion of the sternum. At the apex both murmurs are less audible. The pulse has a decidedly regurgitant character, and succeeds the impulse of the heart by a very perceptible interval. The hepatic dulness commences at a high level, and extends from an inch to an inch and a half below the edges of the ribs. He was directed to take small doses of blue pill, rhubarb, and hyoscyamus, with quinine and iron, and a blister was applied between the shoulders. From this treatment he obtained considerable relief ; the cough became less troublesome ; the expectoration was reduced in quantity and was almost entirely free from blood : and the sallowness of the complexion to a considerable extent disappeared. The bowels acted naturally, he had a fair appetite and was allowed a generous diet, and was able to sit up for a great portion of the day and to take slight exercise. On the 5th of April, when I saw him, he was walking in the ward and spoke cheerfully, and it was with much surprise that I found, on making my visit to the hospital on the 7th, that he was dead. I

then learnt that, on the evening of the 5th, he was seized with severe rigors, and that from that time he became weaker, and died on the morning of the 7th. He retained, however, his intelligence till shortly before his death.

*Post-mortem Examination.*—A slight effusion of firmly coagulated and dark-coloured blood was found beneath the arachnoid membrane covering the upper part of the left hemisphere; it followed the reflexions of the pia mater, dipping down into the intergyral spaces, but did not involve the substance of the brain. A very small quantity of fluid was found in the ventricles, and the brain was healthy.

A small amount of yellowish-coloured serum was contained in each pleural sac. The lungs were throughout sparingly crepitant, and portions, which were distinctly bounded by the interlobular cellular septa, were more or less solid, and of a dark colour. These condensed portions were chiefly situated towards the upper parts of each lung; on section they had a dry appearance, and did not exude much blood or serum on compression. The lungs contained no tubercle. The bronchial mucous membrane was healthy, and the amount of mucus in the tubes not greater than usual.

The pericardium contained a small quantity of fluid. The heart was of large size, and weighed  $17\frac{3}{4}$  oz. avoirdupois. A patch of old lymph, about the size of a shilling, was found on the serous membrane covering the right ventricle, and the right auricle was covered by an irregular layer of a similar description, and several loose adhesions existed between the contiguous surfaces of the aorta and pulmonary artery. The girth of the right ventricle externally was four French inches and six lines, and that of the left ventricle five inches and two lines. The right ventricle was considerably hypertrophied and dilated. The valves were healthy. The left ventricle was very greatly enlarged, both in capacity and in the thickness of its walls, the latter measuring at the base six lines, about the mid-point ten lines, and at the apex three lines. The aortic valves were extremely incompetent, a stream of water poured into the artery flowing rapidly into the ventricle. This incompetency was owing to the imperfect condition of the left and posterior valves, which, instead of having their angle of attachment inserted at the same level as the other valves, were attached fully half an inch below that point, so that the curtains were allowed to drop down and leave a large portion of the aperture unclosed. These valves were also much decurtated, and their free edges were thickened, and the sinuses of Valsalva, especially that between the posterior segment, were of very small size. Above the valves, in the situation at which their angle should have had its attachment, there was a thick and rough plate of bone, an inch in length and three quarters of an inch in width. This plate almost entirely obliterated the orifice of the left coronary artery, and must have interfered with free flow of blood into the aorta. The right side of the left semi-lunar valve and the right valve were much less diseased. A small and irregu-

lar mass of lymph was attached to the lining membrane of the ventricle near its base, and a similar layer covered the anterior fold of the mitral valve; both of these being in the situations upon which the current of blood regurgitating from the aorta must have fallen. At the margin of the free fold of the mitral valve there existed one of those small sacs noticed by Morand, Morgagni, Laennec, Dr Thurnam, and others, to which the name of aneurism of the valves has been applied. In this instance the sac was very small and prolonged, and as it was open at both extremities, it should perhaps rather be described as a tube. It projected and opened into the cavity of the left auricle; but the apertures were too small to have allowed of any material regurgitation. With this exception, the mitral valve was healthy. The lining membrane of the left ventricle was somewhat opaque, and its inner muscular fibres had in some places undergone fibro-cartilaginous degeneration. The cavities of the heart contained firmly coagulated and decolorised coagula. The aorta, throughout its course, was studded with small atheromatous patches.

The peritoneal cavity contained a small quantity of fluid. The liver was large, solid, scabrous, and engorged. Its substance displayed a few masses, varying from a line to three or four lines in diameter, of a pale yellow colour, and distinctly circumscribed, which proved, on microscopic examination, to be portions in a state of advanced fatty degeneration. The spleen was large, measuring seven inches in length and three in average breadth; a fibrinous deposit occupied the centre of its convex side, and passed deeply into its substance. This deposit had a pale buff colour round its circumference, which gradually shaded into a pale red towards the centre. The kidneys, stomach, and intestines, were healthy. The blood from the heart, liver, and spleen, contained an unusual proportion of the pale globules.

*Remarks.*—The case which has just been related does not require much comment, as the symptoms by which it was characterised, when it first came under my notice, were sufficient accurately to point out its nature. Subsequently, after Child's return from India, the dulness on percussion at the upper parts of the chest, the feebleness of the respiratory murmur, and the morbid resonance of the voice and cough, with the bloody expectoration and emaciation, led me to suspect that tubercle had been deposited in the lungs. This inference was not, however, correct, those symptoms being, it will be seen, due to consolidation of the lungs, probably by apoplectic extravasations which had been partially removed. In other respects, the diagnosis during life was fully confirmed by the results of the examination after death.

The cases of rupture of the valvular apparatus of the heart, to which I have referred, amount, together with that now related, to eleven in number. Of these, six were cases of injury of the aortic valves; in four, the fleshy columns of the mitral valve were ruptured; and in one, a column of the tricuspid valve. The cases

alluded to by Dr Townsend are omitted from this enumeration, as the valves affected are not mentioned; and indeed, as the subjects of these cases, and that of M. Bertin, were patients in the last stages of phthisis, it may be doubted whether there was not some disease of the tissue of the heart, which may have rendered it more readily lacerable. As regards the precise injury sustained, one of the patients in whom the aortic valves were injured was still surviving when the report was published; and in a second, no examination was instituted after death. Of the other four cases, in two the injury consisted in an entire breaking down of the angle of attachment of two of the valves; in my own case, though the angle was ruptured, it had probably never been entirely detached from the coats of the aorta, and had acquired a fresh connection at a lower point; and, in the fourth case, the convex margin of one of the valves had given way. Of the cases of injury of the mitral valve, in one the patient was still surviving; in three, the fleshy columns of the valve were torn across, and the corresponding cords were found floating loosely in the ventricle; and in the fourth, a fleshy column of the tricuspid valve was ruptured. It thus appears that, as would be anticipated from their structure and exposure to the pressure of the column of blood in the aorta, the aortic valves most frequently sustain injury during violent muscular exertions. Indeed, were it not for the peculiar mode in which these valves are attached to the fibrous zone, and to their connection with the longitudinal and elastic coats of the aorta, such injuries could scarcely fail to be of frequent occurrence.

The period of life at which the accident occurs is that during which violent exertions are most frequently made. Thus in the cases in which the aortic valves were ruptured, the ages of the patients were twenty-six, twenty-eight, thirty-three, and fifty-four; and two, whose ages are not reported, were both adults. Of the patients who sustained rupture of the fleshy column of the mitral valve, two were thirty and forty-four years of age, and the other two were also in middle life. In the case of rupture of the tricuspid valve, the patient was aged twenty-two, and this was the only instance in which the accident is reported to have happened to a female.

The duration of life, after the occurrence of the injury, varied in different cases according to the valve affected, and the nature of the accident. Thus in the cases of rupture of the aortic valves, life was prolonged for twenty-one days, thirteen months, eighteen months, two years, and twenty-seven months, and in one case the patient was still surviving after an interval of five months. In the case reported in the paper, where the patient lived twenty-seven months, it will be observed that death was occasioned by meningeal apoplexy, and not by the direct effect of the valvular injury. In the cases of injury of the mitral valve, the patients survived nine days and twenty months in two; in a third, the precise interval which elapsed be-



tween the occurrence of the accident and the fatal termination is not stated; and the fourth was still living when the report was published, after three years had elapsed. The case of rupture of the tricuspid valve terminated in death in a few hours; but in this instance, as before mentioned, the patient was in the last stage of phthisis when she sustained the injury. It will thus be seen, that the immediate danger from injury of the valvular apparatus of the heart is very considerable, and death may ensue within a few days. Should, however, the patient rally from the immediate shock of the accident, and the subsequent inflammatory symptoms be overcome, life may be prolonged for a considerable period, varying according to the nature of the injury, and the circumstances of the individual. Generally, injuries of the aortic valves, like affections of those valves dependent on ordinary causes, are more rapidly fatal than those of the mitral; and when the injury is such as to incapacitate them from closing the aperture, its effects are very much more serious than when obstruction to the flow of blood from the ventricle into the aorta only is occasioned. Indeed, while the slighter forms of injury may, under favourable circumstances, be followed by only temporary disturbance of the action of the heart, and occasion very little permanent impairment of its function, injuries by which the valves are rendered incompetent are compatible only with the maintenance of life for a very limited period, though, as shown by my own case, a patient, under such circumstances, may still be capable of a considerable amount of exertion. In the case of Childs, however, owing to the valves having probably never been entirely separated from their connection with the coats of the aorta, the regurgitation was less free than in some other cases. It is also probable that the fibrous zone of the aorta had contracted after the accident, so as to lessen the effect of the valvular imperfection. The capacity of the aortic orifice in the adult is ordinarily from thirty-six to thirty-nine French lines, and that of the pulmonic orifice forty-two lines. In this case, however, the aortic orifice was only thirty lines in circumference, and the pulmonic forty-two. So that the capacity of the former may be regarded as having been from six to nine lines less than natural, and the diminution had probably taken place after the occurrence of the accident, and was due to the tonic contraction of the fibrous zone, consequent on the absence of the arterial tension which attends the ventricular diastole when the valves are in a sound state. In these cases of injury of the aortic valves giving rise to incompetency, hypertrophy of the left ventricle takes place very rapidly, and furnishes the means by which alone the circulation can be maintained. Of the extent to which that hypertrophy may be carried in a comparatively short time, the first case recorded by Dr Quain, and that related in this paper, afford good examples. In the former the heart acquired the weight of  $22\frac{1}{2}$  oz. in two years, in the latter of  $17\frac{3}{4}$  oz. in twenty-seven months.

When, during violent muscular exertions, the valvular apparatus



of the heart sustains injury, the patient usually experiences a sudden accession of pain in the præcordia, which is not unfrequently preceded by a feeling of something having given way in the heart; the pain extends from the sternum to the spine of the back, and is often agonising and attended with syncope, and a sense of impending dissolution, dyspnoea, oppression in the chest, and palpitation. To these symptoms are superadded the physical signs of obstruction, or obstruction and regurgitation at the aortic orifice, or of regurgitation through the mitral or tricuspid aperture. It seems also that when the aortic valves are injured, syncope is one of the most prominent symptoms; while, in cases of suddenly induced incompetency of the mitral valves, the patients suffer most from the sense of suffocation and oppression in the chest. In some of the latter cases, spitting of blood has occurred shortly after the injury. In one of Dr Quain's cases of laceration of the aortic valves, the patient perceived immediately after the accident a peculiar sound, extending up the chest and neck, and in the ears, and he still complained of hearing the same sound several days after. In another case, a sound was heard by the patient when he lay down. The symptoms enumerated, which indicate the receipt of an injury of the valves, may subside in a short time, and be followed by those of inflammation; and these again may be subdued, and give place to the usual symptoms of obstruction or incompetency at the affected orifices, which are too familiar to require mention.

In regard to the treatment to be pursued in cases of injury of the valves, I have little to remark. When such an accident has occurred, and the immediate effects have subsided, the most important circumstance is to secure entire rest to the patient, to avoid all causes likely to give rise to inflammation, and to subdue such symptoms as may arise. The relief of inflammation must be accomplished by antiphlogistic treatment, adapted to the nature of the injury and the strength of the patient. When the aortic valves are injured, hypertrophy of the left ventricle is a necessary result. Great caution should therefore be exercised, lest, by unduly depressing the powers of the system, the natural remedial agency should be interfered with; and in cases in which incompetency of the aortic valves is occasioned, where the danger to be apprehended is from failure of the circulation, the greatest care should be exercised in the employment of depressing agents, and remedies which, like digitalis, act specifically in lowering the action of the heart, should be wholly avoided. Generally, slight local depletion, combined with mild alterative and diuretic medicines, are all which can in such cases be borne, and these remedies require to be followed by tonics, combined with a nutritious but not stimulating diet. In injuries of the mitral valve, the danger to be guarded against is chiefly the congestion of the lungs and other viscera, and the secondary diseases so induced. In these cases depletion may, therefore, be more freely had recourse to, active purgative medicines may be given, and diuretics, and especially digi-

talis, will generally be found most useful. The treatment to be pursued in the several forms of injury of the valves of the heart is indeed precisely that which would be directed in cases of similar valvular imperfection, resulting from any other cause, and need not be more fully dwelt upon. We must, however, bear in mind, that in all such cases entire rest is to be observed in the earlier stages, and that when the immediate symptoms have subsided, the avoidance of active exertion, and of all sources of undue excitement, is essential to the enjoyment of even a moderate amount of health.

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ARTICLE II. *Observations on Stricture of the Urethra, and its Treatment by External Incision.* By ALEXANDER FIDDES, L.R.C.S. Edinburgh, Surgeon, Kingston, Jamaica.

IN the catalogue of diseases which fall to the care of the practical surgeon, there are probably none requiring greater nicety in their management than stricture of the urethra; for although a large majority of cases yield readily to dilatation, when the means of effecting it are judiciously applied, yet it must be admitted that the treatment is not unfrequently difficult and embarrassing to the surgeon, and puts to trial his temper as well as his skill.

The remedial power of the bougie in this disease has been long ascertained and universally known, but different practitioners prefer different forms of the instrument. The metallic sound with a good handle, or the common silver catheter, are certainly the best in the hands of a surgeon conversant with the anatomy of the parts; but some give a preference to the flexible gum bougie,—of which those manufactured in Paris with a tapering extremity are perhaps the best of their kind. They may answer the purpose where the contraction is slight and recent, and as they yield and bend when their introduction encounters resistance, are less likely than the metallic instruments to do mischief when used by a surgeon not well acquainted with the course of the canal and its relations; but in narrow tight strictures they cannot be employed with the same certainty and effect. Other methods have been recommended, and are unfortunately still practised, for the treatment of this common and distressing complaint; the escharotic powers of nitrate of silver and of caustic potash are still advocated and employed by some. Internal incision would appear to be used occasionally, as a few cases in which it had been practised have come within my own observation. One of the latest proposals which is now engaging the attention of the profession in London is that of stretching and overcoming the stricture by the agency of an instrument styled a dilator, composed of a series of graduated tubes which are slid along a director.

All these procedures are at best very sorry substitutes for anato-

mical and manual skill, and will seldom, if ever, be employed by any surgeon who possesses a correct view of the pathology of the disease, knows the *modus operandi* of the bougie, and has a fair amount of tact and dexterity in the use of it.

But although the metallic bougie is the means to be preferred and tried in all strictures, its use is not attended always with success, even when directed by the ablest judgment and guided by the most dexterous hands, for some cases resist its action and remain proof against it.

This is the more likely to happen in cases where the stricture is of old standing, and shows much thickening and induration of the tissues concerned. Although the stricture may in this condition allow the bougie to pass through it, yet it may not permit the accomplishment of such a degree of dilatation as to afford permanent relief.

This opposition and resistance to the dilating process may be dependent on the inherent hardness of the contraction, placing it beyond the influence of interstitial absorption ; or it may result from repeated interruption in the employment of the bougie, through the supervention of constitutional irritation. This happening, which it occasionally does, even under the most careful and gentle use of the instrument, the stricture is almost sure to relapse to its original state of contraction, thereby rendering fruitless the time and trouble previously expended on the case. Surgical writers have noticed these difficulties, and there are few practical surgeons who have not experienced such ; yet no effectual plan to overcome them seems to have been known until Professor Syme published his method of doing so by external incision on a grooved director passed through the stricture. This improvement is certainly one of the most important that have been made in modern surgical practice, and will constitute an era of advancement in this department, like the epoch of surgical reform which the genius of Pott established in the age gone by, when the treatment of several diseases was simplified and improved. This addition to the means of curing stricture, will be most appreciated by those who have had most experience in its management ; they who have witnessed the pain and misery which the disease is apt to entail upon the sufferer, will regard the method by which it may be effectually cured as a boon to humanity ; and they who have practised this operation, and acquired their own evidence of the safety and certainty of the cure obtainable by its means, will be apt to think it strange that the operation has not been brought hitherto into more general adoption. Its tardy and unwilling reception, may be dependent, partly, on the slowness with which new ideas are generally disseminated, partly from anatomical and surgical skill being absolutely requisite for the execution of the operation and its after-treatment, and partly from a certain portion of the profession having questioned the principle of the operation, and opposed its performance.

And here I might be permitted to remark, that the arguments advanced in opposition to the procedure appear altogether jejune and frivolous, if we may judge by the controversial communications on the subject published in the medical press, during the last two years, and by the discussions at medical societies. From these publications little can be elicited, but they reveal an imperfect state of pathological and surgical knowledge which could scarcely have been expected among British metropolitan surgeons. It may indeed be inferred that, as anatomical and surgical knowledge advance, this method of treating obstinate stricture will come into more general repute, and as the measure is safe and its operation rapid, it may be sometimes proper and expedient to employ it, not only in cases that resist dilatation, but also in others that might give way to the continued use of the bougie, but in which circumstances may not allow time and opportunity for such a protracted proceeding. The annexed cases were treated by external incision, from proving rebellious to ordinary treatment. They have been selected from others, and are thought worthy of relation, not from any novelty in my treatment of them, but on account of their severity, and the great extent of urethra which had to be incised in order to complete the operation for their cure. In both cases the whole perineal portion of the urethra was extremely contracted, and the texture surrounding it so thickened and indurated, that it felt like a piece of rope when examined externally. From two to three inches of urethra were incised in the operation. Such extreme cases do not occur frequently, but their successful termination, without an untoward symptom, shows the soundness of the principle upon which their treatment was conducted. Tight and narrow strictures, involving an extensive portion of the canal, appear to occur very rarely in temperate climates, at least European surgical writers have estimated their greatest length at one inch; and Mr Syme, in reference to this point, says, "My commentators, who speak of long incisions in the perineum, and laying open the urethra to the extent of several inches, have no warrant in anything I have said or written on the subject. I never open the urethra beyond the extent of an inch, and seldom beyond that of half or two-thirds of an inch."<sup>1</sup> The experience of this eminent surgeon, who has operated upon more than fifty patients, drawn from the most severe and intractable cases in Britain, must be considered as decisive of the extent or length of the strictures which there occur. In this country, however, I have met, not unfrequently, with narrow and tight strictures much more extensive; and several medical men with whom I have spoken on this subject, have confirmed the observation. This peculiarity may be, perhaps, owing partly to the indifference and neglect with which this half-civilised population are wont to treat all diseases, but another explanation of it may be found in the fact that

<sup>1</sup> Monthly Journal, June 1851.

purulent discharges from the genitals are particularly obstinate and intractable in tropical climates, and the common cause of stricture being gonorrheal inflammation, it may be readily understood how this morbid action may, by long continuance, spread backwards along the canal, and produce organic change in a great portion of its tract. The cases described below show likewise that the perineal portion of urethra may be freely incised, when stricture or other causes render it necessary to do so, and that no dread need be entertained as to the healing of a clean incised wound in this situation, as the whole region possesses a sufficient depth of soft parts to constitute a bed for the support and organisation of the reparative material which is thrown out by the cut surfaces of the wound. The portion of urethra anterior to the scrotum, might not permit the same freedom to be used, as the tube there has but a thin covering, which might not be adequate to repair so extensive a solution of continuity in its walls. In this situation, however, I have never encountered any other than the short form of stricture, which has yielded always to dilatation.

CASE I.—Robert Luxton, aged 42, from Central America, applied to me in the beginning of December last, for stricture. He had been under my care six years before, for the same complaint, complicated with fistulous openings in the perineum, when he was so far relieved that the fistula healed, and a No. 8 bougie could be passed; but having been obliged to return home at this stage of the treatment, he was furnished with a set of bougies, and directed to pass them himself. Notwithstanding his doing so, the contraction returned, and at length became so bad as to make him constantly miserable, and even threatened his life by retention of urine.

This compelled him to come a second time to Jamaica in search of relief, and on his arrival he presented a very sickly and miserable appearance. On examining the urethra, I found a well-marked thickening and hardening of it, from the centre of the scrotum quite to the bulb. With some difficulty a No. 1 catheter was passed into the bladder through this long contraction, every part of which grasped the instrument tightly as it was introduced and withdrawn. After passing the same-sized bougie two or three times, at intervals of a few days, a No. 2 was got through, but I was then obliged to suspend operations in consequence of smart febrile disturbance laying hold of the patient. This caused a fortnight's delay; and when the bougie was resumed, No. 2 was got in. This size was passed several times subsequently without my being able to advance to the size above it, or to see much improvement in the urinary stream. I, therefore, had the instrument retained twenty-four hours in the bladder, and succeeded in getting in No. 3 immediately after its withdrawal. At this period of the treatment I was again obliged to desist, in consequence of fever which lasted several days; and when it had passed away, I found the contraction as tight as ever it had



been, and found as much difficulty in passing the smallest bougie as I had done in the beginning. Having now but little hope of relieving him in this way, I recommended incision, to which he readily consented; and I proceeded, on the 15th February, to execute the operation, not without some misgivings, however, on account of the large portion of urethra which it was necessary to divide. Following the method of operating laid down by Mr Syme, I placed the patient in the lithotomy position, administered chloroform, and passed a small grooved director through the stricture. Then, drawing up the scrotum, an incision was made from its centre to the margin of the anus, in the mesial line. The cellular and muscular textures were then divided and separated, until the posterior end of the stricture was felt exposed; the point of the knife was next guided by the end of the forefinger into the groove of the staff, and carried forwards so as to cut the whole indurated portion. The director was then withdrawn, and a No. 7 silver catheter passed into the bladder, where it was retained till the third day. There being some oozing of blood from the wound, a pledget of lint was introduced, and allowed to remain until next day. Notwithstanding the size of the wound, the urine ceased to escape by it, so early as the sixth day after the operation, and passed freely along the natural channel. He never suffered in any way from the operation; but, on the contrary, commenced very soon after it, to eat and sleep better, and to improve in appearance. In three weeks the wound had firmly cicatrised,—the urine passed in a strong stream,—the largest sized bougie could be introduced with facility; and the patient thereupon returned to his native country, satisfied and delighted with the result of his case, and bearing all the appearances of a renovated man.

CASE II.—Joseph Desporte, aged 52, had been suffering from stricture more than twenty years, and had undergone a variety of treatment. When he placed himself under my care, about three years ago, I found a long and tight stricture in the perineum, through which a No. 1 catheter was carried; but, as unusual difficulty was experienced in advancing the dilatation, I made him keep his bed, and retain the catheter in the bladder. In this way I increased the size of the instrument up to No. 6, but not before the end of two months. As the patient then felt himself considerably relieved by the ordeal to which he had been subjected, he became anxious to return to the duties of his plantation in the interior of the country, and did so chiefly from necessity, and contrary to my advice.

For about two years he went on tolerably well, without any further assistance, when, having been exposed to wet, he was attacked with irritation of the bladder and kidneys, and retention of urine.

This caused him to return to Kingston, and I saw him two weeks



afterwards, with another practitioner, who had been attending him since his arrival in the city, and who had been called to him in the emergency of the attack of retention of urine. On making my visit, I found that a series of very small gum bougies had been passed through the stricture from time to time, apparently with the effect of assisting, in a slight degree, the discharge of the urine, which had been voided for some time before with the greatest difficulty; but this procedure had not relieved the local and general irritation under which he was suffering. He now complained of frequent calls to make water, with pain in doing so, and of pains in the loins, and spasms in the lower extremities, so severe and agonising, that his cries at times alarmed the neighbourhood. The urine deposited much muco-purulent sediment,—his sleep was bad, and the general appearance indicated constitutional disturbance of a most severe kind. As the urine was passed with sufficient freedom to relieve the bladder, I advised that the bougie should be discontinued, and by cupping the loins, applying hot fomentations, and administering sedatives, this train of distressing symptoms passed slowly away; but it was nearly three months before he was able to leave his room, and then he was so emaciated and shattered, that it was deemed advisable to send him to the mountains with the view of recruiting his health.

After a month's residence in the country, he returned to Kingston in the middle of February last, considerably improved in health, but without change in the capacity of the stricture. I therefore recommenced the use of the bougie, and, assisted by chloroform, passed a very small catheter through the stricture, which occupied the whole portion of the urethra between the middle of the scrotum and the bulb. Having advanced, not without difficulty, to No. 3, I found that there was a strong tendency in the stricture to resist further dilatation; I therefore recommended that it should be cut, and the patient having willingly consented, I did so on the 8th March. The whole thickened portion of urethra was cut, which formed a wound from two to three inches long, a catheter was introduced, and a piece of lint placed in the wound. The urine escaped principally by the wound until the fifteenth day, when it began to resume its proper course; but it was a month before the wound healed completely, and the urine passed entirely along its natural channel. By that time the wound had firmly cicatrised,—the urine passed in a powerful stream,—the largest instrument could be introduced with facility, and the marked improvement in the appearance, showed that the system had happily been relieved of a distressing and withering disease.

KINGSTON, JAMAICA, 29th April 1852.

ARTICLE III.—*On the Occasional Epidemic Origin of Furunculous Eruptions.* By HAMILTON KINGLAKE, M.D., Physician to the Somerset and Taunton Hospital.

AN eruption of boils on any part of the body is considered, in ordinary cases, to be simply the result of some temporary derangement of the organic functions; and therefore it is that, as a rule, they observe only a limited or individual existence, whilst their proneness to receive a ready cure through the treatment directed to the primary disorder with which they may happen to be associated, causes them to be regarded as an ailment of comparatively slight import.

If, then, it should be observed of the inhabitants of a large district of country, that they became generally affected at one particular time with a furuncular eruption, in one or other of its forms, without its being associated with any of those well-marked derangements of the assimilative or excretory organs to which we might be inclined in the first instance to refer its existence; but instead thereof, it were found to be attended with that general depression of the vital powers, which implies a specific contamination of the blood rather than the lesion of an organ; and, further, if we should observe in the disease a tendency to run a tedious and intractable course, little influenced by remedies, and oftentimes to recur again and again without any obvious cause afforded, either by the circumstances of the locality or the habits of life of its people,—we should be led to the conclusion, that it must have an origin other than that ascribed to it, when occurring in its sporadic or individual form, and should look for it, accordingly, among the hidden causes of epidemic distempers. Now, it is in the belief that a short description of the boil-disease, as it has existed in this neighbourhood for the last five or six months, may be found to fulfil the conditions above expressed for inferring the epidemic origin of a disease, and therefore of illustrating the true pathology of the one in question, that I am induced to put on record such facts as appear sufficient to establish the point, trusting that they may also serve to indicate the true direction in which its *materies morbi* is to be looked for.<sup>1</sup>

It should be premised, therefore, as a matter bearing, as I am disposed to think it does materially bear, upon the etiology of the disease, that, in the course of last autumn, scarlatina of a very malignant type prevailed more or less in the localities that have since been the seats of the disease about to be described, and so quickly fatal was it in its ravages, more particularly among children, that in some cases within my own observation, it destroyed life in a few

<sup>1</sup> For the purpose of ascertaining how far the boil-disease referred to in the text extended beyond the sphere of my own personal observation, I put myself in communication with various medical practitioners resident in different parts of the neighbourhood (represented by radii of twenty miles and upwards), who all spoke to the fact of its extraordinary prevalence in one or other of its forms, in each of their respective localities.

hours, through the sheer intensity of its inherent poison, before the local manifestations of the disease had time to fully develop themselves. It was upon the subsidence of this epidemic that furuncular eruptions were first observed to prevail in the neighbourhood, to an extent which gradually increased up to within the last few weeks, since which period they have been on the decline; and here it may be remarked that, although the physical characters of the prevailing ailment might fail, as they appear to have failed, to present any special marks by which it may be distinguished from like affections of an idiopathic kind, this fact cannot be adduced at the onset as a reason for refusing to admit its epidemic character, seeing that the local affections, proper to or accompanying undoubted epidemic distempers, are often undistinguishable (the catarrh of influenza, to wit) from those which result from causes of a totally different kind, into which no poison element enters.

The most general form which the furuncular disease was observed to assume, was that of *whitlow*, which in some cases affected two or more fingers at one time; and so unusually prevalent was it in some particular districts, as to gain for itself the appellation of the "finger plague."

One gentleman, in writing to me on the subject, states that "within the last few months ten or twelve patients of a morning, chiefly of the lower class, have applied to me for relief for this particular affection" (whitlow), whilst it is within my own knowledge that a similar form of the disease prevailed at the same time among those whose circumstances in life appeared in all respects favourable to health and vigour. Indeed the disease, in one or other of its forms, was observed to be too general to admit of any marks of limitation, afforded either by age, sex, temperament, or condition of life; thus closely imitating in its ways those of influenza, to which other points of resemblance may possibly be traced.

The next more frequent form presented by the disease was that of *boils*, properly so called, and *carbuncles*, into which the former more usually degenerated.

The following extract of a letter from a medical practitioner resident in a more distant part of this neighbourhood, expresses so nearly the substance, not only of my own, but also of the united observations of the various medical men with whom I have been in communication with reference to this subject, that I am induced to quote it:—

"The affection which has existed in this neighbourhood, more particularly during the last six months, still goes on; for hardly a day passes without some new instance presenting itself. Boils of all sizes, from the smallest pimple to the size of a teacup, are met with in all parts of the body." "Both sexes appear almost equally subject, perhaps males rather more so; no age is exempt; and there is frequently great debility complained of. Half-a-dozen different remedies were tried, without apparently the slightest benefit. Many of the boils have run into carbuncle, or carbuncle-

looking boils, requiring the crucial incision, and in many cases there has been considerable sloughing of the cellular membrane, with an indisposition to get well in weak constitutions."

Deep phlegmonous inflammations, abscesses, and collections of purulent matter in different parts of the body, appear to have been next in order of frequency; but it is not necessary, perhaps, to advert to these other varieties of the furunculous disease, farther than to state that the fact of their prevalence rests upon precisely the same evidence as that which has been adduced for establishing the wider extension of the boil form of the ailment.

With regard to the course and duration of the malady, the constitutional symptoms attending it, and its amenability to medical treatment, it is to be remarked, as the result of various concurrent though independent observations, that there existed in all its phases a manifest depression of the vital powers, requiring a strictly tonic and supporting treatment, whilst its marked tendency to run a certain definite course, as shown by its successive re-eruption in various parts of the body until it is to be presumed its *materies morbi* was finally eliminated from the system, appears to have rendered all attempts to abridge its duration by specific treatment more or less futile.

From the above sketch of the disease, one can hardly fail to perceive (looking more especially to the circumstances preceding its outbreak, its general prevalence throughout an extensive district of country, its intractable and lengthened course, and lastly, the lowered state of the vital powers co-existent therewith) that it has conformed more to the ways of epidemic visitations than to the laws of any other form of diseased action.

Presuming, then, that no further evidence is necessary for establishing its epidemic character, the question arises, Whether such an epidemic is to be accounted as a modified form or offshoot from some graver pestilence, that had left the debris (so to speak) of its virus upon the neighbourhood, or whether it be of a nature that can satisfactorily claim for itself a separate and independent origin? The former view, although not susceptible of actual proof, is more in accordance with the acknowledged general laws of pestilence<sup>1</sup> than the latter alternative, whilst it harmonises with the fact before adverted to, that the localities in which the furunculous eruptions prevailed, were also those in which a malignant form of scarlatina existed immediately previous to its first outbreak, and seeing that certain animal poisons do oftentimes create in the individual, as one of the secondary or ulterior effects of their agency, a disposition

<sup>1</sup> It is a well-recognised principle with respect to all kinds of epidemic pestilence, that they present very different characters, and exercise very different degrees of noxiousness, at their rise and at their decline. Thus, plague of the most malignant type often fails, at its outbreak, to exhibit any of its characteristic local eruptions, whilst the *mildest* forms of the same disease, as observed towards the decline of the epidemic, are marked by the presence of buboes and carbuncles, but generally unattended with any great amount of febrile or constitutional disturbance.

to the formation of boils and carbuncles, as observed during or subsequent to the course of many febrile diseases, more particularly of the exanthemata, it is not perhaps unreasonable to suppose (presuming that such furunculous eruptions correctly represent the localization of the dregs or diluted form of the original poison) that one or other of such animal viri, acting epidemically, *may*, after expending its strength upon the more ready recipients of its virulence, and suffering thereby certain changes in the nature of its morbid affinities, fasten afresh upon other subjects, and upon other tissues of a less vital character, thus giving rise to an epidemic visitation of the nature above described.<sup>1</sup>

It does not fall within my purpose to carry this speculation to the extent to which it is obviously susceptible of being carried ; my main object in recording the prevalence of boils and other similar eruptions in this neighbourhood, being, *Firstly*, To establish their epidemic character ; and, *Secondly*, To suggest the probability of their connection with the scarlatina that preceded them, being of a nature other than that of accidental sequence. Although it be premature in the present state of our knowledge to assign a definite term to the relation thus thought to subsist between two diseases whose points of outward resemblance are so widely different, it is certain that no wrong is done to the cause of scientific truth by bringing together into one view as many of the various forms of morbid action (however unimportant any one of them may at first sight appear) as have been previously found to subserve the same general laws ; for in proportion to the number of results so obtained, will the chances increase of our recognising the connecting links which bind one form of epidemic pestilence to another, thereby preparing the way for arriving ultimately at that point in generalization in the department of zymotic diseases, which has been attained in physical science for the so-called imponderable agents.

TAUNTON, May 1852.

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ARTICLE IV.—*Case of Open Foramen Ovale, occurring in a Child aged Fifteen Months ; with Physiological Observations.* By JOHN STRUTHERS, Fellow of the Royal College of Surgeons, and Lecturer on Anatomy, Edinburgh.

(*Read before the Edinburgh Physiological Society, 13th March 1852.*)

THIS heart presents,—1st, An open foramen ovale ; 2d, A large Eustachian valve ; 3d, Contraction and hypertrophy of the right ventricle, with contraction of both its orifices.

<sup>1</sup> The difference in the physiognomy of the same disease, according to the varying intensity of the virus causing it, and the particular tissue in which it is constrained to locate itself, is illustrated by the two distinct phases of plague, viz., the malignant or non-eruptive, and the mild or carbuncular,—forms of disease which may be held to correspond with the scarlatina and ailment just referred to in the text.



The *foramen ovale* admits the little finger,—i. e., is equal in size to a circular opening of the diameter of half an inch. Its valve is imperfectly developed, thick, and muscular-looking. It does not reach to the upper margin of the foramen, although its horns on each side reach higher than the opening.

The *Eustachian valve* is very large and loose, much more so than at any period of foetal life. It measures nearly an inch (7-8ths) from its attached to its free border; and when lifted upwards, it reaches for one-third of its depth above the upper margin of the foramen ovale. It is cribriform, especially at the right half of its attachment, and also above, near its free margin.

*Right ventricle*.—The walls are 3-16ths of an inch in thickness, whilst those of the left are from 3-16ths to 4-16ths. In addition to hypertrophy of the walls, there is likewise contraction of the cavity, which is apparently about one-half the size of the left.

*Tricuspid orifice* admits the little finger (4-8ths inch), whilst the *mitral orifice* admits the forefinger (5-8ths). *Vena cava superior* admits little finger with difficulty. *Vena cava inferior* admits forefinger easily. *Foramen ovale* admits little finger moderately, as already noticed. *Tricuspid valve* much thickened. *Cordæ tendineæ* shorter and thicker than on left side, and *musculi papillares* large. *Mitral valve* appears healthy, except that there are a few vegetations on both its flaps, where the cords chiefly join them. *Aortic valves and orifice* natural. The orifice measures 3-8ths of an inch in diameter.

*Pulmonary orifice and valves* much contracted and altered. The semilunar valves are united together into one solid mass, with a small aperture in the centre. Seen from above, there is the appearance of a nipple, smooth and rounded, and perforated in the centre by an aperture not larger than a crow-quill, the whole somewhat resembling a cervix and os uteri. Surrounding the base of this nipple-like projection, and separated by partitions which join the central papilla, are *four* semilunar recesses, corresponding to the sinuses of Valsalva, and to the cavities of formerly existing semilunar valves. One of these is nearly as large as the other three together; it is left and posterior; one of the smaller is anterior; the other two are on the right side. Seen from below, the orifice appears blocked up by four irregular tubercular projections, two anterior, and two posterior; one of the posterior is large, the others are of equal size. They are firm and irregular, but covered by a serous membrane; and in between them is the small aperture which leads up through the central papilla. On pushing the probe down into the sinuses of Valsalva, it pushes out the membrane at a considerable distance below the tubercles.

The *ductus arteriosus* admits a small wire with difficulty, and was therefore practically closed. Both *lungs* are studded with tubercles of the size of small peas or grains of corn, on their surfaces as well as throughout their substance.

*History during life.*—A month before death, there was difficulty of breathing, the lungs appeared healthy, and the case much resembled one of disease of the mitral valve. Ten days before death, occasional blueness of the surface made its appearance, lasting only a short time, without any paroxysm of difficult breathing, and then passing off. Death occurred suddenly. The case belonged to my friend Dr Campbell, for whom I conducted the post-mortem examination, and to whose kindness I am indebted for this use of the preparation.

*Remarks.*—A more or less open foramen ovale is well known to be the most common imperfection of the heart, but the causes of this condition are perhaps not generally understood or agreed upon. We may refer it to two causes—1. An impediment to the free passage of the blood through the right ventricle, from contraction of one of its orifices; and 2. To an imperfect development of the valve by which the foramen is usually completely closed at, or soon after, birth.

1. The connection between contracted pulmonary orifice and open foramen ovale has been remarked by various observers. M. Louis regards it as the most common cause of the open foramen, and farther looks upon this pulmonary contraction as almost always a congenital condition. Out of 53 cases of imperfect heart quoted by Dr Joy in the "Library of Medicine," the foramen was open in 33, the pulmonary artery contracted in 22; and it is not unlikely that in many cases the open state of the foramen ovale has been recorded, whilst the condition of the pulmonary artery and its orifice had not been carefully examined. The case I have related illustrates this connection between open foramen ovale and contracted orifices of the right ventricle. Still the question occurs, Why should there so frequently be valvular disease and contracted orifice on the *right* side in the foetus and child, and not in the left, as in the adult. I do not think that this can be explained by the supposition that the right ventricle has, relatively to the left, more labour to perform in the foetus than in the adult. In the foetus, it is usually remarked as interesting to observe that the walls of the right ventricle are as strong as those of the left, or nearly so; but this I think has been overstated by some. In the foetus between the fourth and fifth month, now before the Society, it will be observed that the left is twice the thickness of the right; and in the other foetus I now show—at the third month—the difference is already well marked. We can see no reason then, why, during the latter two-thirds of foetal life, and in early childhood, the *right* ventricle should be the seat of hypertrophy and valvular contraction, and *not the left*. The fact of the foramen ovale being open, seems to be sufficient proof of the correctness of the view of M. Louis, that the pulmonary contraction is congenital, otherwise in the meanwhile the foramen would have become closed. Still it is not apparent why in the foetus the pulmonary orifice should be the seat of malformation or disease more than

the corresponding aperture of the left and stronger heart. The view may occur, that this contracted state of the orifices of the right ventricle and pulmonary artery, is not the cause but rather the result of the condition of open foramen ovale. This view might accord with the state of *simple contraction* of the pulmonary artery or its orifice, as part of the blood which should pass through them has found another channel; but it certainly will not account for *diseased* or *malformed* conditions of the right ventricle and its orifices.

I may observe that in the case I have related, it was the orifice merely which was contracted, not the artery, as, a short distance above their orifices, the measurements of the pulmonary artery and aorta were the same. Some cases have been recorded of contracted or obliterated pulmonary artery, in which the ductus arteriosus remained open so as to allow of a recurrent circulation to the lungs, but in this case the ductus arteriosus was practically closed. From this it may be inferred that the severe contraction of the pulmonary orifice had not been of very long standing, possibly not before the symptoms became aggravated, and occasional cyanosis made its appearance, about ten days before death.

2. Passing over the possibly correct view of some that the foramen ovale is occasionally again opened, or burst open, by severe falls, prolonged fits of coughing, or severe straining, — the next cause of open foramen ovale is the imperfect development of its valve.

In connection with this, it is necessary to understand clearly the natural means by which the closure of this aperture is effected after birth. In the well-developed foetal heart, the inferior vena cava terminates so that it may be said to open into both auricles, and each opening is provided with a valve. The Eustachian valve partly guards or lies over and diminishes its opening into the right auricle; and its opening into the left auricle, which is the foramen ovale, is guarded behind by the valve of that foramen—the *valvula foraminis ovalis*. It is still a disputed question, whether the blood of the lower cava mixes with that of the upper. Now there is nothing to prevent the blood of the lower cava from coming forwards into the general cavity of the right auricle, only it must turn forwards at right angles to its previous course, around the free margin of the Eustachian valve; and that part of it does so, appears evident from the consideration that the inferior cava is larger than the foramen ovale, whilst the tricuspid orifice is as large as those of both cavæ put together. This is seen in the foetus, between the fourth and fifth month, now on the table; and the foramen ovale becomes smaller as foetal life advances, for, although it attains, as usually described, its maximum size about the sixth month, still, relatively speaking, the communication between the two auricles gradually decreases from the first appearance of the auricular septum about the ninth week. Whilst, then, that blood which does go by the foramen ovale is still entirely



of the purer current, from the lower cava, it follows, as the foramen is of less size than the vein, that some of the blood of the latter, that which the foramen ovale cannot take in, will enter the right auricle and mix with the other current. This, therefore, appears to be one of four points where there must occur some mixture of the pure and the less pure currents of the foetal blood—the second is in the left auricle, where the blood which has passed through the foramen ovale is mixed with that blood which comes, whatever its quantity may be, by the well-developed pulmonary veins; the third, where the arch of the aorta and ductus arteriosus join, where it does not appear how some of the blood of the former can avoid passing down to the thoracic aorta; still, however, this is not till the vessels of the head have been filled by the purer current; and the fourth, or rather the first and chief point of mixture, is where the blood of the hepatic veins and ductus venosus is mixed with that of the vena cava inferior.

A great part at least, therefore, of the blood of the lower cava passes through the foramen ovale. But to prevent this after birth its valve is provided. This valve is rather a provision for closing the aperture at birth, than for any part it has to perform before this time. Although, many years ago, it was the subject of much attention in France, it has perhaps been a little overlooked by some, partly, perhaps, for want of a distinctive name. It is the “*valvula foraminis ovalis*,” and I may venture to suggest for it the term *obturator valve*, as its office is to shut up the foramen at and after birth. According to most authorities, this valve begins to be developed towards the end of the third month. It is not until the end of the second month that the septa of the ventricles and bulbus arteriosus are completed, and then an imperfect auricular septum is formed, leaving the foramen ovale at its lower part. The true or defined foramen ovale is itself, therefore, not formed or marked out until, at least, after the end of the second month. Some have found the valve, at the end of the second month (Senac and Portal, according to John Reid), and in the foetus now before the Society, which I have ascertained to be near the end of the third month, this valve is developed so far that it rises up above the middle of the foramen, and is as high as the free edge of the large Eustachian valve, which lies in front of it. At the end of the fifth month, according to Cruveilhier, this valve is large enough to cover over completely the orifice of the foramen ovale; and this will be seen to be the case in the foetus between the fourth and fifth month now before the Society. The valve in its lower 2-3ds grows from the sides of the foramen ovale, but, above, its horns pass upwards and outwards from the opening as far as 1-16th of an inch above it; and, when the valve is lifted or floated up, it is more than sufficient to cover over the foramen ovale.

We see then that, as early as the middle period of uterine life, this valve, or obturator membrane, is so fully formed as to be cap-

able of shutting up the foramen completely. At birth it is floated up by the reversed current, and applied against the foramen; and, becoming united or glued to it, the septum auricularum is completed.

We can understand, then, how an imperfect development of this valve will give rise to the condition of open foramen ovale, as there is no other means by which the communication between the auricles can be closed; and if in any case we find this valve so undeveloped that, on being raised, it cannot shut up the foramen ovale, we may justly consider its non-development as a sufficient reason for the foramen ovale being open.

This imperfect condition of the valve may be attributed either to its simple non-development, as the growth of this, as well as of various other parts, may be withheld without any apparent physical cause; or it may possibly be due to obstruction at the orifices of the right ventricle. Were the latter condition to exist, thus rendering impossible the closure of the foramen ovale at birth, although its valve had been well formed, analogy would lead us to expect that the valve should present a thin and reticular appearance, like an ordinary wasting Eustachian valve, rather than appear simply small and undeveloped; but were the pulmonary orifice to become malformed or contracted during the two first months of foetal life, before the time for the development of the valve had arrived, this might possibly influence the non-development of the valve, besides causing all the other appearances which the heart presented in the case I have related to-day. However, whilst contracted pulmonary orifice is undoubtedly a frequent concomitant and, it may be, precedent, of open foramen ovale, it is at the same time by no means invariably or necessarily so. In examining cases of open foramen ovale, I would suggest that care be taken to examine especially into these two points:—1. Whether there is contraction of the pulmonary or tricuspid orifices, as compared with the aortic or mitral; and 2. Whether, with or without this, there is deficiency of the obturator valve, which, lying on the left aspect of the foramen ovale, should be found in its fully developed state more than sufficient, when lifted up, to shut up the aperture.

These, then, it appears to me, may be laid down as the two causes of the condition of open foramen ovale. In the one case its obturator membrane may have been fully formed, but it is kept open by the blood which cannot find a free passage through the right side of the heart; in the other case, the membrane, which should be ready to close it up, is too small, and it remains open for want of any provision to close it; and again, these two conditions may be found to co-exist, as in the case I have related to-day.

Another question in connection with open foramen ovale after birth is—Whether any mixture of the blood occurs, and, if so, to what extent, and under what circumstances? It is well known that this foramen has often been found open to a considerable extent in those in whom such a condition was not suspected during life—I

mean a much larger opening than the small oblique slit which is very often found at the upper part of the fossa ovalis. Now, to understand this, let us first see how it is in the foetus. I believe it is the common idea that the foramen ovale is merely a hole from one auricle to the other, and that the right auricle drives the blood through it to the left. But it cannot be so. There is no reason to believe that the auricles do not fill and contract together as in the adult, and so also the ventricles. Now the right auricle would require to contract first, were the left filled by it through the foramen ovale, and thus at the same time the right ventricle would be filled before the left auricle had time to contract and distend the left ventricle. We must conclude that the auricles and ventricles act synchronously as in the adult.

The fact is, that the lower cava fills the left auricle just as the upper fills the right, both auricles being filled during their conjoint diastole and repose; and it may be that, after all, this is a purpose designed by the mode of entrance of the lower cava and by the foramen ovale as much as that the two currents should be kept separate, in order that the purer blood may go to the upper half of the body. Whilst either purpose separately would have required this arrangement, both are at the same time accomplished by it.

In the foetus, then, the auricles being filled at the same time in this way, they contract; but no mixture can occur at this time, as the valve of the oval hole will not allow any blood to pass back from left to right. Previous to the third month this does not hold, as the valve is not developed; but until then it may be said that the two auricles simply form *one*, the lower cava opening to the left side, the upper to the right. But what I desire to demonstrate is, that when the parts are well formed, as during the two latter thirds of foetal life, there is no exchange of blood between the two auricles from the mere existence of the foramen ovale; that the right auricle does not fill the left through the foramen ovale, nor does any regurgitate through it from left to right; but the left auricle is simply filled from the lower cava, as the right chiefly is from the superior cava.

Now, it appears to me that after birth it will be very much the same, when the foramen remains more or less open. Supposing that there is no contraction of the pulmonary orifice, which certainly would occasion the employment of the foramen ovale, still it appears to me that there cannot but be some amount of passage of venous blood into the left auricle, and this to a lesser or greater degree according as the valve is developed or not, as the lower cava still pours its stream in the direction of the aperture, through which it must partly pass and encounter and mix with the blood entering the auricle from the pulmonary veins. Still the admixture occurs during the diastole and repose of the auricles; and is not from the cavity of the right auricle, but from the inferior cava, although this can make no difference on the symptoms or to the patient.

Even in those common cases where there is a small oblique slit only, I do not see but that there must be some amount of admixture. This occurs, it is well known, as often as in one out of every five or six subjects. There is usually (from the manner in which the valve shuts up the opening at birth) at the upper part of the fossa ovalis a recess, which occasionally presents a small unclosed passage admitting a probe or even a quill. Of this I have brought a preparation, now before the Society, where a common quill readily passes through a very oblique passage, and along with it is seen a corded condition in front of the oval fossa, and an extremely reticular condition of an old Eustachian valve; and there is in my museum a preparation taken from a female aged sixty, in which the aperture is twice this size; also the Eustachian valve is large and strong. It is commonly remarked, that this condition can allow of no admixture on account of the obliquity of the passage. But whilst I am aware that obliquity of perforation is productive of a very perfect valvular effect, as in the case of the termination of the ureters and bile duct, still the circumstances are reversed here. The force which shuts the oblique passage—the passive flow of the pulmonary blood into the left auricle—is not stronger than, and is not so direct as, the force which tends to open it—viz., the current of the lower cava; and as the direction of this is exactly in the direction of the slit, it seems to me that there cannot but be, during each diastole, a small quantity of venous blood sent into the left auricle. This, however, can be only to a very trifling extent, and would not, perhaps, be worthy of notice, were it not for the sake of fully reasoning out the effect of the various conditions of open foramen ovale.



ARTICLE V.—*Case of Malformation of the Heart and Blood-vessels of the Fœtus: Pulmonary Artery giving off Descending Aorta and Left Subclavian.* By Mr. DAVID GREIG, Dundee.

DURING the summer of 1850, having procured a fœtus, which seemed to be about the ninth month, and which, on external examination, presented nothing peculiar with regard to form, colour, etc., I had it injected by the umbilical vein. On examining the chest, I found the following arrangement of its blood-vessels. The ascending *aorta* is small, passes straight up, and, after a course of about three-quarters of an inch, terminates in three branches of equal size, which are the right *subclavian*, and the right and left *common carotid* arteries. The *pulmonary artery* is very large, measuring about half an inch ( $\frac{5}{12}$ ) in diameter, being nearly twice that of the ascending *aorta*, the diameter of which is one quarter of an inch. After a course of half an inch, the *pulmonary artery* gives off the branches which go to the lungs, these arising from its back part and almost by a common trunk. A quarter of an inch further on, the *pulmonary artery* (or

*ductus arteriosus*) gives off from its anterior and left side the *left subclavian*. The pulmonary artery, hitherto undiminished in size, now contracts rapidly to half its former size, and is continued on as the descending *aorta*. There is thus no communication between the ascending and descending *aortæ*. The right and left subclavian arteries appear of equal diameter. The *venæ cavæ* and pulmonary veins are normal.

*Heart*.—On opening the ventricles, they are seen to communicate by a large aperture, arising from a deficiency of the septum ventriculorum at its upper part. The right ventricle is twice the size of the left, though its walls are at least not thicker than those of the latter. The pulmonary artery arises normally from the upper and anterior part of the right ventricle. The *aorta* arises exactly above the communication between the two ventricles, its mouth being equally visible from either cavity. The aperture of communication is of a rounded form, measuring one-third of an inch in diameter, bounded below and on each side by a smooth thick edge, and above by the opening of the *aorta*. The semilunar valves of the *aorta* and pulmonary artery are well developed and normal. The tricuspid orifice is three times the size of the mitral orifice. The right auricle appears to be dilated, whilst the left is about one-third the size of the right, and a little larger than its corresponding ventricle. The *foramen ovale* is normal, and its valve is fully formed; but the passage remaining between the upper end of the foramen and the free border of the valve, appears to be considerably smaller than that which is usually seen in the full grown foetus.

a, Ascending *aorta*, ending in right subclavian, and right and left common carotid arteries.  
b, Pulmonary artery. c, c, Branches to lungs. d, Left subclavian artery. e, Descending *aorta*.  
f, Vena cava superior.

*Remarks.*—As far as I am aware, no case has yet been recorded in which the above remarkable abnormalities were combined. In the third volume of the “Library of Medicine,” article “Malformations of the Heart,” reference is made to two cases somewhat analogous. In the one related by Sir A. Cooper, the pulmonary artery arose from both ventricles and furnished the descending aorta, the ascending aorta originating naturally. In the second, related by M. Breschet, the left subclavian arose directly from the pulmonary artery. In the above case, however, both of these conditions are combined.

Had the malformation been confined to the blood-vessels alone, interesting inferences might have been drawn, with regard to the foetal circulation. There was no communication between the ascending and descending aortæ; and this corresponds to the view commonly entertained, that the purer blood of the ascending aorta is distributed by the vessels which arise from the *arch*, whilst the descending aorta is filled with the less pure blood from the pulmonary artery. Again, it will be observed that the two superior extremities were supplied with blood from different sources, which, in the normal state of the heart, would have been of different qualities, whilst in no respect was there any difference in degree of development between the superior extremities. The additional complication, however, of such a large aperture between the ventricles, must have established complete admixture of the blood of the two sides of the heart, before it was sent into the aorta and pulmonary artery, so that, independent of subsequent abnormalities of the vessels, all parts of the body would be supplied with blood of the same quality.

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ARTICLE VI.—*Case of Wound of the Abdomen.* By ARCHIBALD BLACKLOCK, late Surgeon R.N., Dumfries.

WHILE some boys were bathing in the Nith, on Saturday, 10th April last, one of them, James Wilson, between 9 and 10 years of age, fell upon a broken wash-hand-basin, which inflicted a wound in the abdomen, extending from a little below the umbilicus, three inches down the linea alba, or, in other words, nearly to the pubis, through which the greater part of the small intestines, the transverse arch of the colon, and the omentum, immediately protruded. When I first saw him, he had been lying upon the bank of the river upwards of an hour, and the protruded parts, which were chiefly hanging over his left thigh, had evidently been in contact with the ground, for a considerable quantity of sand, withered grass, and other extraneous matters, adhered to them; and, from long exposure to the atmosphere, and fruitless attempts on his own part to force them back into the abdomen, they had become of a deep red colour. Twenty minutes more elapsed before a supply of warm water and a sponge could be procured from Nunbank House, the nearest residence; and the washing of the intestines and mesentery, which



was chiefly done by allowing the water to flow over them from the sponge, also occupied a considerable time before they could be replaced, as the poor boy complained, in the most pitiable and distressing manner, when the parts were much touched with it, and occasionally said, in a whisper, "Just kill me." The reduction even was by no means so simple and easy a process as some might suppose; for it frequently happened that patches of sand, etc., which had not previously been observed, were brought into view, as convolution after convolution of intestine was about to be replaced, and of course had to be washed off before the operation could be proceeded with. I may therefore safely say, that it was fully an hour and a half from the time that the intestines were immersed in the cold river until they were fairly returned into the abdominal cavity. The whole, however, being replaced, and the omentum spread out, so as to come as much as possible between the intestines and the external wound, the lips of the latter were immediately approximated, and secured in contact by three interrupted sutures, which were afterwards supported by strips of adhesive plaster. The unfortunate boy was now conveyed to his father's house in Dumfries, about a mile from the place where the accident occurred; and three hours after, when reaction had fairly commenced (for, in the first instance, or at least from the time of my arrival, he was pale and almost pulseless, although little or no hemorrhage had taken place), twelve leeches were applied, and a grain and a half of calomel, combined with one-twelfth of a grain of opium, directed to be given every two hours.

*April 11th, 9 A.M.*—Has passed a pretty quiet night. Leeches bled freely, encouraged by warm fomentation; pulse 124, and of moderate strength and fulness; abdomen is tense, tender, and tympanitic; complains of thirst, and occasionally vomits soon after drinking; has made water twice; twelve more leeches to be applied to the abdomen, and the calomel and opium continued. 1 P.M.—Pulse 140; complains of no uneasiness, unless the abdomen is touched; thirst urgent; still occasionally vomits after drinking; the bleeding from the leech bites has been very copious. 10 P.M.—Is much in the same state as at last visit; pulse 144.

*April 12th.*—Has had another quiet night, and feels quite easy when not moved or meddled with; pulse 122, and of moderate strength; skin comfortably warm and soft; tongue rather moist; thirst, however, continues, and he still vomits the milk and water, which he prefers for drink, but not so often as formerly; urine abundant, and of normal appearance; tension and tenderness of the abdomen have not increased; and the edges of the wound remain in contact. 6 P.M.—The tension and tenderness of the abdomen have evidently increased since morning; pulse 124; apply ten leeches, and continue the calomel and opium. 9 P.M.—Leech bites bleeding freely; abdominal tension and tenderness rather less; pulse 122.

*April 13th, 10 A.M.*—The swelling and tenderness of the abdomen considerably diminished; pulse 114, soft, and natural; thirst less;

urine copious; gums tender. Calomel and opium to be discontinued; has taken altogether forty-five grains of the former, and two and a half grains of the latter. 9 P.M.—Progresses favourably; pulse 104.

*April 14th, 11 A.M.*—Has had a good night, and says that he feels quite well, and is very hungry; pulse 104, and rather full; thirst moderate; urine still copious, but the bowels have not moved since the accident; had an evacuation, however, shortly before that event. To have a little arrow-root at any time he may wish to have it. 9 P.M.—Pulse 94; tongue moist and clean. A suppository of common brown soap to be placed in ano.

*April 15th, 10 A.M.*—Bowels have moved three times since yesterday evening; evacuations of a healthy character; no discharge from the wound; and the dressing has not been interfered with.

*April 17th, 10 A.M.*—Removed the plaster and sutures; wound seems well and permanently closed—nicely healed by the first intention, except at one point, where a little of the omentum, not larger than a garden pea, protrudes; touched this with nitras argenti; appetite good. To have beef-tea and bread.

*April 18th.*—Has had no evacuation from the bowels since the 15th. To have a gruel enema, and to be allowed porridge and milk morning and evening.

*April 19th.*—The bowels have moved twice since yesterday, when the enema was administered, both evacuations of natural appearance, and well formed. From this period he daily gained strength. On Monday, 5th May, returned to school; and up to this date continues perfectly well.

*Remarks.*—The above case of wound of the abdomen, with free protrusion and long exposure of the intestines—not to speak of their being previously immersed in cold water, and afterwards covered with sand and dirt—appears to me worthy of being recorded, as it furnishes a good instance of perfect recovery under very unfavourable circumstances; and shows what may sometimes be expected and prognosticated, even in the worst of cases, provided the coats of the intestines have not been penetrated, when the protruded parts are carefully replaced, the wound closed, and the patient frequently seen and prescribed for (but not too officiously) as symptoms seem to indicate. In this instance the patient had enjoyed excellent health up to the time of the accident, and possesses a fine quiet amiable temper. Indeed, a more tractable and heroic patient never came under my observation, either at home or abroad—in peace or war; and his composed state of mind no doubt greatly facilitated the healing and restorative process, and conduced to his rapid and complete recovery. The total prohibition of food, if we except the milk which was mixed with his drink, during the first four days, and strictly avoiding the employment of purgatives during the whole treatment, are also circumstances not to be overlooked.

DUMFRIES, June 8, 1852.



ARTICLE VII.—*Extracts from Clinical Lectures.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

23d May.—*Stricture of the Urethra.*

UPON the present occasion, I do not propose to consider the origin or symptoms of strictures, but wish that your attention should be directed to their treatment. Here, also, I am unwilling to occupy your time with a catalogue of all the means that have at different times been contrived for this purpose, since there seems little reason to doubt that, in ordinary practice, the simple metallic bougie accomplishes the object most easily and safely. But there are two forms of the disease in which mere dilatation has been found inadequate to afford relief; and it is these that I now wish more particularly to make the subject of observation. In one of them, the contracted canal is so extremely irritable that the introduction of an instrument aggravates instead of alleviating the symptoms, and exposes the patient to various dangers from the local and general disturbance thus excited. In the other the peculiarity consists in a contractile tendency so strong as quickly to counteract the effect of dilatation, and thus render it useless. These two conditions of stricture—the irritable and contractile—long constituted one of the greatest opprobria of surgery; and placed a practitioner in the painful position of either allowing the disease to proceed unchecked, or attempting to remedy it by means not only useless, but injurious. In order to overcome this difficulty, I tried a plan different from any previously employed, which consisted in the introduction of a grooved director through the stricture, and free division of the contracted part of the canal by external incision upon the guide thus afforded. I have now performed this operation in upwards of sixty cases—in every instance with the effect of accomplishing complete dilatation of the stricture, and entirely removing the patient's symptoms; and without hemorrhage or any other serious bad consequence. The safety and efficacy of the treatment being thus established, the only room for question in regard to the estimation of its value is the permanency of recovery. I am here able to say, that in cases of the very worst kind, the relief may prove permanent, the patient requiring no farther assistance, or merely the introduction of a full-sized bougie at distant intervals; and if this be the fact, it seems reasonable to expect that the result admits of being rendered uniformly favourable through attention to the circumstances concerned in determining its success and failure. An imperfect division of the contracted part—want of care to pass instruments while the urethra is healing, as all injuries of the canal are apt to occasion strictures when this precaution is neglected—and subsequent exposure to a repetition of

the causes which give rise to the disease—may prevent the advantage gained from lasting so long as could be desired. The first of these circumstances tending to cause relapse did not originally occur to me, but was suggested by facts falling under my observation, which led to the persuasion that as a stricture gives the canal a sand-glass form at the part affected, it is not sufficient to cut merely the narrowest portion—the conical-shaped contraction on each side of it also requiring division; so that, instead of being limited to a quarter or half, the incision should extend a whole inch through the coats of the urethra. In regard to the after-treatment, as wounds and bruises of the urethra are apt to induce strictures, unless a full-sized bougie be passed occasionally until the part affected recover from the injury which it has sustained, there can be no doubt that if this precaution is neglected after the operation for stricture, there must be a risk of relapse. And it is no less obvious, that however perfectly the patient may be relieved from an existing stricture, he cannot reasonably claim exemption from the production of another, if he exposes himself to the exciting causes. It will therefore be prudent to enjoin such a mode of life as may, so far as possible, afford protection from the influence of circumstances calculated to act in this way. With due attention to all these points, it is, I think, reasonable to expect that the obstinate forms of stricture in question may be effectually remedied by an operation of the utmost simplicity and most perfect safety; and that the relief will generally, if not always, prove permanent. The case which has led to these remarks is thus related in the hospital books:—

“ Thomas Atkinson, æt. 34, a tall, well-built man, of healthy appearance, was admitted into the Surgical Hospital, May 20th, 1852. Is a native of the county of Armagh,—and has been sent thence by a surgeon, who has treated him for stricture of the urethra for upwards of two years.

“ The following is the history he gives of his complaint:—Rather more than two years ago, after exposure to wet and cold, he was attacked with a discharge from the urethra, unaccompanied by pain or scalding. Previous to this, he had not noticed any diminution of the stream in which he passed his water, but soon afterwards it decreased rapidly. The surgeon, after treating the disease constitutionally for a short time, passed a small bougie, and discovered a stricture at the anterior part of the scrotum. This was quickly dilated, so that No. 10 could be introduced with ease; but, notwithstanding, his micturition was in no way improved. In an hour or two after the instrument had been withdrawn, the stream was as small as ever. Frequently he suffered from complete retention for hours together, and was only relieved at length by having an instrument passed, immediately after which he could make water in a tolerable stream, and without pain or effort. In consequence of

the continuance and obstinacy of the complaint, "caustic" was applied, eighteen months ago, to the strictured part of the canal. It caused him great pain at the time, and the difficulty afterwards in voiding urine was considerably increased—the stream was smaller—the retentions more frequent—and the pain and discharge much greater. Three months after, the "caustic" was again applied, and again his sufferings were aggravated; a hard knot appeared on the urethra, opposite the seat of stricture, and he was rarely able to pass a drop of urine without first introducing a bougie. A year ago, a third application of the "caustic" was made, and he was directed to keep a large elastic bougie, No. 10, constantly in the urethra, and never to remove it except when he desired to pass water. He wore it for two months, night and day; and during this time did not suffer any pain or inconvenience, beyond that arising from the instrument. Two days after renouncing its use, all the symptoms of stricture had returned as severely as before, and ever since he has been obliged to pass an instrument before micturating. During the last three months he has always carried a common fowl's feather, about the size of a crow's quill, in his pocket, and has used this instead of a bougie, passing it through the stricture, allowing it to remain for about a minute, and then withdrawing it; he has been able to pass his water in a good stream. If he neglects to make water for many minutes after taking out the quill, he is obliged to re-introduce it before he can pass a single drop.

"On examination, No. 6 bougie is found to pass without difficulty fairly into the bladder. A stricture is encountered three inches and a half from the meatus, which gives way readily upon slight pressure, but grasps the instrument when withdrawn; and at a corresponding part externally, a narrow thickened induration is felt by the fingers placed on the under surface of the urethra."

This case affords a very distinct example of the contractile form of stricture, and well illustrates the futility of dilatation as a means of remedy for this condition. In all strictures remediable by dilatation, nothing can be desired better than the simple metallic bougie, which, introduced gently at sufficiently long intervals of time, excites absorption of the thickened canal, and gradually restores it to a healthy state. But the would-be improvers, who propose to dilate the urethra upon the same principle as they would stretch the finger of a glove, will meet with little success, except in those cases, of no rare occurrence, where the contraction exists only in imagination; and whether they attempt to accomplish their object by separating the branches of an instrument passed through the stricture, or endeavour to do so by introducing a succession of tubes upon a guide—after the fashion of that curious contrivance, the "Dublin Railway Catheter"—will either effect no good that could not have been attained more easily and safely by the bougie, or seriously endanger the life of the patient. The patient being now placed upon the

table with his knees bent, and supported so as to expose the perineum, I introduce a grooved director through the stricture, and then, while the scrotum is held up, make an incision about an inch in length through the integuments, exactly in the line of the raphé. I next insert the knife into the groove of the director below the stricture, and push it forward so as to divide the contracted part of the canal nearly to the same extent as the external incision. The director being withdrawn, I pass a silver catheter (No. 8) into the bladder, and secure it by proper tapes. The operation, you see, is thus completed in less time than would be required for its description, with the loss of not more than a teaspoonful of blood, and with so little pain, that a smile has never left the patient's good-humoured intelligent countenance.

### *Operations.*

The patients subjected to operations yesterday—viz., the woman, for tumour of the breast; the man, for removal of the testicle; and the boy, for amputation at the knee-joint—are all doing well. The man, also, who, a few days ago, had his hand amputated, is going on favourably. With regard to these cases, I beg, in the first place, to notice the way in which the spermatic cord was treated. The fear of its retraction when divided has been a source of great dread to surgeons, some of whom, even at the present day, put a ligature round the whole bundle of vessels, together with the *vas deferens*, as the simplest and most effectual preventive of trouble—if not to the patient, at all events to the operator. Since the cremaster muscle, which is looked upon as the agent of this retraction, seems to be merely an extension of the lower fibres of the internal oblique and transverse muscles of the abdomen, and of course cannot draw its insertion beyond its origin, there does not appear much foundation for the alarm we often see expressed by the strenuous efforts of assistants, who hold the cord with all their might and main. But the plan which I am accustomed to pursue, and which you witnessed yesterday, completely removes all room for apprehension, and, so far from complicating the procedure, really renders its execution much more easy. It consists simply in cutting through the muscular covering, after exposing but before dividing the cord. The vessels are then seen lying like an earth-worm, convoluted, immoveable, and helpless, so that the most timorous of assistants cannot hesitate to hold them between his finger and thumb while the operator divides the cord, holds it in his left hand, and pulling the testicle towards him, separates its scrotal connections with a few strokes of the knife.

As to the amputation at the knee, you will recollect that I was induced to decide upon this operation by the persuasion that it is attended with less risk of life than amputation of the thigh, on account of the cancellated textures concerned in the former, and the dense osseous substance, with its medullary hollow and contents, exposed in the latter.

*7th June.—Amputation at the Ankle.*

The young woman who has been in the Hospital for several weeks past, labouring under disease of the ankle-joint,—which obviously did not admit of any remedy except through amputation,—was in a state of such emaciation and weakness as made me fearful of a fatal effect from the performance of any operation for her relief. The foot was swelled to an enormous size from oedematous effusion, which extended up the leg; and the vitality of the integuments was so low, that a slough the size of a shilling had appeared on the heel, while sores on the back, notwithstanding every effort of precaution, not only afforded additional evidence of exhaustion, but tended to increase it. Within the last few days, it became evident that the patient, so far from improving, was losing ground so rapidly as to make the period for warrantable interference in all probability of very short duration; and I therefore performed amputation at the ankle on Saturday. If my object had been to maintain the character of this operation, by restricting it to cases likely to afford satisfactory results, I certainly should not have resorted to it upon the present occasion, and might have had a perfectly sufficient excuse for not doing so on account of the disorganised state of the limb at the part concerned. But having no desire so to bolster up this procedure, and believing that it would less endanger the patient's life than any other measure that could be adopted, I did not hesitate in my choice. Every thing, you see, has proceeded so far favourably; and the flap, instead of sloughing, already presents a much more healthy aspect than it did at the time of the operation.

The girl who had her foot amputated between two and three weeks ago, is now, you see, very nearly if not quite well. There has never been a bandage or covering of any sort of plaster applied to this wound. It has healed by the first intention soundly and satisfactorily, without the slightest tendency to sloughing. A few days ago I operated in private upon a lady, whose case promises a no less favourable result; and I may add, that I have now performed this operation upwards of fifty times in succession, without any mortification of the flap. This statement I am induced to make by a letter just received from a gentleman who studied here, and is now in London. He tells me that about ten days ago he saw the professor of surgery at King's College perform amputation at the ankle, with commendation of its merits, notwithstanding the risk of sloughing, which he seemed to regard as nearly inevitable; and "strange to say," writes my informant, "on visiting the patient a few days afterwards, I found that the flap had sloughed." In explanation of this result, so different from what he had been accustomed to witness here, he mentions that the operation was performed with complete disregard of a point which I have always believed and represented as essential to success—viz., preserving entire the nourishing vessels of the flap. It is plain that these can only be the anasto-



mosing branches of the integuments; and it is equally clear that cutting at a right angle with the surface must certainly divide them. But it seems that the King's College professor effects disarticulation before he detaches the flap, and therefore *must* apply his knife in this objectionable direction. In representing amputation at the ankle as easy, safe, and free from any risk of sloughing, I understood the operation to be performed in the way recommended; and for the effects, whether immediate or remote, of modifications in the procedure introduced by others, I am of course in no wise responsible. The matter being now explained, I trust that sloughing in London will no longer be considered any disparagement to the success which has attended the operation in Edinburgh.

*Alleged Impermeability of Stricture.*

I have received another letter to-day, which seems to deserve notice on account of the corroboration afforded by it of an opinion which has been maintained by me against opponents no less numerous than united. Strictures of the urethra are believed by many to be occasionally impermeable and proof against the introduction of instruments, so that the patient's relief must be obtained in some other way—as by groping in the perineum for the obstructed canal. This procedure, even when facilitated by the presence of a fistulous opening, is attended with so much difficulty and danger that I see a proposal has been lately made in London to remedy such cases by establishing a permanent opening into the bladder through the rectum, or above the pubes; and to show the expediency of this practice, there is quoted the case of a patient in St Bartholomew's Hospital who, having such an outlet in the latter situation, was under treatment for “syphilis”—from which, says the reporter, it may be inferred that “none of the functions are thereby prevented.” For my part, I have maintained that a stricture is never impermeable; and that if the urine can get out, a bougie may always be got in, so as either to accomplish recovery simply by dilatation, or prepare the way for passing a grooved director—upon which the stricture may be divided safely and effectually. From the letter now in my hand, it appears that a patient in London has, during the last ten years, been treated for stricture by four hospital surgeons, not one of whom ever succeeded in passing an instrument into the bladder. It had latterly been proposed to send the man here, but fortunately, if not for himself, at all events for the interests of surgical pathology, he remained in London, died there, and was dissected by the gentleman who has given me an account of the morbid conditions. Anterior to the stricture, which was seated at the bulb, there were *six false passages*, and the contracted part of the canal admitted the introduction of a No. 3 bougie without any difficulty. Now, had this patient come under my care, and had I been fortunate enough to relieve him, it would no doubt have been said—as has been said in similar cases—that I had *forced* a

passage;—although, whatever Mr Arnott may say to the contrary, I shall always contend that recovery is inconsistent with such a proceeding: or the result might have been explained in accordance with another published opinion, that impermeability may depend upon the place of residence, and that a stricture wholly impermeable in London may be quite permeable in Edinburgh. But under the circumstances as they happened, there is no room for any such fallacies; and it is impossible to deny the fact, that a stricture deemed impermeable by the highest authorities in London, so far from being so, was in truth not very tight. You may be assured, that a pervious passage would always be found if sought for in the same way—that is, by dissection after death; and you will therefore, I hope, understand impermeability to imply nothing more than inability on the part of the practitioner to overcome the difficulty of the case. The less practised and adroit the operator, the more frequent will be cases of impermeability; and incredible as it may seem, I have frequently introduced bougies of the largest size into urethras which in London and elsewhere had been deemed impermeable. Within the last week, I have done so twice.

*Dislocation of the Shoulder-joint.*

A. B., aged 55. Although the patient is a remarkably muscular and apparently strong man, and although the displacement is forward,—in which case reduction proves more difficult than when it is downward,—as only two days have elapsed since the injury was sustained, I think the method we usually employ when little resistance is anticipated, may prove sufficient. The patient being seated on a chair, a band is passed round his chest to secure the scapula, a skein of worsted is fixed by the clove hitch above his elbow, I place my foot upon the seat of the chair, my knee in the axilla, and one hand on the acromion, while with the other I hold the hand of the injured arm so as to effect rotation when desired. The gentlemen on each side then make extension, and counter-extension, with all their might, and I endeavour to assist their efforts by raising my knee, depressing the shoulder, and rotating the limb. The bone still remaining displaced, I lay the patient upon a table, and selecting a gentleman of more than average muscular power, desire him to take off his boot, place himself on the table alongside of, but in an opposite direction to, the patient, and then, putting his foot in the axilla, draw the hand of the dislocated limb towards him with all his force. No effect resulting, chloroform is administered, so as to render the patient completely passive, and the plan of extension with the heel in the axilla is again tried without success. The case, contrary to expectation, proving to be so obstinate, while the patient still remains upon the table, a cushion, placed in his axilla, is secured to a ring in the wall, and the pulleys are applied to the skein of worsted fastened above his elbow. You see that the rope is hardly tightened so as to make extension in the long direction,

when the bone slips into its place. It is seldom that you have an opportunity of seeing these different means employed in the same case, since we are generally able to determine, from consideration of the circumstances concerned, which of them is requisite. But the comparative trial that has thus been witnessed, will not, I trust, prove devoid of instruction.

*Popliteal Aneurism.*

The ligature in this case separated on the 23d day; and the patient, feeling quite well, now desires to leave the hospital.

ARTICLE VIII.—*On Helmholtz's Speculum for Examining the Retina in the Living Eye.*<sup>1</sup> By W. R. SANDERS, M.D.

HAVING repeated successfully Professor Helmholtz's experiments, and been enabled by his apparatus to view distinctly the retina in the living eye, I venture to lay a short account of his discoveries before the profession in this country. The examination of luminous images, and of the structures in the posterior chamber of the eye, hitherto beyond our reach, opens a new field of valuable practical applications in physiology and ophthalmic surgery.

The new eye-speculum consists essentially of two parts,—1st, Reflectors to illuminate the retina; 2d, Lenses to bring its structures within our focus of vision.

*Illumination.*—The possibility of seeing into the posterior chamber of a living eye depends on the fact, that the rays of light which pass into the eye, and fall upon the retina, are not completely absorbed there; but a portion of light is reflected from the retina, and passes out again through the pupil. According to the laws of optics, these reflected rays, in proceeding out of the eye, must pursue, in an opposite direction, exactly the same course which they took on entering it. Thus the rays from a luminous point fall diverging on the cornea, and are converged by the media of the eye to a focus on the retina; in return, the rays reflected from the retina pass diverging to the cornea, and, on leaving the eye, are converged to a focus at the luminous point from which they came. The same applies to a luminous body. The rays from a gas flame unite in a picture on the retina, from which the unabsorbed rays are reflected back to the gas flame, and concentrate there in an image of the picture on the retina; and the reflected image is placed on, and coincides in size and position with, the real gas flame. To see, therefore, into the posterior chamber of an eye, we must bring our vision in the

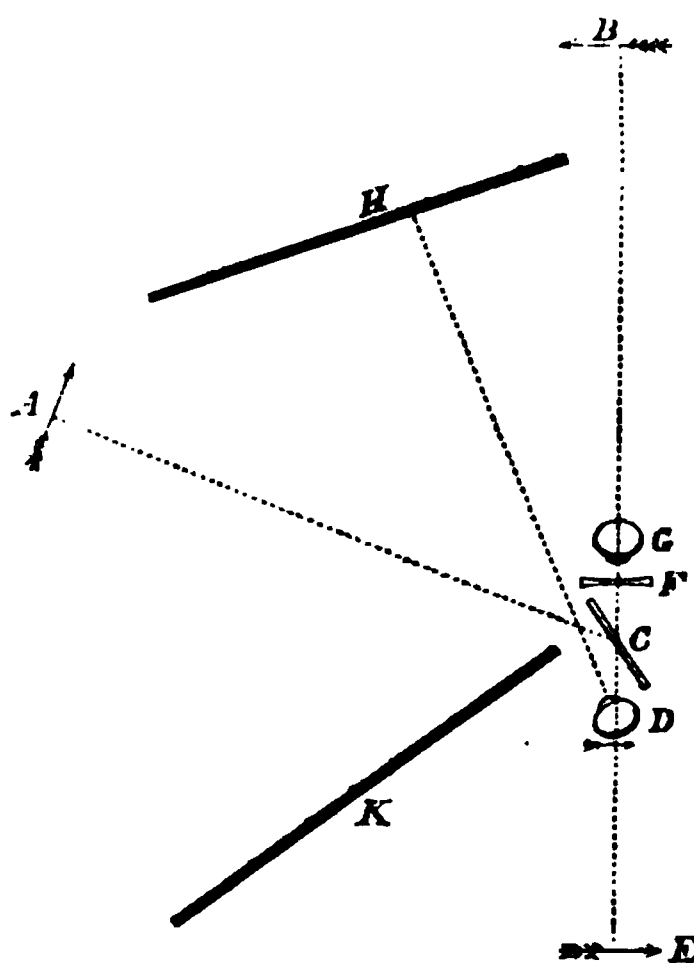
<sup>1</sup> Beschreibung eines Augen-Spiegels zur Untersuchung der Netzhaut im lebendem Auge. Von Professor H. Helmholtz. Berlin, 1851.



strait line of the reflected rays. It is impossible, however, to do so directly, for we cannot interpose ourselves between the eye and the light without intercepting the latter. By approaching, indeed, as near as possible to this direct line, without being upon it, we may catch the irregular rays, and see the pupil of the observed eye illuminated, as Cumming<sup>1</sup> and Brücke's<sup>2</sup> observations have shown; but such methods are inconvenient, and the illumination obtained is insufficient.

By means of a plane transparent reflector, however, such as a piece of flat glass, we can place our vision in the direction in which the rays emerge from the observed eye. Thus let A (Fig. 1) be a

Fig. 1.



flame, whose rays are caught at an angle on a glass plate C, the rays will be thrown along the line CD into the eye D, which will see an image of the flame at B, along the line DB; but the rays reflected from the retina passing out in the same line DC will again meet the plate C; they will be in part turned towards A, but part also will traverse the glass plate C, and go to form a picture at B of the image on the retina; but an eye G, placed behind the glass plate and on the line CB, will meet these reflected rays, and will consequently see the posterior chamber of the eye D illuminated.

The experiment is thus performed:—In a dark room, with a single flame at the side of the experimenters, and on a level with their eyes, the person whose eye is to be observed holds a piece of glass (a microscope glass slip), so as to catch the image of the flame on it; he

<sup>1</sup> Cumming—Medico-Chirurgical Transactions of London, vol. xxix., p. 283.

<sup>2</sup> Brücke—Müller's Archiv., 1847, p. 225.

then, by inclining the glass, brings the image of the flame opposite the pupil of the observer's eye; the latter will then see the pupil of the observed eye luminous, of a reddish-yellow bright colour, like what is called "cat's eye." A person may also see one of his own pupils luminous: standing before a looking-glass, and seeing the image of the flame in the reflector with his right eye, let him bring this image opposite the pupil of the left eye in the looking-glass; the left eye will then perceive the right pupil in the mirror luminous. The observer may also himself place the reflector when observing the eye of another person: by using a very small piece of glass, the reflection from this on a shaded countenance will indicate the direction of the light; and when it is thrown upon the centre of the eye, the observer looking through the glass will see the illumination of the pupil; and by turning the reflector in different directions, and allowing the observed eye to be moved, he may compare the amount of reflection from different parts of the retina.

To obtain the greatest possible amount of illumination, we may either employ a larger angle of reflection, or increase the number of glass reflectors. The most advantageous angles are given as  $70^\circ$  for one glass plate, for three glass plates  $60^\circ$ , for four glass plates  $55^\circ$ . Employing several glass plates is preferable to using a high angle; because the glass being less oblique is more easily seen through, the eyes are better shaded, and at  $55^\circ$  the polarised reflection from the cornea, which interferes with the observation, is nearly all stopped by the glass plate through which the other rays, depolarised on the retina, pass.

For these observations a good oil-lamp or gas-lamp is best; sun light let in through a hole in a shutter may also be used; the observed and observer's eyes should be carefully shaded, and, to prevent mistakes, the eyes not used in the experiment should be closed. This method of illumination is applicable to other cavities with narrow openings, as the ear, the nose, etc.

*Lenses to bring the Reflected Rays to a convenient Focus.*—The luminous rays emerging from the eye D (Fig. 1), and passing through the glass C, unite at B to form an image of the picture on the retina. But at the distance at which B is from the observed eye D, the pupil of the latter, which limits the field of vision, appears so small, that the luminous image cannot be seen through it. We must therefore bring these rays to a nearer focus. Now the rays, on leaving the convex surface of the cornea, are convergent, and our eye can bring only slightly divergent or parallel rays to a focus; all that is necessary therefore is, to interpose a divergent or double concave lens F, of sufficient power, between these rays and the observer's eye. The amount of concavity required in the lens will depend on the distance of the luminous object from the eye; if the focal distance of the lens is equal to FB, the rays will be rendered parallel; or divergent, if the focal distance is less than FB. The conditions are the same as in the Galilean telescope or opera-glass, of which the convex media of

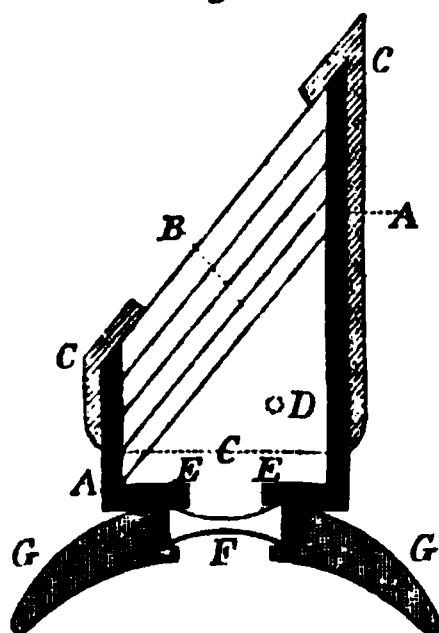
the observed eye represent the object-glass, and the concave lens the ocular. For ordinary eyes No. 10 of the common concave spectacle glasses is recommended; and Nos. 6 to 12 will supply the changes of lenses necessary as the luminous objects are at less or greater distance. Short-sighted eyes will require stronger lenses, or two lenses, one above another.

By means of the lens F, placed as close to the observed eye as the reflectors allow, the picture on the retina will be seen under the same angle,—that is, of the same size as the image B appears to the eye D, or even a little larger; the parts of the retina will also be seen magnified in the same proportion. If the luminous object be distant from the eye eight inches, the magnifying power may be calculated at twenty-four times.

Instead of a concave lens, we might use two convex lenses, of which the first would form an inverted image, which would be magnified by the second. This would have the advantage, that, by regulating the distance between the two lenses, we might accommodate them to the eye looking at near or distant objects, for which purpose we require to change the concave glass. But they are more difficult to manage; there is less light, or the image is less clear; and, as the rays must pass through the axis of the lenses, the requisite steadiness of the observed eye and the instrument is very difficult to obtain. Hence the concave lens is preferred in practice.

*The Eye-Speculum*<sup>1</sup> (Fig. 2) consists of a convenient apparatus

Fig. 2.



holding together the reflectors at the requisite angle and the lens. This is easily done by a square tube, A, A, as short as possible, with one end cut obliquely to form an angle of  $56^\circ$  with the base or other end, which is cut across at right angles to the tube. The exterior is bronzed or darkened; the interior lined with black velvet. The glass reflectors, B, which should be four parallel and well-polished

<sup>1</sup> Fig. 2 represents a view in section of the eye-speculum as made by Mr Bryson, optician, Edinburgh, simplified from the instrument described by Helmholtz.

slips of glass, are fitted on at the oblique end, and fixed close by an open frame, C, C, which is fastened down to the sides of the tube by screws at D; a diaphragm, E, E, and the concave lens, F, are placed at the base, and secured by a hollow eye-piece, G, which can be screwed off and on, so as to allow the lenses to be changed. Biconcave spectacle lenses Nos. 6 to 12 are used; for ordinary use No. 10 is convenient.

*Method of Using it.*—In a dark room, with a flame at the side, and on a level with the eyes, which should be shaded by a screen K (Fig. 1), the reflectors are turned towards the observed eye, and adjusted, so as to throw the light on it in the direction of the axis of the instrument. By a little management, the observer will perceive the inverted image of the flame, and will select a suitable concave glass to see it distinctly. It facilitates much the observation to adapt our sight, as if the image were a distant, rather than a near, object; and the observed eye should be directed towards a screen (H, fig. 1), divided into squares, which are numbered so to regulate the movements of the eye. The observation is also easier with large pupils; hence the importance of shading the eyes; and the use of belladonna might, in many cases, be of great assistance.

*Appearance of the Retina, etc.*—If, when the apparatus is adjusted, and the flame distinctly seen, the observed eye be turned a little inwards, blood-vessels come into view, passing as it were over the flame; and by tracing these from their branches to the main stems, we come upon the central artery of the retina and the optic nerve, at once recognised by its great brilliancy and whiteness; the whole optic nerve is lighted up at once, but no image of the flame is seen upon it. This spectacle of the red vessels on the transparent white ground is of surprising delicacy and beauty; and, when once seen, renders the observer immediately familiar with the objects to be examined; for the largest vessels and the brightest illumination exist here. The artery and vein are distinguished by the deeper colour of the latter, but no pulsation is visible. These vessels dividing pass near the inner side of the optic nerve upwards and downwards, then give off numerous branches on the surface of the retina. There is a semicircular shaded streak at the inner side of the optic nerve, apparently caused by a deficiency of the retina. The retina surrounding the white optic nerve is of a bright red colour, which becomes deeper towards the periphery, and is caused probably by the capillary blood-vessels too small and too faintly illuminated to be distinguished from the gray substance of the retina. Large and small branching vessels pass on its surface, which everywhere shows a distinct image of the flame, brightest around the optic nerve, and fainter towards the periphery. The yellow spot of Sömmerring, or spot of direct vision, is of a dimmer yellowish-gray colour, without trace of capillary vessels; its observation is rendered difficult by reflection from the cornea; and the luminous image is much less bright than on the adjoining parts of the retina.

*Physiological Remarks.*—We can ascertain directly that the eye alters its refracting powers to suit its focus to different distances. Thus the image of a flame and of the retina becomes indistinct when the observed eye looks at an object much more distant than the flame. Or if a thread be held horizontally before a flame, its image on the retina will be seen when near the flame, but become indistinct or disappear when the thread is moved from the flame towards the eye. It also appears that neither the optic nerve nor its fibrils in the retina are adapted for receiving impressions from light; otherwise, when part of the retina is illuminated, the corresponding portion of the periphery should perceive light; and when luminous rays fall on the optic nerve, the whole field of vision should be lighted up, as if the whole retina were acted on. But this is not the case; for even less light is perceived when rays fall on the optic nerve than on other parts of the retina. It is therefore the nerve-cells and corpuscles of the retina which perceive the luminous undulations, and localise them to the point on which they fall.

*The Practical Applications* of the eye-speculum may be expressed thus :—It enables us to perceive alterations on the retina just as the unaided eye lets us see alterations of the cornea, iris, etc. Thus, congestion, varicose vessels, exudations on or in the retina, or between it and the choroid (the fibrin reflecting more powerfully, because less transparent than the retina, and obscuring the vessels). Short-sightedness may be directly detected by the curvature required for the concave lenses. The presence and degree of opacities of the crystalline will be more easily and certainly recognised. In short, nearly all that dissection has yet shown in the dead eye may, by this instrument, be recognised within the living one.

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## Part Second.

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### REVIEWS.

*Class-Book of Botany, being an Introduction to the Study of the Vegetable Kingdom.* By J. H. BALFOUR, M.D., etc. Part I.—*Structural and Morphological Botany.* 8vo. Edinburgh: A. and C. Black.

DR BALFOUR, in writing this "Class Book," has assigned to himself the duty of carefully collecting all the known facts of the science of vegetable organography, and presenting these to the student in a condensed and attractive form.

Like many other physical sciences, botany has, during the last half century, made prodigious advances. It has engaged the attention, or rather has been made the life's business, of many of the most carefully-observing and philosophically-thinking men in the world of science. Any book, therefore, which professes to present a summary of the actual state of botanical knowledge, must embrace an extent of general doctrine, as well as an amount of individual details, not easily mastered, except by the student who applies himself to it with assiduity and earnestness of purpose. We suspect that many of those who attend lectures on botany, especially under the compulsion of a medical curriculum, will be apt to take alarm at Dr Balfour's octavo, when they find that it is only Part First of the "Class-Book;" and we are free to confess, that to those who only wish to learn so much of botany as will enable them to pass current with a medical board, a good deal of the "Class-Book" will appear superfluous. But Dr Balfour, though a member of a medical faculty, bears in mind that he lectures to, and writes for, more than medical students; that botany is not a mere collateral department of medical instruction; that it forms an appropriate part of the education of a gentleman, be his profession what it may, as the science which lays open to him one of the most attractive chapters of the great book of the Creator's wisdom. It is absurd to look upon botany in any other light. We would insist upon its being attended to by all our medical students; but by no means upon any idea that it is directly of practical use to the majority of them when they become established in life. This truism is much in favour with a certain class of people, who call themselves the practical men, and who sneer at pure science,—i.e., at what they know nothing of. *Cui bono?* is their cry. Will a doctor's rhubarb answer his purpose a whit better, that he is at home with the Polygonaceæ? or will his jalap be more successfully cathartic, that he can discuss most learnedly the claims of *Exogonium* to be held as generically distinct from *Ipomœa*? Certainly not; but his habits of observation will be improved, his knowledge of physiology, and of vital phenomena generally, will be more secure, because resting on a broader base, than if his attention had been confined to the human frame; and above all, if he have studied in a proper spirit, his mind will be enlarged and elevated. This is our answer to those who are perpetually calling to us, that of the students who attend botanical lectures, very few in after-life show any fruits from their phytological studies. It is quite true that only a very small number collect herbaria,<sup>1</sup> or plunge into the momentous question as to how many prickles on a raspberry are required to constitute a distinct species of *Rubus*; but it is as true, that very few depart

<sup>1</sup> Some of them, however, do; and we wish that more of our brethren on foreign service did. Let no man be deterred from collecting, because he cannot classify and name. Let him gather and preserve specimens, noting habitat and time of collecting, and send home the collections to those who have more time, opportunity, and knowledge for correctly examining them.



from the botanical class-room without having acquired, if not some scientific, at least some mental, improvement.

We are not entitled, from anything that Dr Balfour's preface contains, to assert that these are his ideas; but we are led to suppose that they are, from the plan upon which his book has been written. He has made it a condensed systematic work, not a mere grinding book, and he has executed his task with perfect success. He has most scrupulously noted, and skilfully condensed, all the best-ascertained facts of his science; and by dint of pictorial illustrations, abundant even to profuseness, has made of his "Class-Book" a very elegant volume.

We look with interest for the second part of it; but we hope that the author and publisher will carefully consider the subject of expense. This first part costs half a guinea—nor is it dear at the money; but the second part must cost less. Students have many demands on their purses, and their class-books must not be too expensive.

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*The Diseases of the Bladder and Prostate Gland.* By WILLIAM COULSON, Surgeon to St Mary's Hospital, etc. London: 1852. 8vo. Pp. 485. Fourth Edition.

MR COULSON'S work on the "Diseases of the Bladder and Prostate" has long enjoyed a well-merited reputation. Since the publication of the third edition some ten years have elapsed—years, moreover, during which, if surgical pathology has not made many important advances, surgical writers have at least manifested unprecedented activity. And as diseases of the bladder, prostate, and urethra constitute no small fraction of the ills to which flesh is heir, and which it is the province of the surgeon to alleviate, we cannot wonder at the number of surgical treatises devoted to their description which have rained upon us from the press. To the literature of these regions, the chemists, physiologists, and physicians have very liberally, if not so bountifully, contributed. The microscope too has been brought to bear upon the subject, and with such success, that the scrutiny of urinary deposits has become a "little science," with which every student is expected to make himself familiar. Mr Coulson's work may be regarded as an encyclopedia of facts ascertained up to the present time; for, with great industry, the author has waded through the mass of literature to which we have just alluded, gathering materials from every available source, and weaving them into a connected form. We do not mean to insinuate that the treatise before us is a mere compilation; on the contrary, the practical experience of the author has enabled him to introduce much that is original and interesting in surgical pathology; and to the general excellence of

the commentary, in which means of diagnosis and of treatment are illustrated, we bear willing testimony.

The first eighty pages are devoted to the consideration of the different states of the urine in health and disease; and it is here that the author has chiefly availed himself of the labours of the physiological chemist and observations of the practical physician. A number of wood engravings are introduced, to illustrate the various forms of urinary deposits observed by means of the microscope. It may not be superfluous to record here our firm conviction, that various points in uroscopy still remain open for original investigation; and in particular, that the evidence which connects some of the rarer crystalline forms with certain chemical urinary constituents, is very flimsy, and absolutely requires confirmation. We have no doubts about the identity of the rhomboidal or lozenge-shaped crystals with uric acid; the prismatic and feathery phosphates, the beautiful octohedra of oxalate of lime are likewise acknowledged facts in animal chemistry; and the six-sided plates of cystine we admit to be highly characteristic. But the mystery of the dumb-bell crystals has not yet been solved. Are these curious bodies forms of oxalate of lime, as Prout and Golding Bird once maintained, or are they oxalurate of lime, as the latter now believes, or do they contain lime at all? Even some of the different forms of lithates have not been established on unexceptionable evidence. Thus, in the delineations given by Mr Coulson at p. 38, we find lithate of ammonia sometimes represented by dark nodules beset with crooked and pointed radii; and, in the drawing of lithate of soda on the next page, certain bodies are introduced, which we confess our inability to distinguish from the former. It may be that the same form is common to both salts, but in that case it were well that we were put in possession of some test, by which their different chemical composition might be readily discriminated.

The diseases of the bladder, including irritability, spasm, paralysis, and inflammatory affections of its different tissues, occupy eight chapters; one is devoted to cancer and tubercle of its mucous membrane, one to wounds and injuries, and one to hernia of the bladder.

The subject of urinary concretions, and the various operations practised for their removal, are next discussed. In the course of the comparison of the respective merits of lithotomy and lithotripsy, we find a most lamentable example of the small reliance which can be reposed in medical statistics. It might be supposed that an honest appeal to the results of the practice of distinguished hospital surgeons would at least furnish an approximation to the true success or mortality which attends the latter operation; and to whose practice could the appeal be more appropriately made than to that of M. Civiale?—

“ M. Velpeau has published an analysis of the cases of M. Civiale, the most experienced lithotritist of the present day, which are arranged in five series:—



Series.	Number of Cases.	Cured.	Dead.	Unrelieved, the stone remaining.	Otherwise, Success in	Failure in
1st.	83	41	39	3	41	42
2d.	24	13	11	0	13	11
3d.	53	30	15	8	30	23
4th.	30	18	8	4	18	12
5th.	16	6	7	3	6	10
	206	108	80	18	108	98

“ That is to say, of 206 patients operated upon, 108 (a very little more than 1 in 2) recover immediately; 80, or nearly 1 in 2½, die; and 13 retain the stone, and will be lost;—108 cases cured, to 98 in which death is immediately induced, or may not be averted within a brief interval of time. M. Civiale has, however, since published an account of his own cases, which differs most materially from the above statement.

“ Of 303 patients, says M. Civiale, who underwent lithotrity, 296 were cured, and 7 died; of these only 6 were under my treatment; for M. Arnoux, who figures in this table, and in that of the relapses, was not operated upon by me a second time.”

Here is truly a discrepancy! According to M. Velpeau, the cures effected by M. Civiale's procedure are 52·4 per cent.; the deaths which follow it, 38·8 per cent. According to M. Civiale, the cures are 97·7 per cent., the deaths 2·3 per cent.

In concluding our notice of Mr Coulson's treatise, we beg to congratulate him upon his rare good fortune in being the author of a surgical work, which has gone through four editions.

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## COLLOQUIA DE OMNIBUS REBUS.

### COLL. II.—POLYLOGUE ON SPURIOUS FACTS IN MEDICINE, ETC.

**TRANSFUSION OF BLOOD.**—*Chirurgus to Obstetricus.* What do you suppose would be the effect of injecting one ounce of blood into the veins of a woman sinking from hemorrhage, after delivery?

*Obstetricus.* Much the same as from holding burnt feathers or hartshorn under her nose.

*Medicus.* Or injecting the blood into her ears.

*Obstetricus.* Very possibly. It might act as a fillip to the remaining power of action, but could be of no permanent advantage whatever. I lately saw a case of the kind, in which seven or eight ounces were injected without producing any sensible change.

*Chirurgus.* Yet you see that at the last meeting of the Medico-Chirurgical Society of London a case was read, the recovery of which was attributed by the author, Mr Soden of Bath, to the injection of an ounce of blood.

*Obstetricus.* There is evidence in his narrative that he was mistaken. It was not till an hour after the so-called transfusion that the pulse began to be felt at the wrist; and in the interval stimulants were sedulously used. Besides, the patient was manifestly suffering rather from the symptoms of collapse connected with inversion of the uterus, than from the effects of hemorrhage. It is a desperate defiance of logic to ascribe such a recovery to transfusion.

*Physiologus.* Have you seen any cases of recovery clearly referrible to the transfusion of blood into patients collapsed by hemorrhage during or after labour?

*Obstetricus.* I have tried transfusion, or seen it tried by others, in eight or ten cases of uterine hemorrhage. All of the patients were insensible, if not indeed moribund, before it was thought fit to have recourse to the remedy. Only one recovered. In most of the others I was greatly disappointed to observe, that a transfusion of several ounces produced little or no tendency to rallying of the pulse, or indeed any other, even temporary, favourable symptom. The more, in fact, I see of transfusion the less faith have I in it.

*Chirurgus.* Nevertheless, Mr Soden's case being entitled a "Case of Successful Transfusion," it will, doubtless, be so recorded, and thus afford another instance of those spurious "Facts," which so encumber the progress of a practical science;—

*Obstetricus.* And be quoted for half a century in the statistics of transfusion by one compiler after another, content with skimming over the surface of things;—

*Editor.* Or made the ground of a charge of ignorance against one more cautious than his fellows, who, looking into the case, may reject it as not genuine. Thus, Professor Syme, on relating a case of ruptured urinary bladder, followed by recovery, and describing it as being unprecedented, was much blamed because he disregarded another case, recorded under the same title; when, in fact, he had found on investigation that this was a case of suppression of urine in a gouty habit, inasmuch as the patient made water in a good stream, without any subsequent inconvenience, four hours after the alleged rupture. His silence was ascribed to ignorance or jealousy.

*Chirurgus.* And criticism, had he exercised it, would have been called personality. Here is another "Fact," of the like quality, in relation to the

**TREATMENT OF CARIOUS TROCHANTER.**—The same London periodical which contains the account of this "successful transfusion," presents us with a statement which will very probably prove a stumbling-block in the practice of surgery—to those at least who derive their

information from such sources. It has long been a recognised principle, that any attempt to extirpate caries from the *trochanter major* is inexpedient, on account of the extreme risk, or rather certainty, of fatal inflammation of the hip-joint being induced by such tampering with the bone. This effect could not have been predicted *à priori*, but being ascertained by dear-bought experience, it ought to stay the hand of even the most adventurous operator meditating so deadly a procedure for the relief of his patient. Nevertheless, Mr Solly tells us, that "caries, limited to a small portion of the neck or trochanter of the femur, sometimes occurs, which might as easily be removed, and with the same prospect of success as the sequestrum we have just been considering;"—which, be it observed, was only found after death. And he adds, that "Mr Knox, in his observations on excision of the pelvic extremity of the femur, has referred to several cases of this kind: two occurred in his own practice." Now, if the reader were to stop here, or if this passage were, as it probably will be, transferred to some abstract or periscope, the inference would be unavoidable, that experience had sanctioned the revival of this exploded practice. But how stand the facts? In one of the two cases mentioned, the disease proved fatal without the performance of the operation; and in the other the patient died from this interference. And who, pray, is Mr Knox?

*Chemicus*. Our old friend, the Doctor, I guess.

*Chirurgus*. Indeed! We have seen the Doctor in many shapes, but never before dressed up by a London hospital surgeon as a surgical authority. Another London periodical of the same week contains farther illustration of the inaccuracy with which what are called "Facts" may be put on record, in the following

NOVEL CAUSE OF DEATH IN A CASE OF STRICTURE.—A lad, who had suffered from stricture of the urethra, and had been treated for it, first in University College, but finally in St Mary's Hospital, was subjected to the old operation for impermeable obstruction; and, as usually happens in such circumstances, he died miserably a few weeks afterwards. On dissection, the perineum and pelvis were found riddled with false passages, and mined with abscesses. Why the case was published at all it is difficult to say, since it could afford no instruction, except to the operator, who, it may be hoped, will take a lesson from experience, and be more careful for the future. But the curious part of the story is, that this death, so amply accounted for by the local mischief, has been attributed to Chorea; which the patient is alleged to have adopted by imitating the occupant of a neighbouring bed. The next writer on chorea, therefore, must either quote this remarkable instance of its origin as authentic; or, passing it over in silence, he must expose himself to the charge of ignorance,—unless, indeed, he adopt the course of protesting against it as preposterous nonsense.

*Obstetricus.* Obstetrical literature, I am sorry to say, has for some time past been rather prolific in such spurious facts; but I know none more memorable than the anatomical views of Dr Lee as to the structure and relations of the placenta. Twenty years ago, there appeared in the "Philosophical Transactions" a long paper by him, with illustrations, to prove that, contrary to established anatomical opinion, the human placenta is not connected with the interior of the uterus by arteries and veins; and that it does not consist of two distinct parts, foetal and maternal, but is exclusively a foetal organ. Being published by the Royal Society, the very fountain-head of science, it was no wonder that Dr Lee's views were incautiously received. He himself, as he afterwards confessed, became aware within a year that he was in error. But, nevertheless, author after author was allowed to repeat the spurious fact. Seiler, Coste, Millard, Noble, Ramsbotham, and others, adopted it. And it was not till ten years after its publication, and nine years after Dr Lee had himself recognised his mistake, that he acknowledged in his published lectures his great anatomical discovery as to the structure of the placenta to be all fudge.

*Chirurgus.* That is one anatomical error disposed of. I shall tell you of one which remains to be corrected. We have been all taught that there are no

STRUCTURAL PECULIARITIES OF TEXTURE IN THE NEGRO, except in the skin. "Take off the skin," it is often said, "and show us how the black man differs from his white brother." But the fact is that the difference between them can be very imperfectly seen until the skin is removed; and the points of difference are not only curious, but also of some practical importance, in regard to operative surgery.

*Medicus.* This doctrine is rather startling. We have all had ocular evidence of the scantiness of calf of the negro, of his thick lips, and of his woolly hair, and I have seen proof pugilistic of the hardness of his head; but I have always thought these were the only well-marked physical characters of the race, in addition to the colour of the skin.

*Chirurgus.* Such is the received doctrine assuredly. But for many years past I have known, from my own observation, the distinctions in question, and have taken every opportunity of pointing them out to others. They refer to colour and consistence; and they occur in the mixed as well as in the pure negro race. *Physiologus* might, perhaps, discover something more with the aid of his microscope. The fat is of a darker yellow, the muscles of a deeper red, the periosteum of a bluish-gray aspect; while the cellular tissue is much more dense and compact, and the cancellated substance of the bone closer and tougher, than in the white race. In consequence of the firmness of the cellular texture, the difficulty of some operations is greatly increased. For instance, I was lately requested

to remove an ugly tumour from the scalp of a lady of colour ; and finding that she had some years previously suffered an operation for the removal of an encysted tumour from the same part, I concluded that a portion of it had been allowed to remain, and was not surprised to experience considerable resistance in effecting its complete separation. But there was another cyst in the scalp, which the patient wished me to remove also ; and I remarked to her ordinary medical attendant, who was present, that the facility with which I should accomplish this would contrast remarkably with the trouble just experienced. I accordingly ran a bistoury through the tumour and superjacent integuments, and seized the cyst with the forceps, expecting to pull it out as usual without the slightest delay or difficulty. But, on the contrary, I found the bag firmly adherent, so as to come away piecemeal, and not without very considerable exertion. There can be no doubt that a similar embarrassment had been experienced on the former occasion ; and thus it may be understood how a portion of the cyst came to be left. Still more recently I had to perform amputation at the ankle-joint in the case of a lady apparently of African origin, and prepared the gentlemen who were to be present for seeing the difference of texture, as well as the difficulty that would be experienced in the operation from the consistence of the different textures. The flap was formed with no less labour than if it had been dissected out of the tough substance of a porpoise's tail ; but, nevertheless, healing took place by the first intention, and a perfect stump has been produced.

*Chemicus.* I do not know any more remarkable instances of falsehood accredited by inveterate usage than some of the old hallucinations in toxicology. From time immemorial, book after book declared that opium, as a poison, causes dilatation of the pupils, until Orfila taught us to look with our own eyes, and observe how contracted they always become. Hemlock was constantly said to kill by convulsions and coma, till Dr Christison found by experiment, that on the contrary death arises from asphyxia by paralysis. The distilled water of cherry-laurel had been in use for a century at least, as an article equally wholesome for physic and liqueurs, until Mrs Boyce of Dublin, indignant that a friend's death should be ascribed to her recipe for ratafia'd brandy, swallowed a glass of her own treacherous cordial, and sunk inanimate beside her bottle. Oxalic acid too was familiarly employed for such common domestic purposes, as the cleaning of brasses, polishing of boot-tops, and making acidulous drinks, when Mr Royston startled the world no longer ago than 1814 with the discovery, that we had been making a familiar thing of a deadly poison. From antiquity, down even into the present century, lividity of the dead body was an acknowledged indication of death by poison in the abstract. What dogma is more preposterous ? And yet the time is not long gone by, when a scolding wife and a bully of a husband ran no little risk of the gallows, if their helpmate looked a little blue after death.

*Chirurgus.* What do you say to the famous case of the cock-sparrow, which, unless my recollection deceives me, Sir William Hamilton used to play off with effect upon the phrenologists some years ago? Gall triumphantly stated that the great proportional size of the cerebellum in this salacious specimen of feathered creation aptly illustrated the soundness of his theory, which placed the amatory organs in the hind-head. Author after author quoted the "fact," that the cock-sparrow had a prodigious cerebellum. Nobody doubted it. At last, instead of Gall's word being taken for it, the creature's parts were actually weighed; when, to the general surprise, it was found that the relative size of the sparrow's cerebellum to its brain and to its body is positively less than in many virtuous birds, and beasts of distinguished continence.

*Medicus.* These are all very absurd errors, and some of them unaccountable. It is some satisfaction, however, that in medicine the correction of such errors usually leads to the discovery of important truths. I know not that so much can be said for another spurious fact, of which the mention of Sir William Hamilton's cock-sparrow reminds me, and which he has himself circulated in circumstances peculiarly unpardonable. But the error is one which it may be worth while to try to arrest before it goes farther.

SIR WILLIAM HAMILTON AND THE MEDICAL FACULTY OF EDINBURGH. Have any of you seen what he says of his University colleagues, the Medical Faculty of Edinburgh, in his recent work, which he calls "Discussions in Philosophy?"

*Obstetricus.* To which of his attacks do you refer? For the Faculty share largely in his attentions.

*Medicus.* To a spurious "fact" in the "Scottish University Commissioners' Report" of 1831, which he has done all in his power to authenticate, regardless of the injury to accrue to the characters of his colleagues and his University. The Commissioners in their Report unguardedly charged the Medical Faculty with laxity in their duty as examiners, and ascribed it to their desire to pocket graduation-fees. In support of this injurious accusation, the Report adduced no other evidence whatsoever, except that for many years the number of graduates had increased in a much greater proportion than the number of students,—a circumstance of which the Commissioners say that "no explanation had been given," and which, they add, "they are satisfied cannot be accounted for from any external causes."

Immediately on the publication of the Report, the Medical Faculty addressed a remonstrance to the Commissioners, representing that none of them had ever been asked for the explanation which was stated not to have been given; that no suspicion had even been expressed on the subject; that the fact of the greater comparative increase of graduates had not been so much as mentioned in the examination of any Professor; and that the fact was easily referable to the following combination of external causes:—viz. 1. The afflux



of old students after the close of the war in 1815, who, unable to graduate at once for want of time and the incessant demand for medical officers for the public services, afterwards arrived annually in great numbers to take their degree, some without any additional study, others after a single year only of University attendance. 2. The preference publicly given at that period by the army and navy medical boards to candidates for admission into these services, who possessed the Edinburgh degree. 3. A decision of the Irish judges, which deprived of the right of using the title of M.D. many practitioners in Ireland, who had long been practising as physicians and doctors upon a Trinity College "Testimonium," and who were thus obliged to go elsewhere to obtain a legal right to a title which they were found by this decision to have usurped. 4. The extension of the course of study of the Royal College of Surgeons, lessening the difference between the requirements for their license and for the Edinburgh degree. The Faculty farther added, that from 1796 to 1831 the proportion of rejected candidates had increased from one in fifteen to one in five--of itself a sufficient answer to the charge of avaricious laxity.

Unfortunately the Commission had discharged all its functions, and no longer existed as a body, so that the members could not formally or officially withdraw the false accusation. But the Chairman, in his own name and that of several other Commissioners, apologised for the affront which had been offered; and more than one of them, in terms not less indignant than the remonstrance of the Faculty, disclaimed individually all participation in the charge, and declared that it had been triumphantly answered.

But what did that signify? The charge stood nevertheless uncontradicted in a Blue Book;—and there it stands for ever—a perpetual puddle at everybody's hand, who may wish to throw dirt at the Medical Faculty of the University of Edinburgh.

*Chirurgus.* I remember that, in 1845, Professor Quain, of University College, London, made powerful use of it in a pamphlet, which, ungenerously and unprovoked, he published with the design of prejudicing the promoters of the medical bill of the day against the University of Edinburgh and its graduates.

*Medicus.* That attack could fortunately be met in good time in the quarter where it was intended to tell. But the attempt of Professor Quain was a venial error compared with that of Sir William Hamilton, who, as a former secretary of the Senate, ought to have known the truth. Unmindful of the cock-sparrow, he appropriates the blunder of twenty-one years since, reprints the accusation at length, stamps it with his approbation, denounces the academic honours of his own University as worthless, and traduces the characters of his medical colleagues.

*Chemicus.* These are hard words. I should like to see what he has said to deserve them.

*Medicus.* The whole discussion should be read, in order to appre-

ciate the *animus* of the Professor of Logic and Metaphysics. But here are a few passages. "As to the largeness of the relative number of medical degrees granted by the University of Edinburgh,—this, so far from being in my opinion matter of honour and satisfaction, should in the circumstances cause only humiliation and regret. For it exhibits nothing but decline;—decline in the number of students,—decline in the requirements of examination,—decline in the qualifications of candidates" [p. 639]. "English grammar and spelling are, by the confession of Edinburgh medical professors, luxuries, not necessities, for those whom the University proclaims to the world as meriting and having received her 'highest honours in medicine'" [p. 640]. "Our Alma Mater, degraded by her members, selling, for their private interest, her highest medical honours, at a lower literary price than is exacted, not only by the other academical bodies, but even by the inferior licensing corporations, is in fact constrained by her own officers to convert her seminary of science into an asylum of ignorance, covering the country with her annual issues of 'graduated dunces'—of 'doctores indocti'" [p. 630]. "There is here no longer any assurance, not to say of superior erudition, but any guarantee against the lowest ignorance, afforded to the public in a medical degree" [p. 631]. "These medical professors"—

*Omnes.* Hold! Hold! No more! We are quite satisfied.

*Medicus.* Then listen for a moment to what Sir William says of medicine itself. "The history of medicine,—and of medicine too when yet a learned and philosophical profession"—

*Physiologus.* When it was all hypothesis and Latin folios.

*Medicus.* "Is nothing else than a marvellous 'History of Variations'" [p. 638]. "Homœopathy and the water cure are now and here blindly anathematised as heretical; in the next generation it is not improbable that these same doctrines may be no less blindly preached as exclusively orthodox. Such is poor human nature! such is corporate, such is medical, authority." *Ibid.* "Fly"—

*Chirurgus.* But, pray, does not this tirade proceed from one who reposed faith in the whole follies of animal magnetism?

*Medicus.* Not the whole. I believe he used to admit himself not quite satisfied with the evidence that a man could read with his stomach.—"Fly doctors and doctor's drugs, as you wish to be well" [p. 252]. "The most intelligent authorities of the profession \* \* \* agree that on an average their science, at least its practice, is a nuisance and send physic to the dogs" [p. 253]. "Priessnitz,"—one of his most intelligent authorities—"declares that the most and the worst afflictions, which flesh is not heir to, but which water has to remedy, are the doctor and the drugs." *Ibid.* "Has the practice of medicine made a single step since Hippocrates?" *Ibid.* "I have heard"—

*Editor.* This is fearful. If his friend Hippocrates could come at him, I think I know what he would prescribe for his case.

*Medicus.* Ἐλεβορος μέλας?

*Editor.* But seriously, can you account for this fury against physic and his University colleagues? It seems something supernatural.

*Medicus.* Not to those acquainted with the author's proceedings as a member of the University Senate. The secret partly peeps out in the following additional specimen:—“These medical professors, now constituting the predominant influence in the *Senatus Academicus*, take upon them, and are quietly allowed, to administer, according to their lights, the affairs of this intended school of learning, and to lavish for their personal interest, and not for the common good, *trusts* fondly confided to the *Senatus*, when the *Senatus* was still comparatively a learned, intelligent, and well-balanced body” [p. 641]. “The personal interest of a majority of its numerous members is now opposed to the general interests of learning, of the public, and of the University, as an organ of education. This is too manifestly shown in the misappropriation also of the funds left by General Reid, ‘*to make additions to the Library, or otherwise to promote the general interest and advantage of the University, in such way as the Principal and Professors shall in their discretion think most fit and proper.*’ This bequest, through the preponderance of a special interest [the Medical Faculty], which has grown into command of the *Senatus* since the will was made,—in opposition to the manifest intention of the testator,—and in opposition to the most significant warnings both from within and from without the body, has been diverted, not only to *special* purposes, but even to the personal advantage of a complement of the trustees;—the small majority refusing a preliminary inquiry, and not listening to the information offered, in regard to the *general* wants of the University; overlooking all disapproval by the highest authorities of the moral character of the proceedings; nay, resiling from their own previously professed intention of interrogating a court of law in regard to the bare legality of any contested measures” [p. 385].

*Editor.* Of course I am quite prepared to hear that this is all a lamentable hallucination on Sir William's part. But he is not the only assailant of the University Senate on the score of the appropriation of that fund. And yet I have not heard that any answer was ever made on the part of the Senate to such scandalous charges. Is there any objection to the public learning the truth?

*Medicus.* None in the world that I know of. You will observe, it is a very easy matter for an individual to publish charges against a public body, but a very different thing for such a body as the University Senate to publish a reply. The reply is simple enough nevertheless. The fund in question, which yields about L.2200 a-year, was appropriated thus:—L.500 to the Music Chair and Concert; L.400 to the General Library; L.100 for the Theological Library; L.200 for Fellowships in the Faculty of Arts; L.200 for the Anatomical Museum; L.50 for management; and L.750 to be available for retiring allowances for Professors *Emeriti*, but not to accumulate

when not applied to that purpose. The Senate also reckoned on a considerable sum accumulating before these applications could be all made, so as to supply a fund for the incidental requirements of the Music Chair and other objects; and in this they have not been disappointed.

*Editor.* Then, what is there in this scheme of appropriation to stir the bile of a Professor of Logic and Metaphysics, and against a Medical Faculty in particular?

*Medicus.* The application of an eleventh part of the annual income to so petty an interest and so sectarian an object as the Anatomical Museum,—besides an occasional small grant from the accumulating fund for other medical purposes, such as for purchasing a collection of specimens in Military Surgery, and to defray the expenses of negotiations in London relative to the Medical Bill.

*Editor.* But still I do not see in all this any cause of logical wrath or metaphysical dissatisfaction. I may be nourishing an *idolum specus*; but it really seems to me self-evident that the “general interest and advantage of the University,” so judiciously mentioned by the testator, is greatly concerned in upholding its Medical School, in improving its formerly defective Anatomical Museum, and in defending and extending its political privileges.

*Medicus.* That is, because you are a logician of the old school of common sense. But according to Young Logic, “the general interest” of a body is not composed of the collective interest of all its separate departments. It must be an interest in which every individual of the body directly participates. Now there is no University object in which every one, Professor as well as Student, directly participates, except the General Library. Therefore the whole disposable fund should go to that object. Such was Sir William’s Q.E.D. The Senate indeed had, in favour of their interpretation of the phrase, the consulted opinion of the most eminent lawyer at the English Chancery Bar. But that mattered not, when Sir William Hamilton was of a different opinion.

*Editor.* What does he mean by the Senate, through the preponderance of the Medical Faculty, “acting in opposition to the most significant warnings both from within and from without the body,” and “overlooking all disapproval by the highest authorities of the moral character of the proceedings?”

*Medicus.* He means what had no foundation in fact. The most important of all the applications of the fund for “the general interest and advantage of the University,” was, in my opinion, the establishment of a fund available for retiring allowances for Professors *Emeriti*. Unfortunately, there was an apparent personal interest on the part of the Senate in this particular of the Appropriation Scheme. Therefore—

*Physiologus.* I don’t exactly see that. As every Professor, who had previously retired on being incapacitated by age and long service, had received a retiring salary in one way or another, it really

signified little to any of the Professors of the time whether the Reid Fund was to bear the charge or not. It was undoubtedly far better for the general interest of the University, however, that the charge should be borne by an express fund than, as usual, by a retiring incumbent's successor, or left to the rare and precarious chance of a Government pension; and that every Professor, as he advanced in years, should know he had such a fund to rely on, than that he should be left at the mercy of prejudice or chance.

*Medicus.* Exactly. Still there was an apparent interest; and therefore the Senate consulted the same eminent English lawyer as to the legality of the measure. They were assured by him of its legality, but cautioned as to the necessity of so acting as to avoid misconstruction. They accordingly referred the matter to the leading law authorities who had been members of the Scottish University Commission of 1831. They admitted the great desirableness of a fund for retiring allowances: they declined to give their advice as to the proposed mode of establishing such a fund, because, as members of the Bench, there might be a risk of their compromising themselves in the event of any litigation connected with the Reid Fund; but they gave neither "warning" nor "disapproval." Afterwards, on a special occasion, the Town Council, as patrons of the University, but without any invitation from the Senate, passed a resolution strongly approving of the measure, and urging the Professors, in allusion to the delicate situation in which they might feel themselves placed,—to act for the public good, and in defiance of possible misrepresentation. Several Professors nevertheless hung back, a few from a conscientious feeling of over-sensitive honour, and others avowedly because they had not courage to do what they thought right, for fear of encountering possible public clamour. A majority, however, possessed that courage; and the appropriation was passed. Not quite one-half of the available fund has been hitherto applied in this way.

*Chirurgus.* One result has been, that the Anatomical School of the University has been rendered the greatest Anatomical School in her Majesty's dominions. For you all know well that this never could have been but for the Retiring Allowance Fund.

*Editor.* I see that all this must have proved somewhat galling to an opinionative man. But it surely does not account for Sir William speaking of his colleagues in the disparaging terms you have quoted from his book.

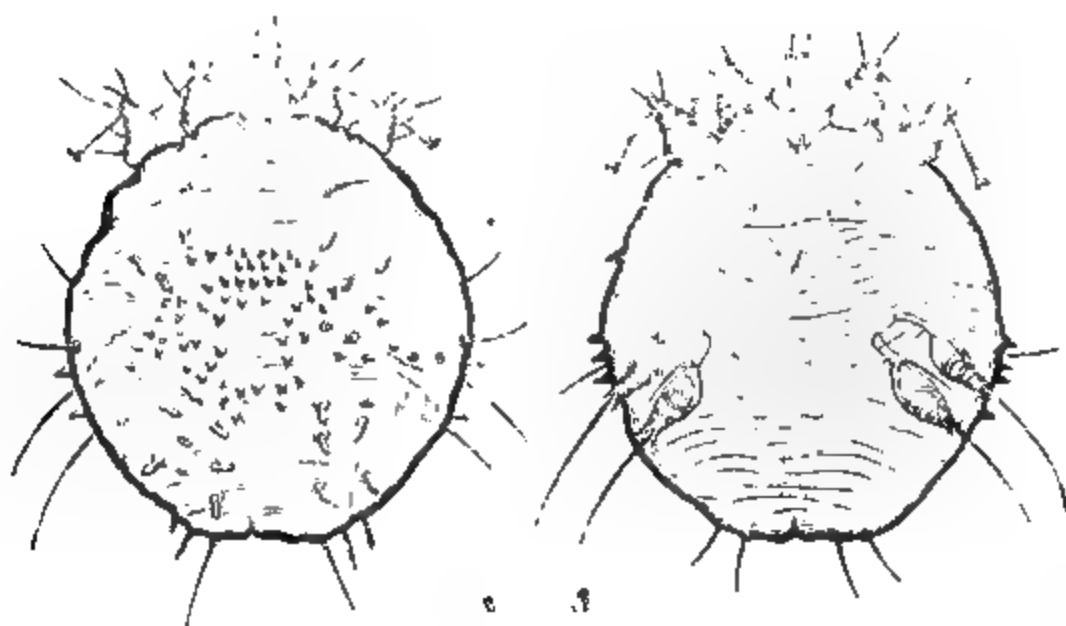
*Medicus.* The Professor of Logic and Metaphysics was never much accustomed to measure his terms in expressing his sentiments. His expressions now are nothing more than he was in the practice of using ten years ago at the meetings of the University Senate, until at length he went so far in a written motion or protest, that the Medical Faculty were roused to resistance, and compelled him to retract. The circumstances of this proceeding were such as entitled the Faculty to believe, that they should hear no more of these ungenerous imputations from the same quarter. But Sir William, it

seems, never forgave them for the injury he had done them. And so he reproduces all his errors and abuse years afterwards,—unmindful of the fate of the cock-sparrow.

*Chirurgus.* Then, “*has* medicine made a single step since Hippocrates?”

*Physiologus.* I think at all events we know something more than he did about the itch, if all be true that we are now told by

M. BOURGUIGNON ON THE ITCH-INSECT. Here is the male with his mate, one hundred times as large as life, but a third less



than she. You will observe that he has suckers on two of his hind feet, and genital organs on his paunch; while his partner is destitute of these appurtenances, is a good deal bigger, and has three sorts of horny spines scattered over her back. He is evidently the more nimble of the two, and in fact may be observed galloping over the skin, feeding now and then by plunging his head into it, but never burying his body in holes, as the female does for depositing her eggs.

*Obstetricus.* Who first discovered him?

*Physiologus.* Eichstedt, and afterwards Lanquetin, an *élève externe* of St Louis. But the best account of the whole tribe is contained in an erudite quarto just published by M. Bourguignon, entitled, *Traité entomologique et pathologique de la gale de l'homme*. The male is not so easily found as the female; for the population seems



to consist of one of the former sex to ten of the latter. He once found, after a successful hunt, only five males on fifty patients. As in the case of other insects, a moderate elevation of temperature greatly increases their friskiness. The females, about to become mothers, then burrow into the cuticle, cutting with their mandibles, and shoving forward with the help of their spiny backs, at the rate of a twelfth of an inch in a night. The males and young females again run up and down, puncturing the skin, and feeding on the human juices and blood-globules.

*Chemicus.* And all alike heedless of the torture they are inflicting on their unlucky possessor, who is just getting warm in bed, and comfortable, as he hopes.

*Physiologus.* These little creatures seem to be characterised by exceeding modesty of manners, and a retiring disposition. The males are assiduous in their attentions to their female friends. But nevertheless even M. Bourguignon's microscope could never surprise them in a single act of indelicacy, or even at the moment of parturition. The female, however, usually remains four days at rest in her hole, laying four eggs in that time. Then she burrows forward for three or four days, and repeats the process; and so on till she has deposited about sixteen eggs in all. M. Bourguignon supposes they escape by a deep transverse groove which is seen between two abdominal hairs. In ten days the young ones break the shell, and come forth in the shape of active infant larvæ with six legs; and after a few days of rapid growth, they throw off their skin like the crustacea, acquire eight legs, and are able to add to the population.

The suckers are organs of locomotion. The mandibles enable the creature to penetrate the cuticle and obtain its food, which passes down a delicate œsophagus into an apparently unorganised molecular pulp that constitutes its whole body. A short delicate tube may be also sometimes observed at what seems to be the anus. There is no visible respiratory apparatus; but globules of air may be seen passing down the gullet, and diffusing themselves through the interior.

M. Bourguignon shows that these insects are the cause of scabies. He is of opinion that the disease is generally communicated from one person to another by sleeping together. For the female never quits her nest except at night, and not even then, if impregnated, unless she is disturbed by scratching, or some other mechanical stimulus. Then, however, she crawls with rapidity, and passes from one individual to another. Accordingly, he often tried in vain to inoculate himself by holding the hands of itchy patients for hours during the day, and in various other ways, during two years. But at length he succeeded by placing on his arm at night an impregnated female, who, eluding his observation, made sundry excursions in every direction, dropping her eggs here and there on the way, and ultimately taking up her quarters permanently in the scrotum, to his no small joy and contentment. He also satisfied himself that

the disease cannot be communicated by the matter in the pustules, by the triturated pulp of living insects, or by the ovum contained in the vesicles. He further is inclined to think that the insect causes irritation both by its actual presence, and also by affecting the constitution with a morbid poison; otherwise it is difficult to account for his having often observed that papulæ, vesicles, pustules, and itching arise on spots which the insect has never infested. It is also worthy of remark, that the *Acarus* of one species of animal, such as the horse or sheep, cannot be induced to migrate to the body of another species. It has a decided predilection for young tender skins, and a distate for hair-bulbs. Hence scabies is most frequent in the young; and in children it spreads over the body, while in adults it affects chiefly the spaces between the fingers and toes, the inside of the thighs, and the genital organs; seven times indeed in ten it is confined to the hands. The eruption is not, in M. Bourguignon's opinion, diagnostic of the disease. The only positive evidence is the discovery of the itch-insect, the *Acarus scabiei*; and this may be detected easily enough by means of a microscope magnifying seventy diameters, furnished with a condensing lens, and attached to a firm stand by means of a moveable arm with several joints. With such an instrument the whole surface of the body may be explored, and the habits of the creatures observed with facility.

*Chemicus.* Then it follows, that the treatment of scabies is a simple affair of murder by poison?

*Physiologus.* So it appears. Instead of trying to destroy the insects by squashing some of them, and administering the product inwardly, like some homœopathic sages, M. Bourguignon watched diligently the action of some unmistakeable poisons on the living insects, and soon arrived in this way at an infallible *ratio medendi*. On watching the effect of an ointment much used at St Louis, composed of two parts of sulphur, one of carbonate of potash, and eight of lard, he first found that, after two frictions and a warm bath, the insects seemed unaffected; in two days they kept close to their burrows; in three days they were flat, but their eggs uninjured; in four days those near the surface appeared dead and shrivelled, and the deeper ones very poorly, and many eggs were now addled; in five days every insect was dead; and in six, all the eggs had lost their vitality. The eruption, however, often remained some time, or even got worse at first from the irritation caused by the ointment, but afterwards speedily disappeared. Hence it is a common practice at St Louis to dismiss the patients, though still covered with eruption, after the insects have been killed; and the number who return is not considerable. From these observations, M. Bourguignon was induced to try the effects of more active poisons. The result was, that iodide of sulphur and solution of iodide of potassium proved most energetic, as they killed the insects in eight minutes; and that the next in virulence was a solution of alcoholic extract of stavesacre, which killed them in fifteen minutes.

The last, however, is preferable as a remedy to the two others, because it does not, like them, act as a powerful irritant upon the patient's skin. When the hands of a patient were immersed for two hours in the solution of iodide of potassium, or in iodide of sulphur, the insects, though lively at first, were found all dead next day, and their ova were destroyed; but the skin was shrivelled, in three days the cuticle came off, and a raw tender surface of true skin was exposed. But immersion for two hours in a solution of stavesacre extract, quickly removed the irritation of the skin, killed the whole insects, destroyed their eggs, except in one instance, and appeared to promote the healing of the eruption. In the end, therefore, M. Bourguignon adopted an ointment prepared by digesting, for twenty-four hours over the vapour-bath, three parts of stavesacre-powder in five parts of lard, and straining the product while liquid. Four days of friction with this ointment will destroy the insects, and cure the eruption; while with sulphur-ointment seven days are necessary.

*Medicus.* Then we must, after all, go back for the cure of itch to the ancient Greeks, who used bruised stavesacre leaves to destroy lice on the body, and cure prurigo and scabies; for which see Dioscorides, *περί Σταφίδος αργίας* Δ. πρῃ.

*Physiologus.* The destructive effects of the seeds on human lice are familiarly known; and the late Dr Duncan recommended the introduction of a stavesacre ointment long ago into our Infirmary Pharmacopœia for the purpose; and I believe it is found a never-failing corrective.

*Medicus.* Your narrative is most interesting. But, is this not a great book on so little a theme? The author must have spent much of his life on the inquiry. The paths of Natural History have been well trodden surely, that enthusiastic naturalists are content to go in quest of so small objects. Did you ever hear what the late Dr Barclay said of similar discoverers in anatomy?

*Editor.* I have heard that his anatomical lectures were sprinkled with sallies of wit and humour, which would have enlivened duller subjects, and made us juniors regret that his mantle had not fallen upon any of his successors.

*Medicus.* On correcting an over-zealous pupil, who fancied he was ever and anon discovering some little undescribed twig of a nerve or artery, he took occasion in his lecture to give us all the following admonition:—"Be assured, gentlemen, there is very little left for us now to find out in descriptive anatomy. The history of anatomy is like that of a harvest-field. First come the reapers; and they have only to stretch out their hands, to gather in a glorious harvest. These are the early anatomists, such as Vesalius, Fallopius, Fabricius, Malpighi, and our own immortal Harvey. After them come the gleaners, who still find ears enough to set up a few stooks. Such were Valsalva, Albinus, Vicq d'Azyr, the two Monros and the Hunters. And last of all come the geese, who, poor things, are

glad to pick up in the stubble a grain or two here and there, with which they go home, cackling with satisfaction. Gentlemen, we are the geese."

*Physiologus.* But, seriously, do you underrate these labours of M. Bourguignon?

*Medicus.* Far from it: No more than of the geese,—the fruits of whose diligence appear at Michaelmas. If the essence and cause of scabies be a louse, and of favus a fungus,—which, strange to say, seems now probable,—we may look for more discoveries of the like kind in regard to other chronic cutaneous eruptions, to the great advancement of the pathology, diagnosis, and treatment of a class of diseases which still continue to be an opprobrium to medicine. If M. Bourguignon's researches be confirmed, I shall also feel ever grateful to him, were it for nothing else than the conclusive removal of a *specific* from the *materia medica*. Previously, for want of positive ideas as to the action of sulphur in scabies, people called it a specific, to cloak their ignorance.

*Physiologus.* I hope soon to see many such researches as those of M. Bourguignon; because it is to such that we must chiefly look for the advancement of rational medicine.—We might hope to see the whole class of specifics wiped out, were it not for the annual additions to the list.—What do you think now of your German specific for eczema and psoriasis,

#### The JUNIPER TAR SOAP?

*Medicus.* The other day, a medical friend told me he had just cured his own eczema with it, and asked me whether it was a thing I had heard of. So I may tell you what I told him.

Three years ago, Dr Velten of Aix-la-Chapelle, when on a visit to this country, mentioned to me, while looking at some cutaneous cases in the Infirmary, that he had found singular advantage from the use of a soap made of juniper-tar in eczema, psoriasis, and lepra. In the autumn of the following year two ladies, who had consulted me for a tormenting eczema of the neck and ears of long standing, and who, after trying various remedies to no purpose, were advised by me to go to Aix-la-Chapelle, returned perfectly cured; and they assured me that, although they did use the mineral water, they ascribed their recovery more to the juniper-tar soap than to anything else. From one of them I obtained a single ball, which I presented last summer to a friend who had been subject for five-and-twenty years to lepra. During that period, he had been sometimes very ill with it,—sometimes much better under various treatments, especially tar-ointment and arsenic,—but never entirely well. Dr Velten's directions were to apply the soap, by means of water, like ordinary soap, for four hours daily, and then to wash it off. I recommended my patient, however, to apply it at night on going to bed, and to leave it until he got up in the morning. In four days he reported to me that an improvement had taken place such as he had never

attained with any other remedy in a month; in a week there was but little of the eruption left; in four weeks a trace only appeared here and there; and in six weeks the skin was everywhere healthy. For nearly six months he remained well; but about a fortnight ago, he informed me that his enemy threatened to revisit him, and that he was about to resume his soap.

Encouraged by this experiment, I begged Messrs Duncan and Flockhart to supply themselves with a small stock, and I have now used it in several cases in private practice, and pretty freely in hospital; and various medical men in Edinburgh have likewise employed it. The result has been variable. I have myself been seldom disappointed in the few cases of lepra and psoriasis in which I have used it; but I have not observed again the same remarkable rapidity of cure as in the instance I have mentioned. In eczema the result has been more irregular. In several cases a cure has been promptly accomplished. In a very bad case of eczema of the legs and loins in a very hale old gentleman of 84, it on the contrary increased the burning itchiness so much the first night, that he could not go on with it. And between these extremes I have observed every degree of intermediate effect. But the general result is that I have not hitherto employed any remedy which succeeds so often or so quickly. I have been quite unable to observe any difference of character in the eruption by which its employment may be regulated. But I suppose you have all observed how uncertain are the effects of other remedies in this disease, even in cases apparently similar. In the aged gentleman nothing has given so much relief as zinc-ointment. In another vigorous gentleman of 70, formerly an East India Civilian, a diluted liniment of citrine-ointment gave him great relief, and in eight weeks effected a cure of the worst and most extensive eczema I have ever witnessed. At one time there was not a spot of his whole skin bigger than a half-crown quite free of it, and the entire back of a well-proportioned figure of six feet four inches was one glowing sheet of weeping eczema. In other instances, the mildest oleaginous applications have proved most serviceable. In one of the two ladies who got well at Aix-la-Chapelle every oleaginous application, mild, stimulant, or astringent, increased the irritation and caused the eruption to spread, until she got the juniper-tar soap.

*Chemicus.* Is not the active ingredient of the soap the same with the *Huile de Cade*, which has been lately used in Paris at the Hospital of St Louis for the cure of various chronic eruptions?

*Physiologus.* Very possibly. I have lately seen a case of inveterate psoriasis of the scalp, of five years' standing, cured in three weeks with this oil.

*Medicus.* The *Huile de Cade* is a distilled spirit prepared from the tar of *Juniperus Oxicedrus*, a species very like the common juniper, and a native of the south of France, Spain, and the



Levant. The Aix-la-Chapelle soap is made with a similar distilled liquor, but apparently less rectified, and said to be obtained from the tar of the wood of common juniper; which in some parts of the continent grows to the stature of twenty feet and more, and has a stem from eight to twelve inches in diameter. This spirit of juniper-tar is converted into a soap by dissolving it in some fixed oil, and then combining the oil with soda-ley; but I do not know the exact formula. The result is a black ball moderately firm, which unites readily with water, and is easily washed off. This constitutes a great advantage over the tar-ointment. Probably, too, the active material penetrates the skin better with water in the form of soap than with oils and fats in the form of ointment.

[*To Chemicus.*] Some one was lamenting a short time ago that we have so many new specifics annually added to the *Materia Medica*. Has it ever occurred to you to consider why these are so numerous? and why so many of them come from London?

*Chemicus.* I think I have a shrewd suspicion.

*Medicus.* The subject is both interesting and important. But it is too long to be discussed so late; and it will keep. Meanwhile, do you care to hear, before we part, from the seat of war in the East,—

A MEDICAL BULLETIN FROM RANGOON?—[*Omnes.* By all means.] This is from a medical griffin, who, while revelling in the pleasures of Calcutta and Dum-Dum in that capacity, was chosen by Dr Montgomerie, the medical head of the expedition, as one of his Hospital Staff, and shipped on board the “Tubal-Cain” hospital-ship on the 26th of March. The Tubal-Cain is an 800-ton vessel, with spacious accommodation for forty patients, and is intended for an intermediate hospital, from which the sick and wounded may be transferred, as they accumulate, to a larger hospital on land, in some healthy situation, distant from the scene of active operations. She was tugged down the Bay of Bengal by the *Hermes* steamer for seven days, with a happy little medical party, 300 unhappy sepoy, and unnumbered sheep, goats, pigs, geese, guns, and other unmedical stores, to the great confusion and delay of the subsequent hospital arrangements. On the 2d April she entered, with the rest of the fleet, the Rangoon river,—a noble stream, deep enough for any vessel, with low, jungly banks, but an open, cultivated country beyond. Here the temperature was 86° F. at noon, and of course very hot; but about two a famous sea-breeze set in, which lasted, to the enjoyment of all, till night. During the next seven days, many interesting occurrences took place, but none medical, except a visit from Dr Bryden, the Last Man of Caubul, and other medical arrivals from Bombay and Madras. On the 10th, the whole magnificent fleet, strengthened by the division from Bombay, sailed up the river, and anchored four miles from Rangoon. In the evening the hospital was opened, in a very encumbered



condition, for six cases of Cholera from the 80th Regiment, two injuries, and an officer from the Fox frigate struck with *coup-de-soleil*.

On the 11th, the fleet moved up close to the town. Some of the vessels got between the Dalla and Rangoon stockades, which opened fire on them. The fire was returned with interest: one stockade was blown up by a shell; and the other was first silenced and then destroyed by a party landing for the purpose. "All the while we looked on calmly, sitting on our poop; the enemy had no spare shot to pepper us with."

The great attack took place next morning. While the war-vessels were clearing the river-bank of its defences and defenders, Dr Montgomerie resolved to make a field-hospital of the only house which the enemy had left unburnt; and as the troops were landing, he took possession of it. It was a Poonji-house, or minister's manse, full of little deities and three great gilt gods, and apparently left for him by the Burmese on purpose. "While our people were sweeping out the dirt and deities, and had scarcely begun to land our hospital stores, five or six doolies, with the wounded, arrived in quick succession, and we found ourselves immediately in the midst of hard work. This continued all day; and as our bedding, medicines, etc., could not be landed in time, we had a great deal of trouble, constantly sending off to the ships for stores.

"There were five officers brought in—two shot through the abdomen, Captain——and Lieutenant——. The latter, a fine young fellow, suffered dreadfully. He died in the evening, lying with the other wounded. Poor Captain —— lived almost a week, with the agonies of a wound of the bowels. This was found afterwards to have passed through two folds of small intestine, the mesentery, and the blade of the left ilium. Of the others, Lieutenant Haines of the 35th N. I. had a narrow escape; a ball having entered near the umbilicus, traversed the parietes, and made its exit in the lumbar region. Trevor of the Engineers had sustained a compound fracture of the arm, and Colonel Bogle of Moulmein a shot in the thigh. Among the men were some dreadful wounds from jinjal balls. These seem to be made by cutting pieces from a bar of iron two inches thick, and hammering them round. The poor fellows bore their pain very bravely. But some of the sights I shall never forget.

"This was not enough, however. The sun also did its work. Brigade-Major Griffiths, and Major Oakes, of the Madras Artillery, were brought in dying from *coup-de-soleil*. A good many soldiers were also brought to us; and Brigadier Warren, Major Foord, Colonel St Maur, and others, were put *hors de combat* by it. We saved all who came to us, by stripping them instantly, pouring quantities of water on their heads, and giving stimulants."

*Editor.* In the last year's No. of the Transactions of the Medical and Physical Society of Bombay, there are several notices on this subject, all concurring with some earlier observations in showing,

that, notwithstanding the similarity of many of the symptoms of *coup-de-soleil* to congestive apoplexy, the main source of danger is an embarrassed, tumultuous, ineffective action of the heart; that bleeding is detrimental, sometimes apparently fatal, in its effects; and that the true treatment is the affusion of cold water on the head,—just as was done in the hospital at Rangoon.

[*Gryps Medicus.*] “At night we found ourselves without a guard, the jungle close at hand, with the enemy in it, and our troops more than a mile off. Dr Montgomerie applied to the admiral, who at once sent his marines: but this was not enough; so the officer of marines set off to the camp, and was allowed a company of the 80th in addition. A ship’s boat, with a howitzer, discharged shell over us into the jungle all night. The enemy there kept firing away at our picquets. We were so tired, we did not care. I went to bed, that is, I lay down on the floor, till midnight; then I kept watch till four; and then again lay down till seven. Mr Baker, chaplain of the Fox, rendered great assistance to the wounded, though weak from bad health. Several times he lay down on my blanket, quite exhausted. Alas! two days afterwards he died of cholera on board the Fox, most sincerely regretted by every one in the fleet. Next day I found myself quite done up with the heat and hard work; and the Doctor sent me off to the ship.

“I am sorry to say, that, in consequence of the troops having been greatly exposed to the sun by day and to the dews by night, and also on account of the want of supplies at first, especially water, cholera made its appearance among them, and did a deal of mischief. More than one officer have been cut off by it. But now, as the troops get housed and comfortable (April 21st), this evil is passing away.

“On the 14th the troops advanced on the great stockade; but, turning its flank, they moved round to the great pagoda, which they carried; and the town of Rangoon was ours. We calculate our killed and wounded at 200, exclusive of those cut off by cholera and sun-stroke.

“Since the action our people have had less to do, the cases only requiring regular attention. Still they have a good deal of fag. For my part, I am picking up again, though for a time I felt regularly spent. The wounded officers were removed on board the hospital-ship, and are doing well.

“On the 19th an invaliding committee, consisting of Dr Montgomerie, Dr White, and myself, set off in doolies for the troops’ quarters at the pagoda. We first passed nothing but stumps of burnt trees, and the piles on which the burnt houses had stood. Then we got on a brick pathway between trees and low jungle; and this opened on rugged ground, with the great stockade fronting us, which had been intended for our men coming that way, and no other. This consists of erect timbers close to one another, and supporting a transverse beam—a formidable *chevaux de frize* in front

—and a sloping bank of earth behind, with mounted guns pointing under the cross-beam, and holes dug in it for the fellows to lie in. We entered by the gate, ascended a tolerably steep slope between rows of pagodas, and soon found ourselves in front of the great pagoda. This is an astonishing structure, said to be 400 feet in height, and gilt from top to bottom, with many smaller pagodas and ornamented poles around it. I shall never see so remarkable a sight again.

“On the way back we were separated, and my bearers lost their way in the dark. After sundry excursions hither and thither, they got quite stupid; and after asking a Burmese the way, they were worse. At last I came upon some sepoy round a fire, one of whom showed me the way. I suppose it was too desolate a place for an enemy to lurk in. I had no arms except my sword. But I felt more afraid of what Dr Montgomerie would think, than of any foe. He had waited an hour for me, and was preparing a party to look for me.

“Our movements are unknown. I believe that we are only waiting for reinforcements to arrive, in order to advance upon Prome. The plunder consists almost entirely of little deities made of marble, of which I have two about a foot high. The country is very fine, and the climate excellent; so people tell us who know it best, and it has every appearance of being so. The present sickness is attributable to the hard service of the last few days. Dr M'Cosh, an old campaigner, says he has never seen such rough work.”

*Chemicus*. Rough enough, to all appearance, for the “non-combatants” of a war expedition. I see from the despatches, that the surgeon of the Fox is one of the wounded. But of course, say the fighting department, “that is of no manner of consequence to him,—or to us: he is a non-combatant: what right had he to get wounded?” I am only afraid, however, that the bullets don't see the difference; and that the Burmese won't, so long as white men's heads are quoted at fifty rupees each at Ava.

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## Part Third.

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### CLINICAL REPORTS, LECTURES, ETC.

#### CLINICAL MEDICINE.—PROFESSOR BENNETT.

ANEURISM OF THE SUPERIOR MESENTERIC ARTERY AND AORTA—OBSCURE ANEURISM OF DESCENDING THORACIC AORTA—TREATMENT BY THE METHOD OF VALSALVA—PLEURITIS—CARIES OF THE VERTEBRÆ, SOFTENING OF THE SPINAL CORD, AND PARAPLEGIA—SUDDEN DEATH BY POISONING WITH TINCTURE OF ACONITE.

CASE I.—Henry Smith, æt. 35, sailor, admitted December 19th, 1849. States that, about twelve months ago, while at sea, he received a severe blow on the back from the tiller of the vessel. He was knocked down, and lay insensible

for a short time. Since then he has experienced pain in the abdomen and back, and latterly pulsation in the abdomen, and a sensation of tingling and numbness in the thighs, legs, and feet, especially on the left side. States that, about three weeks after the accident, he was admitted into the Liverpool Infirmary, where he remained for about eleven months. He was treated by opiates and other anodynes, and latterly also by leeching and cupping over the pained part of the abdomen. From this treatment he did not receive much benefit. On admission, he is of a dark complexion; appearance strong and robust. A tumour is distinctly seen pulsating in the left hypochondriac region. It is of an oval form, and measures about three inches transversely; its long diameter cannot be ascertained, as its superior portion ascends below the ribs; but the inferior and lateral margins can be distinctly felt. He complains of great pain and tenderness in the region of the tumour, and of a beating, which is increased on exertion, and also upon assuming the erect posture. He feels easiest when lying doubled up, resting on his elbows and knees, and in this position he is generally seen during the day. The pulsation of the tumour is forcible, isochronous with, or immediately succeeding, the heart's impulse. On auscultation, a soft bellows murmur is distinctly heard over the tumour, and is loudest at the lower part. The apex of the heart beats about an inch below the nipple. Impulse tolerably strong. On percussion, the cardiac dulness extends transversely about two and a half inches. On auscultation, the sounds are normal in character; the first is heard loudest over the apex, just below the nipple; and the second is most distinct at least three inches above and to the inside. He has no cough or expectoration. The right side of the chest is more resonant on percussion than the left, both in front and behind. On auscultation, the respiratory murmurs are normal. Appetite tolerably good. Bowels regular. Urine natural in quantity; sp. gr. 1.025, not coagulable; presents a deposit of lithate of ammonia. *Ordered a morphia draught at night.* 23d.—Has never slept properly since his admission. States that it requires a very large opiate to produce any effect upon him. *Ordered to be bled to syncope*, and his diet to be as follows:—*Breakfast.* Bread, four ounces; milk, eight ounces. *Dinner.* Steak, two ounces; bread two ounces. *Supper.* Bread, two ounces; tea, eight ounces. 24th.—He was bled to thirty ounces, without syncope or nausea being induced. The blood drawn exhibits a distinct buffy coat. Pulse 88, weak and soft. Pain easier, and sleeps better at night. 25th.—Dislikes beef for dinner, and would prefer a little rice pudding with the bread at dinner. *To have one ounce of mutton and three ounces of rice pudding for dinner. Twelve leeches to be applied over the tumour.* 26th.—Leeches bled freely, and he is now easier. Urine still presents a deposit of lithates. 30th.—Complains of constipation; pain in abdomen rather increased. *To have Elect. Sennæ, 3j. daily. Ten leeches to be applied to the region of the tumour.* 31st.—Leeches did not bleed so well. Pain still severe. *Applicet. Emplast. Cantharid. 3 + 3 parti dolenti.*

January 2d, 1850.—Blister gave some relief. Pulse becoming stronger. *Ordered to be bled 3xij.* 3d.—The blood exhibited the buffy coat, but in a less marked degree than formerly. No faintness or nausea was induced. *Two ounces of bread to be taken off his breakfast, and half an ounce off his supper. To be allowed a bottle of lemonade daily.* 7th.—Sleeps very badly. *R. Sol. Mur. Morph. Tinct. Hyoscyam. aa, 3ss., Aquæ 3ss. M., to be taken every evening.* 9th.—Sleeps rather better. Pain in tumour somewhat increased. *Eight leeches to be applied.* 10th.—Leeches gave relief. 13th.—Still complains of constipation. *To have a colocynth and hyoscyamus pill daily.* 16th.—States, that for the last three or four days he has felt much stronger, and the pain and pulsation in the tumour have increased proportionally. *Ten leeches to be applied.* 17th.—Leeches gave relief, but still he does not sleep well. 21st.—Pulse tolerably strong. *Ordered to be bled to syncope.* 22d.—He was bled yesterday to twenty-six ounces, without inducing faintness or nausea. To-day his pulse is weak and soft, and he expresses himself much

easier. The blood exhibited a distinct buffy coat. Urine loaded with lithates. 25th. Yesterday he was ordered to be bled until some faintness was induced, and 28 oz. were abstracted before that effect was occasioned. To-day the blood is cupped, the pulse weak, and the urine loaded with lithates. *A chloroform draught at night.* 29th. Again bled to 10 oz. February 8th. Great pain in the tumour at night, preventing sleep, for which sedative draughts and enemata afford little relief. 20 oz. of blood were in consequence taken from the arm to-day, which produced faintness, and at once removed the pain. 19th. Size and impulse of tumour evidently diminished. Does not think he is much weaker since admission, but is unable to sit up so long. Pulse small. To-day pain returned in tumour. 12 leeches to be applied. March 2d. Bled yesterday to 14 oz., without inducing syncope. Blood not buffed. 10th. Pain returned with violence. Again bled to 23 oz. 19th. Again bled to 8 oz. He has continued on the low diet, which was altered to-day as follows: *Breakfast*, 1 biscuit (1½ oz.), tea, 10 oz.; *Dinner*, 2 biscuits (2½ oz.), eggs 2; *Supper*, 1 biscuit (1½ oz.), tea, 10 oz. The analysis of the blood drawn on the 19th is as follows:—

Specific gravity of Serum	...	...	...	1028
Solids in 1000 parts:—				
Fibrine,	...	...	...	4.6
Globules,	...	...	...	42.7
Serous Solids,	...	...	...	88.2
Water,	...	...	...	864.5
				Total, 1000

April 6th. Bled again to 13 oz. 15th. Was strong enough to walk in the back-green, but felt exhausted after it. 21st. Bled yesterday to 34 oz., at his urgent request, insisting that he felt nothing, until he fell back in a state of syncope, from which he slowly recovered. To-day, appearance anemic, pulse feeble, feels weak. May 5th. Has been suffering from constipation, which has been relieved by colocynth and henbane pills. Pulsation in the tumour evidently diminishing. To-day, complains of shooting pains in the back, between the shoulders, and down the arms. As he dislikes the eggs, 4 oz. of calves-foot jelly were ordered instead, the other articles of diet remaining the same. June 1st. Considerable pain, and no sleep for three days. 8 leeches to be applied. 17th. 2 oz. of meat instead of the calves-foot jelly. July 17th. Has occasionally been walking a little in the open air, which, however, causes some pain. Apply 12 leeches. August 9th. Pain increased, induced by hemorrhoids, for which injections of cold water have been ordered with benefit. 16th. Pain in tumour returned. Apply 12 leeches, which caused faintness, for which ʒij. of wine were given. 30th. Went out of the house yesterday by permission. October 20th. Has been allowed to go out of the house once a week for exercise. To-day pain in tumour severe. Apply 8 leeches. 25th. The tumour was observed to be moveable to-day. When he lies on his left side, the prominence is concealed below the cartilages of the ribs, but when he turns on his back, it moves three or four inches towards the right side. He says he has only noticed this mobility during the last week. November 20th. Complains of a sharp pain under the left clavicle, and six leeches were applied there in consequence. 26th. Pain under clavicle continues, but was removed by cupping on the 22d and to-day. December 27th. Bled to ʒviiij., to remove pain in tumour.

1851, February 17th. Since last report, has continued to feel pain in the tumour. Again bled to ʒviiij. 20th. Present diet: *Breakfast*, one roll and tea; *Dinner*, part of a fowl and two potatoes; *Supper*, one roll and tea, wine ʒiv. The tumour now feels hard and solid, is the size of a pigeon's egg. March 30th. Bled to ʒvj. April 2d. Bled to ʒxij. The bleeding generally relieved the severity of the pain, but on this occasion failed to do so, and a blister was applied. May 2d. Bled to ʒx., with relief. 30th. 17 leeches were applied.



June 1st. No relief followed the application of leeches. *To be cupped on the back to 3vj.* June 8th. Pain not diminished, and he was *bled to 3xij.*, which caused great relief. 24th. *Cupped to 3viij.* 26th. *Bled to 3xij.* October 29th. Bleeding has been occasionally resorted to, to relieve pain; otherwise he has remained the same. Diet at present is: *Breakfast*, one roll and a pint of tea; *Dinner*, one flounder and two potatoes; *Supper*, one roll and pint of tea, *brandy 3ij.* December 12th. Since last report has been comparatively easy, getting up daily, and feeling pain only for about two hours after rising in the morning. To-day, the pain having increased, *12 leeches were applied.*

1852, January 10th. Complains of weakness, so that he is obliged to use a stick in walking. 15th. Tumour fully the size of a walnut. Aneurismal murmur greatly diminished. Complains of numbness in left side, and pain in right leg. Walking is more difficult. *Bled to 3viij.* 23d. Weakness in lower extremities during walking increased. On the 29th, was seized with general coldness, without distinct rigour. He had also severe pain in the left side of the chest, increased on inspiration. 30th. There is frequent cough, and copious expectoration not tinged with blood. The left shoulder is also very painful. Skin hot, total loss of appetite, great thirst, pulse feeble, great prostration. On percussion, the whole of the left side of the chest is dull throughout. On auscultation, the respiration is feeble superiorly, and inaudible inferiorly. A friction noise is heard external to the nipple. No crepitation can be discovered anteriorly or posteriorly. *Mixture of ipecacuanha and morphia ordered.* 31st. Cough and pain diminished. February 2d. Sputum slightly tinged with blood. 4th. Cough and expectoration diminished. Sputum consistent, free from blood. Friction still present anteriorly, cegophony posteriorly. 8th. Return of pain in chest and shoulder. Physical signs the same. *Blister to left side.* 16. Dulness less below left clavicle, and slight motion of ribs observed there during respiration. March 1st. Friction now audible over the whole of left side of chest posteriorly, and over apex of lung anteriorly. Percussion clear over upper third of lung, but still completely dull inferiorly. Pain, cough, and febrile symptoms have now disappeared. A bad sore has formed in the sacral region, which has been poulticed, and now exhibits a disposition to slough. The abdominal aneurism has undergone no change. Divided paralysis of motion in the inferior extremities, but their sensibility is unaffected. March 8th. Slough has separated from sacrum. April 8th. Since last report, paralysis in inferior extremities has become complete. He has lost all voluntary power over them, and when they are pinched or pricked, no sensation is produced. He experienced, however, twitchings and startings in the paralysed limbs, but no pain. His evacuations are passed in bed, and the sore on the sacrum continues to discharge. In this state he continued until May 31st,—the lower limbs paralysed, but, with the exception of increasing weakness, much the same as at last report. To-day he swallowed a considerable portion of a liniment, containing one-fourth of tincture of aconite. He rapidly became pulseless. The intelligence, for three or four minutes, was unaffected. The respiration was embarrassed, and he was dead in a period variously estimated at from five to seven minutes.

*Sectio Cadaveris*, June 1st, 25 hours after death.

*Thorax.*—General firm adhesions between pleuræ on the left side; on the right side, slight adhesions between pleuræ at the apex. The pericardium contained three and a half ounces of straw-coloured serum; the blood everywhere fluid; heart healthy; right lung mostly crepitant, with considerable induration and puckering at the apex. On section, several cretaceous encysted concretions existed in the pulmonary tissue at the apex, surrounded by considerable carbonaceous deposit. Here and there also small portions of the lungs were collapsed. The left lung somewhat compressed posteriorly, but otherwise crepitant, and apparently normal. It is attached at the posterior part of lower lobe to a sacculated tumour, the size of a foetal head, in front of the dorsal vertebræ, evidently arising from the descending aorta. The tumour is situated



more to the left than to the right side, and, on being cut into, is found to be only partially filled with recently coagulated blood. At that part of the sac which is adherent to the lungs, its wall is strengthened by the deposition of fibrin in laminae, the whole at its thickest part being an inch thick.

The bodies of the 5th, 6th, 7th, and 8th vertebræ were to a great extent absorbed, being apparently scooped out, leaving the intervertebral cartilages prominent between them. The caries had also affected the heads of the corresponding dorsal ribs on the left side. Posteriorly the tumour had projected about an inch, presenting an oval, rounded surface, which had compressed the spinal cord for about an inch and a half of its length opposite the 8th and 9th dorsal vertebræ. On removing and bisecting the cord, its medullary substance at the compressed portion was somewhat softened, an alteration much more marked for two inches both above and below, where it was pultaceous, gradually passing into the spinal medullary matter of normal consistence. The softening was white throughout, with no red spots; and on being demonstrated under the microscope, with a power of 250 diameters linear, was everywhere ascertained to consist of broken-up medullary tubes. Many of the varicosities had enlarged and separated, forming round, oval, and variously-shaped transparent corpuscles, with double lines, mixed with fragments of the tubes, and numerous molecules, granules, oil globules, and broken-down ganglionic cells. No compound granular corpuscles were anywhere visible.

*Abdomen.*—The pancreas is stretched over an abdominal tumour, the size of a small cocoa nut, in front of the aorta, which is moveable, and tolerably resistant and firm. The stomach was healthy, and about a third full of pultaceous lumpy matter, smelling strongly of linimentum saponis. The other abdominal organs were healthy.

On dissecting the tumour, it was ascertained to be an aneurism formed at the root of the superior mesenteric artery, and partly involving the anterior wall of the descending aorta. It was of an oval shape, with one extremity resting on the vertebræ, the other lying immediately below the integuments. Its long diameter measured four, and its transverse three inches. On taking off a thin slice on the left of the tumour, so as not to interfere with the exit of the mesenteric artery, it was seen to be almost wholly occupied by concentric layers of fibrin, except where a channel, larger near the aorta, but becoming smaller at its distal extremity, allowed a free communication of blood with the efferent vessel.

*Commentary.*—This case has been in the Infirmary two years and a half, and during the whole of that time its progress has excited unusual interest. We had to do with,—1st, An abdominal aneurism ; 2d, A thoracic aneurism, which was not suspected during life ; 3d, The treatment of aneurism by Valsalva's method ; 4th, Acute passing into chronic pleurisy ; 5th, Gradually increasing, and at length complete paraplegia ; and 6th, Poisoning by aconite, and the most rapid death by that drug on record. I shall notice the principal facts of his case in succession, point out the difficulties of the diagnosis, the effects of the treatment employed, and state what occurs to me with regard to the mode of his death.

#### *Diagnosis of Thoracic Aneurisms.*

The thoracic aneurism in Smith's case was not suspected during life. On looking back upon the facts observed when he was admitted, I find that, after receiving the injury which produced the disease, he complained of pain in the back, as well as the abdomen. It is also stated that, when admitted into the Infirmary, "the right side of the chest is more resonant on percussion than the left, both in front and behind." These facts were too vague at the time to enable me to distinguish a thoracic aneurism in addition to the abdominal one, more especially as the respiratory murmurs were normal ; there was no cough, expectoration, or other pulmonary lesion. The idea, therefore, of a thoracic aneurism never occurred to me, nor, if it had, is it likely that it could have been confirmed, although now, on looking back, the importance of the facts above stated are apparent, and prove that such aneurism really existed when he first came into the house. On going over the reports which have been kept of his progress during the two years and a half he has been in the Infirmary, I find it stated that, on the 6th of April, when under the care of Dr Christison, he "complained of shooting pains in the back, between the shoulders, and down the arms." On the 20th of November, in the same year, when under Dr Alison's care, he "complained of a sharp pain under the left clavicle." On both occasions the pain was of short duration. I can find no other symptoms which could be attributed to the thoracic aneurism until the 29th of January 1852, when he was seized with all the symptoms of acute pleurisy. For a long time previously his chest had not been examined, but when, on this occasion, it was percussed, the whole of the left side was found to be dull, both anteriorly and posteriorly. This, as well as all the other symptoms noticed at that time, were ascribed to pleurisy with a large amount of exudation, and on carefully weighing these symptoms and physical signs, I do not see how we could have arrived at any other conclusion ; for a pleurisy did certainly exist, as proved by the friction during life, and by the dense chronic adhesions found after death, although now we can have little doubt that the dulness, increased vocal resonance, and other signs, were for the most part dependent on the aneurismal tumour. Another symptom usually present in thoracic aneurism was absent, viz., hemoptysis, or bloody sputum. On one occasion only was this observed, viz., on February 2d, four days after the pleurisy was established. I remember that it induced me to examine his chest with the utmost care, with a view of discovering if pneumonia also existed ; but, as stated in the report, no crepitation could any where be discovered. I am satisfied, from the

careful examination at that time, as well as when he first came into the house, that there was no blowing or other abnormal sound in the chest caused by the aneurism. It is not to be wondered at, therefore, that from this period the dulness on the left side of the thorax, unaccompanied with other symptoms, should be referred to chronic pleurisy, rather than to a thoracic aneurism. It so happens, also, that you have at this moment a man in the ward, named Finnie, labouring under chronic pleurisy on one side, who presents all the thoracic symptoms and signs which existed in Smith. You may consider, therefore, that the detection of the aneurism was almost impossible; for, supposing even that it had been suspected by any of you, and that your attention had been directed to confirm such a theory, you may be at a loss to ascertain how it would have been supported. Such an idea, however, as to the impossibility at any time of discovering this aneurism, would be erroneous, and would do discredit to physical diagnosis; for there can be little doubt that had the chest been carefully re-examined,—say a short period *before* the attack of pleurisy—I think it would *then* have been apparent that a tumour existed in the chest, and if so, that tumour, from its seat and concomitant circumstances, would have been declared to be aneurism low down in the thorax. It was simply because no suspicion of its existence occurred to us, and because no physical examination of the chest was made *at that time*, that the tumour was not detected during life.

*The general diagnosis* of thoracic aneurisms has always been considered a matter of great difficulty. When, indeed, a tumour with a distinct impulse is perceptible, we, in the majority of cases, know with what disease we have to do. But even here occasional errors by men of the greatest experience have sufficiently proved that the art of detecting these tumours with exactitude has been wanting. Again, when aneurismal tumours are seated at the upper part of the thorax, it is important to determine whether they arise from the aorta, or from the large vessels coming from it, and if the latter, which vessel is affected. Thus aneurisms originating from the upper part of the descending aorta press upon neighbouring nerves, as the superior and inferior laryngeal and pharyngeal branches of the pneumo-gastric, giving rise to various symptoms; or they compress the larynx, trachea, bronchus, œsophagus, or the lung itself, and so occasion laryngeal, œsophageal, or pulmonary symptoms. Lastly, when deep in the thorax, their progress is often latent, as in the case of Smith. Hence the signs and symptoms of thoracic aneurism vary,—1st, According to its seat; 2dly, According to the size of the tumour and its pressure upon neighbouring parts; 3dly, On the character of the aneurism, its formation, and state of the vessel.

The means at our disposal for detecting these aneurisms are,—1st, Percussion; 2d, Auscultation; 3d, Palpation and vascular impulse; 4th, Symptoms.

1. *Percussion*.—That the situation and size of the aorta can be accurately determined by percussion, was first proved by Piorry.<sup>1</sup> I have frequently succeeded, in favourable cases, in marking out on the chest the size of this vessel. To do so with accuracy, it is first necessary to limit the margins of the heart in the manner previously explained (see “Monthly Journal,” October 1850), and then carrying the pleximeter upwards in the course of the aorta, and over the sternum, the dulness of the vessel, when compared with the resonance of the lung on both sides, may be made very apparent. In the same manner, the extent of saccular, or simple, aneurisms by dilatation may frequently be determined with accuracy when seated in the ascending or transverse arch. In such cases, however, the existence of pain often renders percussion impossible, and at all times it should be conducted with great gentleness. When an aneurism is seated in the descending thoracic aorta, its limitation is more difficult, as we have then to percuss through the lung anteriorly. But careful manipulation, and varying the force of the blow, together with percussion posteriorly, will

<sup>1</sup> De l'Examen Plessimétrique de l'Aorta, etc. 1840.

frequently enable us to determine the position and size of the swelling. If, on the other hand, the aneurism be small and deep-seated, while the lungs are healthy, and if, at the same time, no suspicion of the disease be entertained by the practitioner, he is very likely to overlook the importance of slight dulness on one side of the chest. This is what happened in Smith's case, and constitutes a valuable lesson, which none of us I trust will soon forget.

2. *Auscultation*.—The sounds heard in an aneurism may be single or double; and considerable discussion has taken place, whether, in the latter case, the second sound originates in the tumour, or is propagated along the vessel from the heart. This is a theoretical point which is not yet decided. Whether single or double they must be judged of according to their character and seat. With regard to their *character*, they may be,—1st, Soft and blowing; 2d, Harsh and rough, when the vessel is generally diseased, and its lining membrane more or less atheromatous or calcareous; 3d, There may be a peculiar clink, or abrupt harsh resonance, approaching towards, but never reaching, a metallic sound. It is generally heard when a saccular aneurism, free from coagula, is present, with a small opening, having thin and elastic margins. With respect to the *seat* of these sounds, when near the heart, they are generally synchronous with those of that organ, and their discrimination is very difficult. When situated in the arch of the aorta, there is a distinct separate source of sound. This latter can only be successfully studied by carefully comparing the moment of impulse of the heart with that of the tumour, as well as the character and intensity of the cardiac and aneurismal sounds. You should carry the stethoscope carefully from one to the other, and observe the diminution and increase of the murmurs, as you lengthen or shorten the distance from the origin of the sounds. It is necessary also to study the direction in which the sounds are propagated, those of a blowing or rasping character having a tendency to pass in the direction of the current of blood. Hence, in aneurisms of the innominata, the murmur is prolonged in the course of the right carotid and axillary arteries, while those of the aortic arch, and especially its descending portion, may be heard in the aorta, on applying the ear to the back.<sup>1</sup> In this manner careful and *repeated* auscultation, conjoined with percussion, will enable you, in the majority of cases, to determine exactly, not only the existence and seat of the aneurism, but in many cases its form and structure.

3. *Palpation and Vascular Impulse*.—When an aneurism points externally, a tumour and an expansive impulse can be felt by the hand.

The position of the tumour varies according to the part of the aorta, or the large vessel from which it originates. Thus saccular aneurisms immediately above the aortic valves pass downwards. When situated in the innominata, they manifest themselves above the clavicle on the right side. If originating in the transverse portion of the arch, there is often no external tumour; and when it does occur, it generally appears on the left side of the sternum, above or below the sterno-clavicular articulation. Aneurisms, lower down in the arch, are most common in the left thoracic cavity. These rules are by no means absolute; for, although an aneurismal tumour for the most part tends to enlarge in the direction in which the impulse, from the course of blood, is applied,—this, in several cases, cannot be determined in the living body.

The impulse of the tumour is synchronous with, or follows the systole of the heart. Occasionally there is no impulse, a circumstance most frequently observed, when the tumour does not present externally, and is only determined by percussion. The pulse of arteries connected with the aneurism may be weakened or retarded. The pulse at both wrists should be always carefully studied;

<sup>1</sup> For the differential diagnosis of aneurisms of the aorta and of the innominata, I would recommend the perusal of an excellent Thesis by Dr T. S. Holland, presented to this University on his graduation in 1850. It has been since published in the Dublin Quarterly Journal, vol. xiii. New Series.

for if one be weaker than the other, it is clear that an interruption exists in the current of the blood in the axillary artery. This may arise from two causes, —1st, From the vessel being involved in the tumour; 2d, From its being compressed by it externally. The former condition exists most commonly when there is aneurism of the innominate, when the weaker pulse will be on the *right* side. In aneurisms of the arch, on the other hand, the feebler pulse is usually on the *left* side. The retardation of the pulse, when it occurs, is owing to causes very similar to those which affect its strength.

4. The *symptoms* which are present in cases of thoracic aneurism, vary according to the size of the tumour, and the parts on which it presses. When seated at the upper part of the chest, it may, by pressure on the larynx, produce alteration of the voice, more or less cough, and stridulous respiration; by affecting the branches of the eighth pair, occasion increase or diminution of their special functions; impede deglutition by constricting the œsophagus; or modify the respiratory murmur by pressing on the trachea or larger bronchi. Occasionally there is a crepitating murmur in the lung, with many of the signs and symptoms of pneumonia, for which it has often been mistaken, including rusty sputum, dulness, and increased vocal resonance. Pressure of the tumour on the axillary vessels and nerves, may induce more or less œdema of the extremities, and paralysis more or less complete. Sometimes there are dull, gnawing, or lancinating pains in various parts of the chest; but nothing is more remarkable than the size and formidable nature of some aneurisms which have caused little pain. Occasionally there is a feeling of oppression and constriction,—dyspnoea with or without exertion, and hæmoptysis to a greater or less extent.

The combination of the results obtained by percussion, auscultation, palpation, and vascular impulse, and the functional symptoms, vary infinitely in different cases, and their careful detection, combined with a knowledge of physiology, will in the majority of cases enable us to form a correct opinion as to the nature of the disease. It must not be forgotten, however, that there are some cases which have been so obscure as to baffle the efforts of the most able physicians; and that, generally speaking, the deeper the aneurism the greater the difficulty of detecting its exact nature, and the complications connected with it. It is also well ascertained that the symptoms may be simulated by a tumour situated outside and upon the vessel; and occasional mistakes made by the most experienced surgeons, men who, during their professional lives, have carefully examined a large number of these tumours, prove the excessive difficulty of detecting aneurisms, even when situated in the limbs or in the neck. How much more difficult must be the appreciation of these symptoms, when the aneurisms are below the sternum or clavicles, not to speak of their occurrence deep in the thorax. Yet these very symptoms, when *combined with* percussion and auscultation, enable the physician frequently to triumph over the greatest difficulties, and to demonstrate what may properly be called the greatest triumph of his art.

#### *Diagnosis of Abdominal Aneurisms.*

When Smith entered the house, the abdominal aneurism was of considerable size. It measured three inches across. Its inferior and lateral margins only could be felt, the superior portions being covered by the ribs. The impression conveyed to me by examining the tumour, however, was that it was about the size of a cocoa-nut. It was prominent, especially when he stood up, and pulsated strongly. There can be no doubt that its volume must have undergone considerable diminution; for, previous to his death, it felt through the integuments about the size of a small hen's egg;—in some of the reports, it is said of a pigeon's egg, and of a walnut. Yet, as you now see, it is the size of a large orange, elongated. Its form is a long oval, one extremity of its long axis resting deep upon the vertebræ, the other directed towards the skin. Hence, during life, we could only feel one of its rounded ends. You observe, however, that the whole tumour is dense and resistant,—and on section it present numerous



concentric laminæ of coagulated fibrin, with a small canal running through the centre, keeping up the communication between the aorta and the superior mesenteric artery. The man presented habitually a jaundiced skin, which, as in other cases of abdominal aneurism, collected by Dr Gairdner,<sup>1</sup> was, doubtless, owing to the pressure of the tumour on the duodenum and biliary ducts.

The physical phenomena most distinctive of an abdominal aneurism are a swelling more or less defined, an expansive impulse on applying the hand, and a bellows murmur synchronous with, or immediately following, the heart's systole on applying the stethoscope. This bellows murmur is generally loudest over the tumour, and is propagated down the aorta,—although, when immediately below the diaphragm, it may be confounded with the first sound of the heart. The symptoms are very various, consisting of dragging, or other pain, more or less acute and prolonged, owing to pressure and stretching of the neighbouring nerves, together with functional disturbance of one or more of the abdominal viscera. Various cases on record, therefore, have presented a train of very anomalous symptoms, and at various times been considered as different diseases by medical practitioners. A complete re-investigation of the symptoms and signs of abdominal aneurisms is much required. This is a task, however, which will require a thorough knowledge of all that is now known of physical diagnosis and morbid anatomy, combined with great powers of observation, and such opportunities as fall to the lot of few individual members of the profession.

*The Paraplegia and Spinal Softening.*—He first complained of weakness in the lower extremities early in January of the present year; at the end of that month my period of attendance on the wards ceased. In the report of March 1st, I find it stated that there was decided paralysis of motion in the inferior extremities, whilst sensation still resulted when they were touched. On April 8th, the paralysis was complete,—that is, volition failed to cause movement in the lower extremities, and stimuli applied to them failed to induce sensation. Involuntary movements, however, occurred, consisting of twitchings and startings, but he never had pain in the limbs. In cases of myelitis the usual symptoms are, pricking and tingling in the soles of the feet. These symptoms were absent, and the reason of this may, I think, be found in the nature of the softening in the spinal cord. It contained no granular cells, the result of exudation, and its transformation into fatty granules; but the tubular substance of the cord was broken down, forming round and oval fragments of the tubes. Hence it was a mechanical softening, the result of gradual pressure merely. These distinctions have not been hitherto sufficiently attended to in pathology. You will observe that the aneurismal tumour commenced pressing on the left side, and from before backwards, and the symptoms indicate that weakness was felt in the left inferior extremity before the right one was affected,—and that motion was paralysed first, sensation last.

*Poisoning by Aconite.*—The facts which I have been able to make out regarding the unfortunate poisoning of this man are as follows:—On Monday, May 31st, about 11 o'clock in the morning, the attention of Mr Broadbent (now resident clerk) and of Dr Murchison (resident clerk), both of whom were at the time in the ward, were directed to Smith by a groan or cry. He was then observed to be sitting up in bed, leaning forward, and groaning like a man labouring under colicky pains. Mr Broadbent, who was nearest at the time, went to his bed-side, and asked, "What was the matter?" Smith made no immediate reply, but continued to groan, and moved his arms in a feeble manner, and it was noticed by Mr B. that his hands dropped considerably when the arms were raised. He then tried to reach the spit-box, but not being able to do so it was given to him, and he seized it, raised it to his mouth, and spit into it. He then said, with short pauses between his words, "Is there anything wrong with my face?—it is very painful; what medicine have I been taking?" On

<sup>1</sup> Monthly Journal for January 1850.



being asked to point out the bottle on the shelf, he did so, saying, "that little bottle there." On looking at it, Mr Broadbent saw by the label that it was a liniment, composed of Tr. Aconiti, ℥ss. ; Lin. Saponis. c. Opio, ℥jss. Dr Murchison, on being informed what had happened, also went to Smith, found him pulseless, and on letting go his arm observed that it fell down powerless at his side. Smith then repeated more than once, "Can nothing be done for me?—What can you do for me?—Can you get me a vomit?" etc. An emetic of sulphate of zinc was immediately sent for, and it was further observed that the pupils had undergone no marked change, that there was no lividity of the lips or other part of the countenance, that no impulse could be felt in the cardiac region, and that the respiration was more slow and laborious than usual. Dr Murchison now left the patient to get a stomach-pump, and Mr Broadbent saw Smith retch twice, as if endeavouring voluntarily to vomit. He therefore went into the side-room to get a feather, or some object to tickle his fauces with, but was immediately summoned back by the intelligence that Smith was worse. On returning to the bed-side he found that the patient had fallen on his bed, the head thrown back, face and lips remarkably pale, a little saliva running from the corner of the mouth, the respirations occurring at long intervals with gasping, the pupils neither dilated nor contracted, and the eyelids paralysed, when opened remaining fixed, and not contracting on blowing into the eye. He was now insensible, and consequently the emetic, which now arrived, could not be given. About a minute after, Dr Murchison, on hurrying back with the stomach-pump, found him dead. Notwithstanding, more than a pint of semi-pultaceous matter was immediately drawn off from the stomach, smelling strongly of the liniment, and artificial respiration was kept up in vain for five minutes.

The period that elapsed from first noticing Smith's cry or groan, until Dr Murchison's return, when he was dead, is differently estimated by the two gentlemen concerned at five and seven minutes. The liniment consisted originally of Liniment. Sapon. c. Opio, ℥jss. ; Tr. Aconiti, ℥ss., and it is believed that the whole of this quantity (viz., two fluid ounces), was in the bottle when Smith began to drink it. There were found in the bottle afterwards 3v. remaining,—so that the presumption is, that he swallowed three drachms of laudanum, and upwards of two drachms of tincture of aconite.

Whether Smith's death arose from accident, or whether he committed suicide, is not likely ever to be known. Those who knew him best in the ward, as well as the nurse, are of the latter opinion, based principally on the character of the man, which was such as to prevent his mistaking a liniment for a draught. It seems, also, that no one was more habitually careful as to the medicines he took—that the liniment was never ordered for him, and that he must have kept it on his shelf for some days; and lastly, that since the paraplegia had become complete, he had been unusually despondent and morose. With regard to the phenomena produced, it is most likely that, immediately after swallowing the poison, he experienced those violent tingling and stinging sensations in the mouth and fauces which aconite produces, and hence the pain complained of in his face. Being already paraplegic, nothing is known as to how far the poison affected the muscles of the lower extremities; but it is evident that, whilst the intelligence remained perfect, the arms became weak, then powerless. Subsequently, he could not support himself in the sitting posture; and, on his falling back, the muscles of the face, and of respiration, were paralysed, and he died asphyxiated. Previous to this, however, a powerful sedative effect had been produced on the heart, for when first noticed he was pulseless, and shortly after, no impulse could be felt in the cardiac region.

According to Dr Christison, the least variable symptoms of poisoning by aconite in the human subject are, "first, numbness, prickling and impaired sensibility of the skin, impaired or annihilated vision, deafness, and vertigo—also, frothing of the mouth, constriction at the throat, false sensations of weight or enlargement in various parts of the body,—great muscular feebleness and

tremor, loss of voice, and laborious breathing,—distressing sense of sinking, and impending death,—a small, feeble, irregular and gradually-vanishing pulse,—cold clammy sweat, and pale bloodless features,—together with perfect possession of the mental faculties, and no tendency to stupor or drowsiness;—finally, sudden death at last, as from hemorrhage, and generally in a period varying from an hour and a half to eight hours.”<sup>1</sup> Although in this case many of the symptoms just mentioned were not noticed, it must be evident that the leading ones, indicative of the physiological action of the drug, were observed. When the large dose of the poison is considered, and the great rapidity of its effects, it may be easily understood how the minor symptoms, and especially those having reference to the sensations of the patient, were not ascertained, if indeed they really existed.

Dr Fleming considers that aconite may cause death, “first, by producing a powerfully sedative impression on the nervous system; second, by paralysing the muscles of respiration; and third, by producing syncope.” He observes, “that the second mode of death has never been recognised in man; the quantity of the poison taken in no case having been sufficient to exert such an effect on the nervous and muscular systems, as is necessary to induce it.”<sup>2</sup> The case of Smith, indeed, is the only one of this description, so far as I am aware, that has ever occurred, in which the dose of poison was so large, and the death so rapid. It is difficult to separate the effects of syncope from those of asphyxia in such a case, as the first condition must induce the other. Both were apparently combined. It is also difficult to determine how far the effects on respiration were occasioned by paralysis, creeping from below upwards, as in the case of Gow I have recorded,<sup>3</sup> who was poisoned by the conium maculatum, and in whom all the symptoms were observed, as seen by Plato in the case of Socrates. There are some facts, however, noticed by Dr Christison, which lend support to this doctrine; and it will be observed, that paralysis of the hands and arms preceded that of the muscles of the back and face in the case of Smith.

*Treatment by Valsalva's Method.*—A short time previous to the admission of Smith, I treated another case of abdominal aneurism by the method of Valsalva, for a period of forty days,—at the expiration of which time, he walked out of the house, with little assistance, to the nearest cab-stand, a distance of nearly 250 yards, and left the city.<sup>4</sup> In the case of Smith, therefore, the bleedings were more frequently repeated, and greater in amount, while the diet was even more diminished; and yet, after nearly a month's treatment, the pulse was of such good strength, that I ordered venesection to *syncope*—an effect that was not produced after the loss of twenty-six ounces of blood—so that the clerk, afraid to proceed farther, bound up the arm. Three days afterwards, twenty-eight ounces of blood were removed, with the effect of only producing a feeling of faintness. Similar bleedings were practised at no distant intervals, besides numerous applications of leeches, and the restricted diet; and yet the report of 21st April 1850 is, that “he was bled to *thirty-four ounces*, at his urgent request, insisting that he felt nothing, until he fell back in a state of syncope.” I am induced to suppose, therefore, that in this case, as in the preceding one, the treatment had not been carried out to its full extent. The nurse, indeed, now informs me, that perhaps, during the first two months, his diet was really limited; but she thinks so simply because, at that period, he suffered great pain, and seemed very anxious to follow the advice given to him. Subsequently, there is every reason to suppose that he obtained food from his companions, or from some other source. I find from the reports, indeed, that whilst his diet was still nominally at a very reduced amount, up

<sup>1</sup> On Poisons. Fourth Edition. Pp. 871.

<sup>2</sup> An Inquiry into the Physiological and Medicinal Properties of the Aconitum Napellus. Edinburgh, 1845. Pp. 42.

<sup>3</sup> Edinburgh Medical and Surgical Journal, No. 164.

<sup>4</sup> See Medical Journal, February 1850, p. 169.

to July, he was at the same time walking about with considerable vigour. From my attempts at carrying out Valsalva's treatment in these two cases, I conclude it to be impossible to practise it on patients in an open ward, or indeed under any circumstances, without a degree of *surveillance* that it would be very difficult to obtain.

The good effects of the treatment, notwithstanding its imperfect nature, were so evident as to strike all who witnessed it, and to cause the patient continually to request that he might be bled. In fact, after every general bleeding, the dragging pains, and other uneasy sensations, he experienced in the abdomen, invariably left him, and he enjoyed longer or shorter periods of perfect ease; then, the pain gradually returning,<sup>f</sup> when it became unbearable, he was again relieved by bleeding; and so on. During the progress of his case, also, it was observed that the abdominal tumour gradually diminished in size, and became harder. In October, the tumour was ascertained by Dr Christison to be somewhat moveable; but in the following December, when I examined it, it was again stationary. During the whole of 1851 he enjoyed comparative comfort,—occasionally, however, feeling abdominal pain, which was relieved by leeches or bleeding. At the beginning of 1852, the general opinion of all who examined him was, that, on the whole, this case was a remarkable example of the good effects of Valsalva's treatment. Then, however, the paraplegia came on, indicating that the disease was really not conquered, but, by its pressure backwards, was affecting the spinal cord. Then came the attack of pleurisy; and from this period it was evident the disease would terminate fatally.

The examination of the body after death was, in this case, not only important, as determining the nature of the aneurism, and in a diagnostic point of view; but it served, in my opinion, to point out what value must be attributed to Valsalva's treatment. It affords an example of a wide generalisation to which the cultivators of rational medicine have been gradually tending,—viz., that not only is the examination of the body after death necessary for diagnosis and pathology, but that it is essential, in order that we may properly appreciate therapeutics, and the utility of different plans of treatment. Let us suppose, for instance, that this man had died at the commencement of 1852 from the attack of pleurisy, and that, as so often happens, we had been refused permission to open the body, my conviction is, that under such circumstances this case would have been recorded in the annals of medicine as a successful instance of cure by the method of Valsalva. But now, when all the facts are before us, it is evident that the diminution of the abdominal swelling was owing to the increase of the thoracic one; and that, as the force of the current of blood became lessened by the enlargement of the aneurismal dilatation above, so the flow of blood was retarded in the tumour below. In consequence, the concentric depositions of fibrin, the lessened size of the abdominal swelling, and the more permanent relief of pain, instead of being attributable to the treatment, as we had supposed, must now be more rationally ascribed to the increase of a thoracic aneurism, not detected during life, which had produced these results mechanically, and altogether independently of art.

The treatment of internal aneurisms by the method of Valsalva, has for some time been discouraged in this country, on the ground that it gives rise to a general irritability, and to symptoms of a distressing nature, which are often intolerable; whilst, on the other hand, it is seldom attended by a permanently good effect. In the case before us, as well as in that I formerly treated, no unpleasant symptoms could fairly be ascribed to the practice; but, on the contrary, it produced (especially the bleedings) well-marked relief. The question of the permanency of these good effects is, I admit, in no way supported by my experience.

## Part Fourth.

### PERISCOPE.

#### MEDICINE.

ON THE PHENOMENA WHICH INDICATE ADHESIONS BETWEEN THE HEART AND PERICARDIUM. BY DR JOSEF SKODA.

(Translated from *Zietschrift der k. k. Gesellschaft der Aerzte zu Wien*. April 1852.)

While submitting to the medical public my observations on the phenomena which enable us to diagnose adhesions between the heart and pericardium in the living body, I consider it necessary to prefix a short account of what others have written on the same subject.

Heim<sup>1</sup> pretended to distinguish adhesion of the heart to the pericardium by a depression observed during each systole to the left of the scrobiculus cordis, and immediately beneath the false ribs.

Dr Sander, a regimental surgeon in the duchy of Baden, described an analogous phenomenon—namely, an undulating movement on the left of the scrobiculus cordis, as the sign of pericardial adhesion.<sup>2</sup>

Laennec, Bouillaud, and Piorry, have seen neither the depression alluded to by Heim, nor the undulation described by Sander.

A depression in the scrobiculus cordis, or somewhat to the left of it, during each systole of the heart, and an elevation in the same region during each diastole, are always perceptible when the heart moves unusually to the left during the systole. In cases of insufficiency of the aortic valves, with horizontal position of the heart, the systolic depression and diastolic elevation in, or to the left of, the scrobiculus cordis, are but seldom absent. When, on the other hand, the heart lies vertically, a strong systole induces an elevation of the scrobiculus cordis, which disappears again during the diastole. It will be afterwards seen that it is only when the heart lies vertically that adhesion to the pericardium causes depression of the scrobiculus cordis during the systole; and that when the position of the heart is horizontal or normal, depression in, or to the left of, the scrobiculus cordis, occurring during the systole and accompanied with simultaneous protrusion of a left intercostal space, must be regarded as a proof that adhesion of the heart to the pericardium does *not* exist.

According to Hope, pericardial adhesion is indicated,—1. By a sudden jolting motion of the heart, easily detected by means of the stethoscope, and especially frequent when hypertrophy and dilatation co-exist; in which cases the jolting usually corresponds with the systole and diastole of the ventricles. 2. By a blowing murmur accompanying the first heart sound—especially when the organ acts strongly—and audible likewise in the aorta. 3. By the circumstance that the heart, notwithstanding its enlargement, beats as high as in the normal condition, and sometimes causes a protrusion of the left costal cartilages.

As for the sudden jolting movement of the heart, it must be remarked, that although it certainly may be observed when the heart adheres to the pericar-

<sup>1</sup> Kreysig, Thl. II., Abth. 2, s. 624.

<sup>2</sup> Hufeland's Journal der Heilkunde, Bd. 51, Jahrg. 120, Nov. Heft, s. 19-46 : Sander's Abhandlung über eine beobachtete Herzkrankheit — Verwachsung des Herzens mit dem Herzbeutel.

dium, it has no necessary connection with this lesion. The same may be said of the blowing murmur, and of the elevation of the left cartilages in the precordial region. As for the position where the heart's impulse is observed, we shall presently have occasion to judge of the incorrectness of Hope's assertion.

According to Charles Williams,<sup>1</sup> in cases in which intimate adhesions exist between the heart and pericardium, and likewise between the pericardial and costal pleuræ, the heart's movements may be felt far more distinctly and extensively, and, as the intercostal spaces are drawn inwards during each systole, they become also visible. In such circumstances, these movements always take place close to the wall of the chest, instead of being communicated, as in the normal condition, to the lungs, which at each inspiration are fully distended with air; for the wall of the chest carries them along with it, when heart and pericardium adhere; but when no such adhesion exists, it moves during inspiration upwards and away from the heart. The fixing of the heart to the wall of the chest causes a dullness on percussion alike during inspiration and expiration, and in all positions of the body, over a space corresponding to the extent of the adhesions and size of the heart, and in which the pulsations of the organ may be felt.

Here we may observe, that the extent of the space where the heart's action may be felt and seen, depends far more on other circumstances—on the size of the heart, on its position, the extent of its movements, the thickness of the walls of the chest, and the width of the intercostal spaces—than on adhesion between the heart and pericardium; and as for the depression of the intercostal spaces, it must be recollected that it may be observed in the third or fourth left intercostal space, and at every beat of the heart, in most individuals the parietes of whose chests are thin; and when the heart is enlarged, or acts powerfully, in the third, fourth, and fifth spaces. Further, even when the heart adheres to the pericardium, the movements of the heart are not of the same strength during both inspiration and expiration, and according to various circumstances they become alternately stronger or weaker during one or other of these acts. Finally, in cases of copious effusion into the sac of the pericardium, of adhesions of the costal and pulmonary pleuræ about the pericardium, of sacculated pleuritic exudation in the same situation, and of tumours of the mediastinum reaching to the wall of the chest, the percussion-sound during inspiration and expiration, and in every position of the patient, remains as uniformly dull as when the heart and pericardium are united,—so that, although from the alteration of the percussion-sound during inspiration and expiration the freedom of the heart from adhesions may be inferred, the non-alteration cannot be regarded as decisive evidence of its attachment to the pericardium.

According to Aran,<sup>2</sup> in cases of adhesion of the heart and pericardium, the second heart-sound loses in clearness, duration, and extent, and the more so the more firm the adhesion and wide the heart's cavities; when the lesion is of old standing, the sound may even be inaudible over the whole cardiac region. As, according to Aran, the aspiration (suction) of the ventricles contributes to the production of the second sound, and as the full distension of the ventricles becomes impossible in cases of pericardial adhesion, the aspiration being enfeebled or abolished, he supposes that the second sound must in such cases become weak, or absolutely inaudible. It is hardly necessary to remark, that the second heart-sound does not originate in the way Aran supposes, and that it does not disappear as a consequence of adhesion of the heart and pericardium.

Francis Sibson<sup>3</sup> describes several cases of adhesion of the heart and pericardium apparently with great exactness.

From his observations, it would appear that, in general, during the systole the

<sup>1</sup> German translation, by Dr Fr. J. Berend. 1841.

<sup>2</sup> Arch. Gén. de Méd., Avril 1843.

<sup>3</sup> On the changes induced in the situation and structure of the internal organs, under varying circumstances of health and disease, and on the nature and external indications of these changes. Worcester, 1844.



wall of the chest became retracted; but in some instances the heart's apex was strongly urged forwards during the systole. In one case the sternum and costal cartilages on both sides covering the right ventricle were during the systole drawn inwards, and on the completion of the systole sprang immediately forwards with a shock; in two cases, on the other hand, the lower half of the sternum was at the commencement of the systole urged forwards, and during the rest of the systole was retracted. This last observation was made when the patient's pulse was ranging in frequency between 140 and 180.

As I hold the opinion that, when the pulse is so frequent, the distinction of systole and diastole is very difficult, even when the impulse and sounds of the heart are normal, and that when the beat is disordered and murmurs are super-added, it becomes in general impossible; the appreciation of two divisions (moments) of the systole—its commencement and the rest of its duration—appears to me absolutely impossible. Hence I consider myself justified in assuming, that Sibson, in his observations, has often confounded the systole with the diastole; and that his assertions regarding the beat of the heart in cases of pericardial adhesion are consequently incorrect. The small variation of the percussion during inspiration and expiration is also prominently mentioned by Sibson; in some of his cases, the normal second heart-sound was distinct; in one instance only was it absent.

Bouillaud<sup>1</sup> has in six or seven individuals diagnosed a firm and universal adhesion of the pericardium to the heart by the following signs:—1. A distinct depression of the precordial region, analogous to that observed on the side of the chest when pleuritic effusion has terminated by adhesion. 2. The hand and ear applied to the precordial region enable one to perceive that the heart does not move so freely, is not so unconstrainedly active, as when the pericardium is in a healthy condition; its beats are in some measure impeded, and the apex does not give the usual punctuate and well-developed stroke against the wall of the chest. Bouillaud attaches especial importance to the first of these signs.

Depression of the precordial region may, however, be caused by old pleurisy, or may exist independently of any disease of the respiratory organs; and, on the other hand, when the heart and pericardium adhere, the arching of the precordial region may be normal or even exaggerated. As for the second point, were Bouillaud's observation correct, we should be obliged to admit the existence of adherent pericardium in very many men whose hearts are acting quietly; and in the most of Sibson's cases in which adhesion actually existed, according to rule there should have been no adhesion at all.

In passing to the results of my own observations, I cannot dispense with the relation of some cases, without exposing myself to the charge of constructing the signs of pericardial adhesion rather out of theoretical considerations than from the results of observation. I will, however, only adduce three cases, neglecting at the same time all particulars which do not bear strongly on the question at issue.

CASE I.—Thomas Hrubec, seventeen years of age, watchmaker's apprentice, received into No. 102,<sup>2</sup> on the 8th April 1845.

A flattened, painless, firm swelling, in the second left intercostal space near the sternum; percussion-sound, both during inspiration and expiration, dull from the second left intercostal space to the ensiform cartilage, and from the middle of the sternum outwards to the right nipple; the tumour in and about the second intercostal space is raised at each systole, and sinks back with each diastole, of the heart; the third, fourth, and fifth (left) intercostal spaces sink considerably inwards at each systole, and fill out again during the diastole; the apex cannot be distinguished; the heart-sounds are normal; the second sound of the pul-

<sup>1</sup> *Traité de Nosographie Médicale*. I. 1846.

<sup>2</sup> These numbers seem to refer to the wards in the Allgemeine Krankenhaus, or General Hospital of Vienna.—[*Editor*.]



monary artery<sup>1</sup> is double (*gespalten, split*); pulse at wrist normal, and synchronous with systole; slight palpitation.

*Diagnosis.*—The heart is universally adherent to the pericardium. Being thus fixed to the sternum, it cannot, during the systole, make its normal movement to the left. On the contrary, the part of the heart to the left of the sternum is at every systole drawn towards that bone; and, as the pericardial adheres to the costal pleura, the heart drags upon the left intercostal spaces, causing them to sink inwards during each systole.

The systolic elevation of the second left intercostal space is caused by dilatation of the pulmonary artery, or by its being surrounded with thick masses of exudation; or it may be caused by dilatation of the conus arteriosus of the right ventricle, or by exudation-masses surrounding it. The conus arteriosus, in order to communicate the systolic shock, must be *paralysed*.<sup>2</sup> Probability points in this case to dilatation and paralysis of the conus arteriosus, for the second sound of the pulmonary artery is heard loudest at the upper margin of the swelling. The contents of the tumour in the second intercostal space cannot be determined.

*Sectio, 28th May 1845.*—A cheesy mass, situated under the left pectoralis minor muscle, in the second intercostal space, but shut off from the cavity of the chest, by the intact pleura. The pericardium connected by band-like cords to the left wall of the chest, and everywhere adherent to the heart by dense cellular tissue. Heart normally situated; right ventricle enlarged; right auricle converted into a stiff, friable, tuberculated mass, which spread for an inch and a half towards the second left intercostal space into the substance of the dilated conus arteriosus, of the muscular structure of which only a few pale bundles remained, so that the thickness of the heart's wall was here reduced to a stratum of about one line in depth. A tubercular mass, in the form of nodules, surrounded the great vessels in such a manner, that the aorta and the descending cava seemed to be stuck through a ring. The tunics of the great vessels, and all the valves, were healthy.

**CASE II.**—Wenzel Pruscha, 16 years old, a joiner's boy, was admitted to No. 102 on the 20th September 1845.

The patient was of sturdy frame, had, till his present illness, enjoyed good health, and for the last fourteen days had suffered from stitches in the breast—for four days past, from dyspnoea. Examination detected a considerable pericardial effusion, and inflammatory infiltration of the lower lobe of the right lung. The grazing-murmur of the pericardium very loud all about the heart.

*30th September.*—The pericardial exudation had diminished by a third; friction-sound still distinct over whole heart.

*4th October.*—Pericardial friction-sound weak, and only to be heard at the heart's apex, which is recognised by a slight systolic heaving in the fifth intercostal space.

*8th October.*—No friction to be detected; the systolic heaving of the intercostal space hardly perceptible.

*16th October.*—Return of fever; no new symptom referable to heart.

*26th October.*—With each systole, there were formed, in the third, fourth, and fifth intercostal spaces, depressions, which disappeared again during the diastole; the apex could not be felt. In the second intercostal space, the pulmonary artery communicated a systolic shock; the sounds of the heart and arteries were normal.

On *24th November*, Pruscha left the hospital apparently well, only on climbing a stair he became breathless. Besides the systolic retraction of the third, fourth,

<sup>1</sup> It is necessary to explain that Skoda supposes the cavities of the heart, the aorta, pulmonary artery, and the valves, all to contribute to the production of the normal heart-sounds. When, therefore, in the course of this article, the reader meets with such expressions as "sound of the left ventricle," "of the pulmonary artery," etc., he will understand that the *sounds heard in the immediate vicinity of these parts* are alluded to.—[*Editor.*]

<sup>2</sup> i.e., passive during the systole.—[*Editor.*]

and fifth intercostal spaces, there was, up to this time, observed a systolic depression of the lower half of the sternum. After the systole, the breast-bone sprang forwards; and there was felt over the lower half of the sternum a shock accompanying each diastole.

*Diagnosis.*—Total adhesion of the heart to the pericardium, and of the outer surface of the latter to the neighbouring parts. By the fixing of the heart to the sternum, the systolic retraction of the third, fourth, and fifth intercostal spaces is accounted for. The systolic retraction of the lower sternum is caused by the adhesion of the heart to the vertebral column. After the systole, the breast-bone springs forward by its own elasticity.

On 13th February 1846, Pruscha was re-admitted to No. 102, with pneumonia of the left side. The phenomena referable to the heart were the same as were reported on the 24th November, and remained unaltered up to the time of the patient's death.

*Sectio, 8th April 1846.*—Heart situated somewhat towards the median line; right ventricle enlarged; valves all sound; firm adhesion of heart to pericardium; the outer surface of the latter firmly attached by a tubercular degenerating exudation, very thick posteriorly, to the anterior border of the left lung (which was infiltrated with tubercle), to the walls of the ribs, and to the vertebral column.

*CASE III.*—Josef Eder, 44 years old, a labourer, admitted to No. 84, on May 13th, 1851, with insufficiency and contraction at the mitral orifice, bronchial catarrh, ascites, and œdema of the feet. The unusual dulness, corresponding to the dilatation of the right heart, remains the same during inspiration and expiration. In the fifth left intercostal space a considerable retraction accompanies each systole; after the systole the retraction immediately subsides, so that a shock coinciding with the commencement of the diastole is at this point to be felt.

*Diagnosis.*—Adhesion of the heart to the pericardium, and of the left pleural surface of the latter to the wall of the chest. The heart being fixed to the sternum, the apex must move towards the sternum during the systole, and drag the intercostal space inwards. On the cessation of the systole, the elasticity of the intercostal space causes it to resume its normal form, and gives rise to the diastolic shock.

The œdema of the feet and the ascites having diminished, the patient feeling himself better, left the hospital. In July he returned, and was placed in another ward, where I had no opportunity of examining him.

*Sectio, 17th September 1851.*—A firm union of the heart to the pericardium, and total adhesion of the costal and pulmonic pleura; insufficiency and advanced stenosis of the bicuspid valve; considerable hypertrophy and dilatation of the right heart.

*Remarks.*—From these three cases of adhesion of the heart to the pericardium, it may be seen that the diagnosis was always based upon phenomena from which it might be not only concluded, that the heart, during its systole, was impeded from moving downwards and to the left, but likewise that its apex was drawn to the right and upwards. I will now illustrate these phenomena more closely, and, for the sake of perspicuity, will again allude to some of the signs of adherent pericardium, which have already been shown not to be characteristic. *The heart's apex gives no systolic beat; it is either not to be felt, or appears to cause a shock during the diastole.*<sup>1</sup>

*During the systole, depressions become visible in the intercostal space corresponding to the apex, and frequently in one or more spaces above it, when, in addition to adhesion of the heart and pericardium, there is likewise adhesion of the pericardium to the costal pleura.* Without this last condition, retraction of

<sup>1</sup> An exception to this rule may be imagined,—viz., when the heart's apex is paralysed. I have sometimes suspected this paralysis of the apex, but have never observed it in connection with adhesion between the heart and pericardium.

the left intercostal spaces does not take place; and in such cases we must endeavour to appreciate the systolic retraction of the apex by the sense of touch. The systolic retraction of the left intercostal spaces to which Charles Williams attaches especial importance, and to which Sibson also refers, does not of itself, as we have shown, enable us to diagnose adherent pericardium. The examination must likewise show that, simultaneously with the retraction of one or more intercostal spaces, the heart's apex is nowhere urged against the parietes of the chest. *The systolic retraction of the lower half of the sternum is a certain sign of adhesion of the heart and pericardium, and of their fixtude to the vertebral column.*

Retraction of the scrobiculus cordis, or of the parts to the left of it, I have never seen in connection with pericardial adhesion. It appears that the fixing of the anterior surface of the heart to the sternum prevents any unusual dragging of the diaphragm. However, when the diaphragm lies low, and the heart consequently in an unusually vertical position, a systolic elevation of the diaphragm, and corresponding retraction of the epigastrium, might be likely to occur. Hitherto I have observed no such case.

A systolic beat at the base, or above the base of the heart, has no connection with adhesion of the heart and pericardium, although frequently observed along with it. The diagnosis may be founded on *the immobility of the limits of dull percussion in the cardiac region during inspiration and expiration*—the percussion must not, however, be forcible—*when it is possible, from the ensemble of symptoms, to exclude the existence of all other abnormal conditions which can cause the same uniformity of the percussion-sound.*

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## Part Fifth.

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### MEDICAL NEWS.

#### EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING VII.—*Wednesday, 21st April 1852.*—Dr BEGBIE, President, in the Chair.

##### COMPLICATIONS OF SCARLATINA.

*Dr Alex. Wood* read a communication entitled, "Notes on some of the more unusual complications of scarlatina, and more especially on the relations between scarlatina and erysipelas." The paper began by referring to the variety of character exhibited by this fever even in the same localities and at the same seasons—and to the frequency with which dropsy followed the mildest forms, of which illustrative examples were given. The author narrated several cases of the *scarlatina sine exanthemate*; showed the connection subsisting between them and the severe attacks of angina, to which attendants on scarlet fever cases who have themselves had the disease, are exposed. He considered the diphtheritic affections of the throat occurring in the disease, and narrated cases where the throat, and from it the conjunctiva, meatus auditorius, nerves, and trachea, were consecutively affected. He showed the occurrence of a similar affection in erysipelas, as also the tendency to affections of the brain and kidney in both diseases. He likewise referred to the diffuse inflammation of the cellular tissue of the neck, as particularly illustrating this connection. Of this sequela several cases were given, and Dr W. attempted also still farther to connect it with the disease of the pudendum described by Mr Kinder Wood, demonstrating the connection between both of these affections and albuminous urine.

The *President* remarked that *sore throat* was a very constant attendant on erysipelas of the face, and that in general it would be found that the inflammation of the fauces spread continuously from thence to one or other of the external outlets, as the angle of the eyelid or mouth, the opening of the nostril or meatus auditorius, and afterwards to the face and head; but the sore throat, though often troublesome and even dangerous, he did not regard as corresponding in character with that of scarlatina, and certainly he had never observed in the erysipelatous sore throat the pellicular exudation called *diphtherite* which was common, and often fatal, as an accompaniment of scarlatina.

*Dr Sellar* said that the suppuration of the cellular tissue described by *Dr Wood* as occurring after scarlatina, was quite a different affection from erysipelas; indeed the skin proper was not involved in these cases in the inflammatory action. He agreed with *Dr W.* in thinking that suppuration in the neck after scarlatina was a very formidable, and indeed generally fatal affection; in fact, it seldom attacked any but unhealthy children, or those living under very bad sanitary conditions. He had seen a great number of cases of this complication in an epidemic of scarlatina some years ago, chiefly in the Cowgate of Edinburgh; and there was in these cases no true erysipelas. He had always regarded the prevalence of these complications as determined in a great degree by the peculiarities of epidemic constitution.

*Dr Andrew Wood* remarked, that he agreed with *Dr Sellar* as to the fact of the more frequent occurrence of severe cases of scarlatina characterised by diffuse cellular inflammation of the neck and suppuration of the cervical glands in particular epidemics; and he believed that unhealthy children were most apt so to suffer. Even in mild epidemics, cases of this kind were frequent amongst the children of the poor, whose powers of resistance to morbid action were small, in consequence of the influence of various depressing causes, as insufficient food and clothing, want of cleanliness, deficient ventilation, etc.; but children of those in comfortable circumstances were by no means exempt, especially in families of naturally delicate constitution, or in individuals of families whose constitutions had been weakened by previous ill-health. *Dr W.* considered it of great importance to apply judicious and efficient treatment at as early a stage as possible, not only in the severe forms of scarlatina, but also in those mild attacks which might appear to require little or no remedial aid; because, as every one who had had much experience in this disease must have observed, it was impossible to predicate that dangerous symptoms might not develop themselves, either during the course of the primary fever or subsequently at the period when that formidable sequela—dropsy—was most wont to supervene. In this way the occurrence of dangerous complications might often be altogether prevented, or, at least, be greatly modified in severity. An extensive experience in the treatment of scarlatina, had led him to the conclusion, that the most efficient, as well as the most safe, method of cure was by acting powerfully on the skin, and thereby assisting nature to eliminate the poison of scarlatina from the system. It was well known that the ordinary medicinal diaphoretics frequently failed in scarlatina. The warm-bath, when properly used, was often a most valuable remedy; but sometimes, especially amongst the poor, this was, for obvious reasons, not easily made available, and even when available it occasionally disagreed, causing sickness, giddiness, rigors, and other unpleasant symptoms. *Dr W.* had in consequence been led to have recourse to a diaphoretic plan of treatment, which was easily available under all circumstances, whether in the hovels of the poor or the palaces of the rich, and from the use of which, in a large number of cases, he had obtained the most satisfactory results both in relieving present symptoms and in averting future mischief. The remedy to which he referred, was what might be deemed *the modified vapour-bath*, for the suggestion of which the profession was indebted to the late distinguished *Dr John Thomson* of this city, and which deserved to be more extensively known than it was as an important adjuvant in the treatment not only of scarlatina but of many other febrile diseases. The following was the mode of using this remedy:—You take several

long worsted stockings, or long narrow flannel bags; after having wrung them out of water as hot as can be borne, you then fill them with common beer bottles containing very hot water. These are to be laid alongside the patient, but not in contact with the skin,—one on each side, and one between the legs, will generally be sufficient; but more may be used if deemed necessary. The patient is to lie between the blankets (the head, of course, being outside) during the application of the bottles, and for several hours after. In the course of from ten minutes to half an hour, the patient is thrown into a most profuse perspiration—the stockings may now be removed. In mild cases, the effect is easily kept up by means of draughts of cold water, and, if necessary, by the use of two-drachm doses of *Sp. Mindereri* every two hours. In severe cases, where the pulse is very rapid, the beats running into each other—where the eruption is either absent or only partial, or of a dusky purplish hue—where the surface is cold—where there is sickness or tendency to diarrhoea—where the throat is aphthous or ulcerated, and the cervical glands swollen, then it is expedient to follow up the vapour-bath by four or five grain doses of carbonate of ammonia, repeated every three or four hours. Should this be vomited, then brandy may be given in doses proportioned to the age of the patient. In general, after several doses of the brandy, the stomach becomes tolerant of the ammonia, which may then be resumed, and continued till the course of the symptoms indicates its discontinuance. The carbonate of ammonia, the use of which in scarlet fever was long ago warmly advocated by Mr Peart, he had found a valuable remedy; it seemed to act beneficially in several ways,—1st, by supporting the powers of life; 2d, by assisting the development of the eruption; and, 3d, by acting on the skin and kidneys. In the treatment of the sore throat, he trusted chiefly to poultices applied externally, and frequent sponging of the pharynx with a gargle of dilute nitric acid, of the strength of  $\mathfrak{z}\text{ij}$ . to  $\mathfrak{z}\text{ijj}$ . of simple syrup. Where the vapour-bath was used early in the disease, and its use continued daily, or even twice or thrice a-day, according to circumstances, he had found that the chance of severe sore throat was greatly obviated; and in regard to the supervening dropsy, he believed that by the use of the vapour-bath, with the other necessary precautions as to exposure, diet, etc., its occurrence would be rendered much more rare: at least, such has been the case in his own practice. In the treatment of the dropsical cases, it was also very useful, and even might be trusted to entirely in some instances. Dr W. condemned the use of purgatives in scarlatina during the first ten days, as not only not required, but positively dangerous, as tending to interfere with the development of the eruption. In the later stages, as well as in the dropsy, they were often of great service.

*Dr Seller* remarked that Dr Wood's stimulant plan of treatment, however applicable to the treatment of scarlatina in the present day, could not have been followed out by any judicious practitioner in the epidemics which he had witnessed about twenty-five years ago, and in which, according to the testimony of all authors, an antiphlogistic treatment was most successful in obviating the dropsy and other complications. He was satisfied, from personal observations, that the disease had undergone a change of type, which was apt to be too little attended to by those who observed it at the present day.

*Dr Christison* begged to corroborate Dr Seller's statement as to the change of type which had taken place in scarlet fever in this city. Between 1817 and 1827, in this, as in all other febrile diseases, and also in the acute local inflammations, the sthenic or inflammatory character was much more marked than it has been since; and the treatment by depletion, and other reducing means, was then practised with propriety and success. No man would dream of using blood-letting and purging in fever and scarlatina now, as they were used with success, at all events with perfect safety, in the earlier period. Differences in "epidemic constitution" were far too little regarded by many writers in the present day.

*Dr MacLagan* and *Dr Gairdner* supported Dr Christison's statement; and in



regard to scarlatina, Dr G. alluded to Dr Abercrombie's introduction into this city of the antiphlogistic practice in the dropsy of scarlatina, and bore testimony to its remarkable success.

*Dr Andrew Wood* said he had no doubt of the success of antiphlogistic practice in the scarlatina dropsy. In conjunction with the vapour-bath he did not hesitate to employ it in suitable cases, but considered that the important object of treatment was to prevent dropsy and formidable local complications, and that formerly attention had been too exclusively directed to these complications, and too little to the original blood disease, and to the existence of a poison in the system which was eliminated by the skin.

*Dr Christison* repudiated the idea that the general disorder had ever, at any period, been overlooked in the treatment of scarlatina. The depleting treatment by blood-letting and purging, practised between 1817 and 1827, was not directed to local affections, but to the state of general re-action, the constitutional disease.

*Dr J. W. Begbie*, reverting to the subject of the relation between scarlatina and erysipelas, remarked that another point of resemblance between the two diseases was the occurrence of coagulable urine in a considerable proportion of cases of both affections. He proposed to read a paper connected with this subject at next meeting of the Society.

*Dr W. T. Gairdner* said that he believed many of the complications described by Dr Alex. Wood as common to scarlatina and erysipelas would be found prevailing at particular seasons in other epidemic diseases. A marked instance was the epidemic fever of 1847-8, in which a throat affection very similar to what occurred in erysipelas was exceedingly common, and led to the more frequent performance of tracheotomy in a few months than had been necessary for several years before or since that period. Many of these cases were abdominal or enteric typhus, but the throat affection occurred in some instances in the ordinary typhus, and also, he believed, in the relapsing fever to a less extent. He had likewise seen, at the same period, a few instances of formidable suppurations of the parotid and cervical glands during the convalescence from typhus.

After a few words from *Dr Glover*,

*Dr Alex. Wood* said that it had not been his intention to dictate any exclusive treatment. He had merely wished to show that possibly the erysipelatous character of some cases of scarlatina might account for the differences of treatment required.

#### TREATMENT OF CONTINUED FEVER BY QUININE.

*Dr Bennett* read a paper on "The Treatment of Continued Fever by large doses of Quinine." (See Clinical Lectures, Med. Jour. for April 1852, p. 355, and for June 1852, p. 364.)

*Dr Christison* said that, on lately taking his turn of duty as clinical professor, although he understood from Dr Bennett that Dr Dundas's treatment had been tried by him in several cases without success, he resolved to make trial of it also in conformity with a promise made to Dr Dundas. The first suitable case for the experiment was that of a girl of fourteen, who, while a patient in the clinical ward, had caught fever apparently from having been allowed by the nurse to be in constant close communication with another girl convalescing from simple spotted typhus. An eruption, persistent on pressure, appeared on the fifth day; and the other symptoms of simple typhus were characteristically exhibited. On that day five eight-grain doses of sulphate of quina were given every two hours; and next day these doses were repeated. The only discoverable effect was some increase of a habitual deafness. No amelioration whatever ensued, and no reduction of the pulse. Twenty-four hours after the quina was abandoned a temporary amendment took place, as is not unfrequently observed after the close of the first week. But at the same time diarrhoea set in; the eruption disappeared on pressure; the other symptoms also put on the character of enteric typhus; and at



the present moment the patient is moribund,—in the fifth week of fever. [She died in two days more. The *fæces* had become solid for three days before death. The intestines did not present any appearance of *dothi-enteritis*.]

This case was no great encouragement to persevere. He was at a loss to account for the success of Dr Dundas and others, and the total failure of the remedy here. Dr Bennett had mentioned one source of fallacy. He might mention another. In the spring and autumn of last year, most of the cases of fever in the clinical wards put on the characters of simple typhus, sometimes in great intensity, and terminated by a gentle diaphoretic crisis, beginning on the eleventh, and ending on the fourteenth day. One of the worst cases he ever saw put on that form, and so ended favourably in the most unpromising circumstances. An epidemic, in which such cases occurred, would be a great source of fallacy in the course of trials like those made by Dr Dundas and others.

Dr Robertson had, since the 20th of January, been making a series of observations on the quinine treatment. He had treated in a male fever ward of the Infirmary thirty-four cases of fever, whereof twenty-one were unmistakeable examples of typhus. To eight of the twenty-one, quinine in ten-grain doses was given, in exact accordance with Dr Dundas's directions. The treatment was commenced at all periods, from the fifth or sixth, to the tenth day of the disease. The quantity used in each case varied from 40 to 130 grains. It produced no appreciable good effect,—did not cut short the fever in a single case,—and, while it generally seemed to lower the pulse, sometimes caused cerebral excitement, followed by coma of a very alarming character. Dr Robertson was convinced that in cases of the Edinburgh typhus at least, the practice was useless, if not dangerous. True, the eight cases all recovered; but the epidemic at present was a mild one,—for of thirty-four cases treated in the same ward during the last three months, one only had proved fatal.

Dr Maclagan read an extract of a letter from his son, Dr Philip Maclagan, surgeon of the 20th Regiment at Montreal, of date 13th December last, to the following effect:—"We have a good deal of common fever. I have been trying to cure some of them by inducing *cinchonism*, certainly with very marked success, but the number is too small as yet to give any opinion on the system. But in cases apparently *parallel*, and lying side by side, the treatment by large repeated doses of quinine produced a rapid change, while under our ordinary *expectant* treatment the cases ran on in the common way."

The *President* remarked, that he had not had any opportunity of testing the value of quinine as a febrifuge in typhus; but he had on many occasions witnessed its beneficial effects in the ordinary remittent fever of children. In such cases he had administered quinine early in the disease, when the tongue was loaded, the skin hot, and the pulse frequent; and had observed that, in such circumstances, without the aid of purgatives, or other antiphlogistics, cases which would have run the course of *ten* or more days, had been cut short in *four* or *five*.

Dr Christison and Dr Maclagan also attested the utility of quinine in this disease; but Dr Maclagan always employed purgatives in addition.

#### REGISTRATION OF DEATHS.

A second report was read from the committee on registration of deaths, to the effect that, though the scheme of the committee had been approved by a majority of the Society, in terms of Dr Seller's motion at last meeting, they recommended the Society not to proceed at present in carrying out the scheme.

Dr Seller moved, and Dr Maclagan seconded, the adoption of the second report, which was carried unanimously.

MEETING VIII.—*Wednesday, May 5, 1852.*—Dr BEGBIE, P., in the Chair.

#### DISEASED PROSTATE.

*Dr W. T. Gairdner* exhibited a specimen of enlarged prostate gland. The urethra was very tortuous at its prostatic portion, and a false passage existed at the junction of this with the membranous part of the urethra. A good deal of pus could be squeezed from the orifices of the prostatic ducts, and there seemed reason to suppose that some of the veins about the urethra had been injured, as the patient, a healthy old man, died with low febrile irritation, and commencing multiple abscesses were found in the lungs. The preparation was further interesting, as exhibiting an anatomical peculiarity which had been pointed out to Dr Gairdner by Mr Bickersteth, and had been found by that gentleman in three or four cases. The recto-vesical fold of peritoneum descended so low as to cover the posterior surface of the prostate, and it was therefore obvious that the bladder could not, in this instance, have been tapped from the rectum without wounding the peritoneum in two places.

#### ADHERENT PERICARDIUM.

*Dr W. T. Gairdner* likewise showed a specimen of complete and close adhesion of the pericardium, of old standing, from the body of a patient who had died of chronic disease; in order to illustrate the position maintained in Dr Gairdner's memoirs on adherent pericardium, that under such circumstances the heart frequently remains of the normal size, and its functions in all probability continue to be, in some instances at least, performed without material disturbance.

#### TEMPORARY ALBUMINURIA.

*Dr Jas. W. Begbie* read a paper on "Temporary Albuminuria, more particularly as occurring in the course of certain febrile or other acute diseases." The author defined *temporary* albuminuria as the manifestation and continuance of albumen in the urine during a limited period, and without any serious organic change in the kidney. He recognised three varieties of temporary albuminuria, each of which formed appropriate illustrations in some well-known conditions of acute disease. Thus *desquamative albuminuria*, the first variety, was found in simple scarlatina, in erysipelas, and in Asiatic cholera; *inflammatory albuminuria*, the second type, was illustrated by the condition of the urine in the dropsy following scarlatina; while, according to Dr B., a third kind, a *critical albuminuria*, occurred in pneumonia and in certain cases of typhus fever. In desquamative albuminuria the occurrence of albumen was associated with the separation of epithelium from the mucous surface of the kidney and bladder; and in scarlatina this was likewise connected with a desquamative change in the skin, as it was in cholera, with a similar condition of the mucous coat of the intestines. Desquamative albuminuria was likewise found in some other febrile diseases, but these were the most frequent and familiar examples. The characters of the urine under the microscope (which Dr Begbie described in detail) presented certain minor differences in different cases; but there were characters common to all cases of this affection, distinguishing it as a special form of albuminuria. Inflammatory albuminuria was characterised by the presence of renal tube-casts, often granular cells, and very generally blood-corpuscles, in greater or less abundance, showing a certain amount of renal congestion and disorganisation. This was, according to Dr B.'s experience, the invariable type of albuminuria in scarlatina attended with dropsy. It might, or might not, be temporary; and probably in some cases led to permanent renal disease, especially if neglected or unskilfully treated. In most cases, however, it might be regarded as a curable

disorder. Critical albuminuria occurred about the period of the resolution of certain fevers and acute inflammatory affections, particularly of the abdominal typhus, and acute pneumonia. It was distinguished, not only by the period of its occurrence, and the symptomatic changes with which it was connected, but by the collateral changes in the urine itself; viz., the occurrence of precipitated lithates, etc., as in the ordinary urine at the crisis of acute affections; and likewise by the absence of the large quantities of epithelium, and the other microscopic elements formerly alluded to. These were replaced, in the critical albuminuria, by a large number of very minute molecules and granules, which Dr B. had been led to regard as the debris of the exudation from the lung or other organs, chemically unchanged. The critical albuminuria, therefore, was to be regarded as the evidence of the change taking place in the primarily diseased organ during its restoration to a healthy state.

*Dr Christison* said that his own experience, although consisting of scattered observations, went far to corroborate some of the results arrived at by Dr Begbie. He had not found albuminuria quite so common in scarlatina, however, as Dr B. had indicated, and though this might be owing in part to imperfect observation, he was inclined to believe that there might be a difference in different epidemics of the disease. In Heriot's Hospital, where his cases had been observed on the greatest scale, in conjunction with Dr Andrew Wood, he thought it possible, also, that the inferior frequency of albuminuria might be in some degree owing to the strong derivation to the skin in the treatment practised by Dr Wood. Dr Christison advised Dr Begbie to watch attentively the after-history of those individuals having temporary albuminuria. He (Dr C.) was inclined to think, from what he had observed, that the recurrence of the affection might in many cases be looked for at intervals; and possibly it might be found that the persons so affected fall a prey in the end, in a considerable proportion of cases, to organic disease of the kidney. He had seen instances of this, both in the alleged temporary albuminuria from eating certain kinds of food, and also in that which followed the application of blisters.

*Dr Andrew Wood* laid before the Society at considerable length the particulars of his experience in Heriot's Hospital with reference to albuminuria in scarlatina. His details were generally corroborative of the statements of Dr Christison, showing that albuminuria occurred in scarlatina in about one-half the cases in which a daily examination of the urine was made. His experience differed from that of Dr Begbie in a few other points. He had in one case met with a relapse of albumen in the urine. He had likewise seen the tube-casts, etc., (the inflammatory albuminuria of Dr Begbie) in the simple scarlatina without dropsy; and in one case these occurred during steady improvement of the general symptoms. With regard to the influence of treatment alluded to by Dr Christison, Dr Wood had observed that among cases in which the vapour-bath had not been efficiently used (at the commencement of the epidemic) there had been seven cases of albuminuria; whereas in the succeeding eighteen cases, treated by the vapour-bath and otherwise according to his method (see last meeting of the Society) there were only five cases of albuminuria.

*Dr W. T. Gairdner* remarked that, from the period of his earliest observations in regard to the diseases of the kidney, he had been induced to regard albuminuria as a species of catarrhal affection of the renal tubuli, or of the external urinary passages. He had found this view confirmed by the evidence both of morbid anatomy and of clinical inquiry. He regarded Bright's disease of the kidney as the analogue of bronchitis or broncho-pneumonia in the lung, as he had stated at a meeting of the Medico-Chirurgical Society several years ago. Although concurring in many of the observations of Dr J. W. Begbie, he (Dr G.) could not regard the three species of albuminuria described by him as being quite established. He (Dr G.) was rather inclined to regard them as varieties, depending merely on different degrees of intensity of the affections, and perhaps on a few other circumstances not perfectly understood. He also thought with Dr Christison that the occurrence of albuminuria, whether temporary or per-

manent, was always to be regarded with suspicion. He could not bring himself to look upon this circumstance as a favourable pathological indication, and thought that if it did not indicate the actual occurrence of renal disorganisation, it ought generally to be accepted as evidence of some tendency to that condition.

*Dr J. W. Begbie* replied in a few words, and thanked the Society for its reception of his first communication.

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NOTE ON DR CHRISTISON'S PAPER ON THE DIET OF PRISONERS.

Dr Lonsdale, of Carlisle, informs us, that an error has been committed in the statement made in Dr Christison's paper, as to the diet of the prisoners at Carlisle; and that, in consequence, the nutriment has been made less than it is in reality. The error has arisen partly from the statement in the report of the Inspector of Prisons not having been sufficiently explicit as to the composition of the prison soup, but chiefly because the diet-table gives the allowance of milk according to the old measure called *gill*, which, in Carlisle, is larger than in Scotland, in the proportion of  $2\frac{1}{2}$  to 1, viz., 10 ounces instead of 4 only.

With the necessary corrections, the total daily nutriment in the table, at p. 424 of the May Number, representing the dietary of Carlisle prisoners, confined from 14 to 42 days, becomes 3.29 ounces of nitrogenous, 11.97 carboniferous, and 15.27 total nutriment. And that in the table, at p. 425, representing the nutriment of prisoners at hard labour between 42 and 90 days, or confined longer without hard labour, consists of 3.73 nitrogenous, 13.03 carboniferous, and 16.76 total nutriment. The former is still materially defective, though not so much so as appeared from the faulty data in the paper. The latter is closer to what appeared, from the investigations in the Scottish prisons, to be the necessary nutriment for prisoners not at hard labour, and confined for less than 60 days. But the conclusions drawn from the whole inquiry, lead to the result that this allowance is too scanty for prisoners at hard labour, or confined longer than 60 days.

The error pointed out by Dr Lonsdale, of course requires correction; but it does not affect essentially the fact, which it was the purpose of the paper to point out, viz., that the average loss of weight of the prisoners at Carlisle was referable to a defective alimentation.

The average daily allowances for the former class of prisoners, as calculated from Dr Lonsdale's data, are, oatmeal 8.43 ounces, skimmed milk 22.1, bread 6, potatoes 8.14, meat 1.3, vegetables 0.43. The averages for the latter are oatmeal 10.43 ounces, milk 22.1, bread 6, potatoes 6, meat 2.14, vegetables 0.43. With these numbers, it is easy to correct the erroneous data in the paper.

MR COULSON.

In order to show that Mr Syme was not original in proposing external incision upon a grooved director for the remedy of obstinate stricture, Mr Coulson (*Lancet*, 19th June) has quoted from Desault (not *Dessault*, as he always misspells the name) an account of the "Boutonnière," or "Buttonhole" operation, for the relief of retention of urine. Desault does not appear to have ever performed this operation, which he characterises as necessarily either useless or injurious. The object of it was to make an opening in the perineum, through which a tube might be introduced and allowed to remain in the bladder. For this purpose, says Desault, the incision may be made into the urethra, as in

cutting for the stone by the *apparatus major*—or prolonged to the neck and body of the bladder—or directed at once to the bladder. He then describes the procedure appropriate to each of these situations; and, with regard to the urethra, he thus expresses himself,—“On ne suit pas toujours le même procédé en pratiquant la boutonnière sur le canal de l'urètre. Lorsque l'on peut introduire un cathéter dans la vessie, on se sert de cet instrument pour faire sur sa cannelure l'incision du canal, et conduire un gorgeret *qui doit servir à faciliter l'introduction de la canule destinée à rester dans la vessie.*”<sup>1</sup> In quoting this passage, Mr Coulson has omitted the latter part, which is here printed in italics, actually breaking through a clause of the sentence, without even the pretext of a comma for doing so, and has thus completely concealed the object of the operation, which was to pass a tube from the perineum to draw off the water, and not to remedy a stricture, as he would have it seem. Desault nowhere mentions the operation of Boutonnière except as a mode of relieving retention of urine, and we are not a little surprised that Mr Coulson should either wish or attempt to deny this.

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On the Enlargement of Articular Extremities of Bone in Chronic Rheumatic Arthritis. By W. Adams, F.R.C.S., Assistant-Surgeon to the Royal Orthopædic Hospital, &c. (From Trans. of Pathological Society of London.) London: Bentley & Co. 1851.

Norsk Magazin for Lægevidenskaben. Six Parts for 1851 and 1852. Christiania: Feilberg & Landmark.

The Symptoms and Treatment of the Diseases of Pregnancy. By William John Anderson, F.R.C.S. London: Churchill. 1852.

The Spirometer, &c. By John Hutchison, M.D. London: Churchill. 1852.

Blennorrhagia and Syphilis. An Analysis of the Letters of Ricord. By M. H. Stapleton, Bachelor of Arts and Medicine, Trinity College, Dublin. Dublin: Hodges & Smith. 1852. (From Dublin Quarterly Journal of Medical Science.)

An Essay on Unhealthy Inflammation. By M. B. Gallwey, Surgeon, Royal Regiment of Artillery. London: Renshaw. 1852.

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Dictionary of Domestic Medicine and Household Surgery. By Spencer Thomson, M.D., &c. Part VI. London: Groombridge & Sons. 1852.

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Knox on Great Artists and Great Anatomists. London: Van Voorst. 1852.

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<sup>1</sup> Œuvres Chirurgicales de P. J. Desault. Par Xav. Bichat. Troisième Edition. Paris, 1818. Tome III. Pp. 330-331.

## EXCHANGE LIST.

- British and Foreign Medico-Chirurgical Review,—regular.  
 Chemical Gazette,—ditto.  
 Medical Times and Gazette,—ditto.  
 Pharmaceutical Journal,—ditto.  
 London Journal of Medicine,—ditto.  
 Psychological Journal,—ditto.  
 The Veterinarian,—ditto.  
 Provincial Medical Journal,—ditto.  
 Dublin Quarterly Journal of Science,—ditto.  
 Dublin Medical Press,—ditto.  
 American Journal of Medical Science,—ditto.  
 Boston Medical and Surgical Journal,—ditto.  
 Buffalo Medical Journal,—ditto.  
 New York Journal of Medicine,—irregular.  
 Philadelphia Medical Examiner,—ditto.  
 Annales Médico-Psychologiques,—regular.  
 L'Union Médicale,—ditto.  
 Journal de Médecine et de Chirurgie Pratique,—ditto.  
 Gazette Médicale de Paris,—ditto.  
 Gazette des Hôpitaux,—ditto.  
 Révue Médico-Chirurgicale,—ditto.  
 Bulletin Général de Thérapeutique,—ditto.  
 Vierteljahrschrift für die Practische Heilkunde,—ditto.  
 Zeitschrift der k. k. Gesellschaft zu Wien,—ditto.  
 Henké's Zeitschrift für die Staatsarzneikunde,—ditto.  
 Journal für Kinderkrankheiten von Behrend und Hildebrand,—ditto.  
 Zeitschrift für Rationelle Medicin, von Henle and Pfeufer,—Part III. for 1850  
 has not been received ; with the above exception, regular.  
 Verhandlungen der Physicalisch-Medicinischen Gesellschaft in Wurzburg,—  
 ditto.  
 Nederlandsch Weekblad voor Geneeskundigen,—ditto.  
 Bibliothek for Læger,—irregular.  
 Hygiea,—irregular, and sometimes sent by post.  
 Illustrierte Medicinische Zeitung. Exchange accepted. Only one number has  
 as yet been received.  
 Nederlandsch Lancet. No numbers have as yet reached us.

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We beg our foreign correspondents, and the Editors of foreign journals, to address their communications, for this Journal, to the care of Messrs Williams and Norgate, foreign booksellers, Henrietta Street, Covent Garden, London. No parcels sent by post can be received, unless the carriage be prepaid. Several American journals, which do not appear in our present Exchange List, have been omitted on account of repeated infraction of this invariable rule.



## Part First.

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### ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Practical Remarks on Insanity as it occurs among the Inhabitants of Bengal.* By THOMAS A. WISE, M.D., late Surgeon H.E.I.C. Service. —(Continued from Vol. XIV., p. 515.)

*Mortality.*—The mortality among the patients in the Dacca asylum has always been very great; but subject to very considerable changes, from the peculiarities of the season, etc. During one year that a predecessor of mine superintended the asylum, the deaths amounted among the males to 32 per cent.; and the cures, including cases relieved and made over to their friends, to 51 per cent. of the admissions. Among the females, during the same period, the per centage was 45½ cured, to 38½ per cent. deaths.

The year 1848 was peculiarly unhealthy, from the high inundation of the rivers, and from the unseasonable fall of rain at the commencement of the cold weather. During the whole year, the deaths amounted to 17½ per cent., or 15½ per cent. during the last six months of that year. The difference in the number of deaths in the asylum, and in the contiguous Jail Hospital was remarkable; for while, in the former, it sometimes amounted to 32 per cent., in the latter the mortality was only about 1½ per cent. The deaths among the insane were chiefly the result of that exhaustion, accompanied with diarrhoea, which, notwithstanding all the precautions used, carries off so many of the patients annually. The removal of the sick-ward in the insane asylum to a drier and better aired room, and the precautions used in defending the patients from cold, in clothing them, and allowing them warm and nourishing food, with regular work and exercise, explained the diminution of the mortality in the hospital. Indeed, during the two months after the new arrangements had been completed, or during the months of December and January, no deaths from the state of collapse occurred.

The cured amounted during the same year to 54½ per cent. of the admissions.

*Classification.*—In attempting to classify the insane, we meet with some cases of eccentricity of conduct, with little or no diminution of the powers of the mind, which sometimes pass into madness, and monomania, and may terminate in fatuity. Bearing these peculiarities and changes in mind, insanity will be arranged under certain groups of symptoms, which are generally correct, and are useful in practice. A few characteristic examples of such varieties may here be added.

1st. When the erroneous judgment relates to one object, or *monomania*, which will include nostalgia, or the pining for one's country, and is much less marked in the Mussulman than the Hindu. And among the hill tribes the same attachment to their homes and barren jungles, is shown as in Europe. Circumstances may oblige the mountaineer to leave his hills, and descend into the plains, to collect a certain sum of money by labour, but he always returns to his cherished home.

2d. Should the aberration of judgment affect more points than one, it forms the different varieties of *mania*. The following may be related as an example:—

*Sonatun*, an Assam hill chief, murdered one of his servants for some fancied cause, in a fit of insanity; probably produced by his inveterate use of opium, and other intoxicating drugs. He was very indignant that he should be tried for what he considered as "doing as he liked with his own." He was fifty years of age, and of a large and muscular frame. His body exhibited several cicatrices of wounds, which told the troubled life he had led; and his face was pock-marked. He spoke Bengalee imperfectly; but was always very voluble while in my presence, and always had a number of questions to ask about his dear country, and those gentlemen he had known.

When first brought to the asylum, I asked him what sort of work he was accustomed to. "Work," quickly answered the astonished chief; and, in raising himself, he added with dignity, "the only work I know is giving orders and dictating despatches,"—as it would be highly indecorous to suppose a chief knew the mechanical work of writing, or anything of the business of a clerk. However, I told him he must work, and he turned out an excellent basket-maker, and worked with considerable diligence and neatness, which proved the habit he had had of working for his subsistence. He had occasional paroxysms of madness, but, on the whole, they became less severe. He always ate by himself, with his face to the wall; probably from habit, or rather for fear of the influence of the evil-eye.

3d. The third variety of insanity is *acute mania*, or *furious madness*, and may be exemplified in the following cases, which exhibit the usual symptoms of more or less violence of action, and furious resistance, at any fancied restraint:—

*Synd Mojud Alli*, æt. 46, knew a little Persian, was naked

and raving, morose, and passionate; and would neither wear clothes nor work: no lucid interval. On several occasions he tried to escape from the asylum, but was caught and brought back. Was then very irritable and passionate, and sat naked, and silent. He had periodical paroxysms, when he was very violent, walking about, grinding his teeth, and threatening violence to those near him. At other times he was morose in his habits, preferred remaining in his cell, and performed the various movements of the body, that are used in the Mussulman prayers, in dumb show. Sometimes a word or two of the Koran was distinguished; but without meaning, or connection. His cell was left open, but he seldom left it, and, when he did, it was to walk about the grounds, without speaking or noticing any one; his eyes were wild, and face expressive of anger. Sometimes, when thirsty, he walked to the tank, and there satisfied his thirst, and on his return took up his position with the other insane, and dined with them. During the paroxysm, he ate his dinner by himself, in his cell. He used to be locked up during the day in his cell, which rendered him violent, more especially during a paroxysm, when he would not wear his clothes. Afterwards the door of his cell was always left open, and his violence subsided; but he retained his morose bad temper, and when walking among the patients he once or twice struck them, for obstructing his passage. This man had only slight and distant signs of improvement during several years he had been in the asylum, and there was little chance of his ultimate recovery; still he was disarmed of his violence, by being allowed a certain degree of freedom. He had nothing to obstruct or impede his movements; he was quiet, and if his faculties had been more normal they might have been employed, and thus allowed the morbid trains of thought to be weakened and cured. Probably this peculiarity was produced by ill treatment when first attacked with the disease, by which it was confirmed.

No effect was produced, in this case, by repeated trials of mesmerism, most probably from the fear and disgust which he produced in the mesmeriser. The patient always threatened violence when the mesmeriser commenced his passes, and showed great expertness in spitting at him. He would not work; and when thwarted in his wishes was very violent, striking and kicking with great force. It was curious, however, to see with what readiness he followed the wishes of the strong *burkundaz*, when required to take him to his cell, or to make him do anything. He was most violent to any one else; but when this keeper approached, was quiet, and allowed himself to be taken by the arm, and led about like a child, from an apparent conviction of his power to enforce his will. The same man had inspired respect into other insane patients, by his power and activity.

*Kureem Khan*, æt. 39, lost some land, after much vexatious litigation, which, together with the free use of opium and probably

gunjah, appeared to be the exciting cause of his insanity. He was admitted into the asylum in a sufficiently sane state to answer questions, and to agree to diminish the pernicious habit he had contracted of eating large quantities of opium. This was done by commencing with fifteen grains of solid opium, his usual daily quantity, and diminishing the dose by a grain daily, until none was left. He now appeared to feel no want of the drug. During the interval of the paroxysms, he was a strong intelligent man, and assiduously worked in the cook-room as an assistant, or in preparing screens, brooms, etc.

He took charge of an insane orphan boy, who was brought to the asylum. This unfortunate child was found near the place where the Hindus burn their dead. He was very much emaciated from starvation, and was in a state of amentia, probably from ill treatment. What a story of cruelty might he have revealed, but this was denied him! His reason was quite gone—"his lamp was out." But the broken reed was cherished, and supported by Kureem Khan; and it was an interesting sight to observe the care with which he attended to the helpless child, who crouched behind his protector, when any stranger approached; and then he would look up and smile upon his benefactor for placing him in security,—and it was such a smile of sweetness! His spare body supported one of the most beautiful heads I ever saw. Such a beauty of form!—such a brow!—such large black expressive eyes, sheltered behind such graceful eyelashes!—and such a beautiful chiselled mouth, as would have formed a study for Raphael! The foster father never left his child, and tried to instruct him. He carefully taught him to repeat scraps of Sadee; but such was the defect of the child's memory that he required continual prompting. His own name he did not recollect. Poor boy! gratitude appeared to be the only remnant of his ruined mind. His protector, when the paroxysm of insanity was on him, changed his whole manner; for some time he remained silent, and seemed to wander about to get rid of the inward distress that preyed upon him, and the insane child was the first to feel its effects; indeed, such was the acuteness of his instinct of danger, that, on the first indication of the coming paroxysm, he fled, and hid himself from his violence. The madman's face assumed a most diabolical expression of rage; and he immediately went in pursuit of the boy, and his other enemies, who were now to be chastised. This he supposed he did by rolling up a portion of his clothes, and beating them with a stick, and heaping all kinds of abuse upon the bundle. In other cases he took a brick, or the trunk of a tree, and beat it; sometimes with a stick, with his hand, and with his elbow; and this was done with such violence as to bruise, and injure his arm. On these occasions he changed his elbow for his feet, and struck at his clothes, or the brick he had procured, until quite exhausted. I once called him to me, when in this state. He told me a long list of grievances, of his riches, his villages, and his rank; which had

all been taken from him by the base treachery of lawyers, and had reduced him to his present state. He then appeared quite satisfied, sought out the boy, and returned to his work. On one occasion this madman had a visit from his brother, and they remained for some time on most amicable terms, until something irritated him; when he suddenly became enraged, and beat his brother, so as to oblige him to fly for his life.

To obtain ornaments for the insane boy was the great stimulus to exert himself, and he executed a good deal of work, to get money to purchase clothes and ornaments with which to decorate him. Again a paroxysm of insanity would occur, and the boy was obliged to hide himself; although a short time before he had decorated him with ornaments, promised to make him a landed proprietor, and called him his son and brother.

These ornaments were generally stolen during the boy's sleep. At other times the rupees which the protector got for his work, were put into the hands of some one in the asylum, and so bad was his memory, and unjust his friends, that he was often cheated out of them. In other cases he seemed to accuse those around him falsely for having, he alleged, misappropriated his money.

*Puerperal Madness.*—The variety of mental derangement incident to women soon after parturition seems to be less common in Bengal than in Europe. A respectable Mohammedan requested me to see his daughter, fifteen years of age, who had become deranged after her first confinement. The visit was made in the evening, and the picturesque thatched house was surrounded by numerous majestic palms, plantains, bamboo clumps, and other beautiful tropical plants, which were partly illuminated by the setting sun. When we were seated, the afflicted female, dressed in a thin muslin dress, and guarded by several attendants, was brought and seated before us. During the interview she continued laughing, and chattering nonsense. Her parent, in his long Arab dress, and flowing beard, stood beside her, and related how she had been happily married, had lost her infant, and, by the sudden stoppage of a dysenteric affection, had been reduced to her present state. "She was the apple of my eye." "She could recite our prayers," he said. "None was equal to her in learning; but now she has forgotten all." "And I," continued her father, "was so fond and proud of her; and now she will not attend to me, or even eat of her favourite dishes. I prepared them for her yesterday, set them before her, and urged her to eat them for my sake; but she spurned them from her, abused me, and tore my face and clothes. Her loss to me is like the loss of repose: I shall never recover it." The father stated these and other particulars with the minuteness of one intensely interested in the subject. His pale countenance and tearless eyes proved that his grief was passing show. He continued,—“She now knows her father no longer;” and, turning to me, he added,—“Now, I look to you as her father, and mother, and her only refuge. Your kindness in

coming to visit my afflicted daughter shall receive my everlasting gratitude. She will not even eat any longer for me."

The treatment that was required in such cases was stated. Her fine hair was to be allowed to be cut off, with the exception of a central portion; but the husband was to be consulted, before the general plan of treatment was to be commenced. The father declared it would be a disgrace to the family—who lived in a hut, and found difficulty in procuring the means of subsistence—to remove the patient to the asylum, where she would receive the greatest attention and kindness, which would give her the best chance of recovery from her terrible malady. But such is the power of habit, and the weakness of human nature, among unenlightened races.

4th. *Mania chronica* includes *melancholia*, and is the incoherence of ideas, or of the usual and natural connection between ideas, thoughts, and emotions. In this variety of madness there is always a want of order and association in the ideas or thoughts, followed by a failure, in a greater or less degree, of the powers exercised by the will over the trains of ideas.

*Churn Das*, sepoy, æt. 40, was reprimanded for some fault, which was probably the incipient appearance of the mental disease, which sent him to the asylum. He complained of having been unjustly treated, and was very often abusive; but on walking up to him he always stopped, and "stood at ease," with his eyes staring on vacancy. He generally answered questions naturally enough; at other times he remained quite silent, and did not seem to understand what was said to him. His case was almost hopeless, as he would not work, or engage himself in any pursuit.

Another patient, afflicted with this variety of mania, was interrogated as to the reason why he was manacled, when he replied, "It was not for theft, or for like base acts, but merely for murdering my own child in a fit of insanity."

5th. *Complicated mania* is that variety which is associated with endemic and epidemic diseases, such as fevers, dysentery, cholera, or organic diseases of the brain.

Madness does not appear to have any marked deleterious influence on health; but it renders the individual more subject to various complaints, according to the climate, and nature of the endemic and epidemic diseases, of the country. Some diseases appeared to be rendered less common, by the presence of insanity, than in an equal number of patients in the city of Dacca; such as cutaneous diseases, piles, elephantiasis, enlargement of the spleen, and tuberculated leprosy. Even fever more rarely attacks the insane than those in health, and it assumes in them the remittent form, and generally continues only for a few days. The accessions occur twice a day, with a frequent soft pulse, considerable warmth of the surface, febrile uneasiness, with thirst and constipation. During the state of feverishness the insanity remains unchanged. The furious maniac continues his rhapsodies, and the melancholic con-



tinues disinclined to motion, and averse to, or incapable of working

While epidemic small-pox was raging in Dacca, the prisoners in the jail who lived well, and were lodged in healthy wards, generally escaped, although a considerable number were unprotected. Three only were attacked with small-pox, and one case was fatal. The insane in the adjoining asylum escaped until a late period of the epidemic, when eight patients were attacked with a very slight form of the disease. The small-pox appeared to linger longer in the insane hospital than in the city, and subsequently several of the insane died of the disease.

In Europe, it has been found that the diseases of the insane differ considerably in different climates. In Germany and France, many die of phthisis pulmonalis; and, in the damp climate of Holland, diarrhoea is very fatal. This disease is so fatal in the moist climate of Bengal, that one-third of the poorer inhabitants are believed to die of the disease, and it likewise carries off a large proportion of the insane.

*Dysentery* occurs most frequently in the beginning of the cold season, and is liable to recur in damp and cold weather. It begins as a watery looseness, which often quickly reduces the person, and, from the formation of ulcers in the colon, the patient sinks.

In such cases, the compound chalk mixture, ipecacuanha in doses of three grains, when there is pain, with a quarter of a grain of opium, or smaller doses, as two of ipecacuanha with one-fourth of a grain of opium, morning and evening, with an occasional aperient of castor-oil or rhubarb, was found the most useful plan of treatment. The compound kino powder, substituting prepared chalk for the cinnamon, is most useful, when there is great weakness, with a discharge of blood. Sinapisms and blisters over the abdomen are likewise of use in these cases. The practitioner is liable to be deceived in this complaint, from the state of the pulse, as it is often imperceptible or very weak, when there is severe inflammation present. In the body of a middle-aged insane patient, the caput cœcum, and the large intestines, were found very much thickened, and extensively ulcerated; the ilium was inflamed; a vomica in the lower part of the left lobe of the lung, with extensive effusion of recent lymph and fluid in the sacs of the chest, from inflammation of the serous membrane. This occurred in a patient who had died of what might have been mistaken for collapse, and in whom the weakness was very great. The diet in such cases must be in small quantities, and of a farinaceous description.

*General Debility.*—This very common and fatal disease among the indigent class of natives occurs generally at the end of the rains, or during the cold season; and appears to be a breaking up of the vital energies, without any apparent organic lesion. The nights being damp and cold in Bengal, the insane require to sleep in warm rooms, and have warm clothing; for although, in health, Asiatics are

not so susceptible as Europeans to external impressions; in their weak state, and with their scanty covering, exposure to cold has a marked and unfavourable influence upon the powers of the system. On this account, the sick-ward was removed to a recently-built room, which was much higher and drier than the former one; and by attention to warmth and ventilation, to nourishing food and warm clothing, a great amelioration in the health of the insane took place, with regard to this fatal disease. In it the person is first observed to lose his appetite, and becomes languid and weak, with an aversion to move or work. His pulse is small and fluttering, and soon becomes imperceptible at the wrist. The respiration is slow, and the expired air is imperfectly decarbonised. The sexual functions are much weakened, and the cerebro-spinal system partakes of the deficient energy of the other parts of the body. The intellectual powers and affections, with voluntary motion and sensation, are altogether insensible to ordinary excitants. The skin is dry and rough, in some parts is covered with sordes, and is so cold, that in one case I found the animal temperature at  $88^{\circ}$ ; and the average of a number of cases was  $95^{\circ}$ . Under these circumstances, the patient loses flesh; his extremities feel flabby and inelastic, and unless when great care is taken at the commencement of the disease, little can be done to avert its fatal course. When a patient showed any of the above symptoms, he was immediately removed to the sick-ward, where his diet was regulated, and remedies administered for the cure of the disease. These patients were often attacked with diarrhoea, and, from the digestive apparatus wanting its natural power, the food passed unchanged. So great was the weakness, that the patient could often not speak above his breath; but lay in the attitude he was placed in, and had not even energy enough to chew his food. Restlessness generally preceded the fatal termination, which often occurred without the patient complaining of pain, although the condition of his mind was apparently improved. In many of these cases diarrhoea occurred, of a liquid matter, generally of a yellowish appearance. In other cases, when there was more strength, the discharges were more liquid, and of a whitish colour.

The medical treatment of patients in a state of collapse consisted in the application of heat externally, as by fomentations, particularly with bran and turpentine; stimulants, such as ammonia, wine with sago, arrow-root, fine well boiled rice, soup, etc., according to the circumstances of each case, were administered internally, in order to revive the powers of the system. These means were often ineffectual, no favourable change was produced, and the patient got weaker, and died.

The application of the horse-shoe tourniquet, so as to impede the blood in its progress to one or more extremities, I found the most effectual stimulus in these cases. The quantity of blood that would circulate through the extremities is thus retained in the circulation of the trunk; and by this reduction in its extent, affords the most

ready, and the most powerful means of rousing the system, and allowing time for the use of medicines. For this purpose, the tourniquets may be applied to the four extremities, and kept on until the circulation is restored. In some cases I have kept them on for two days, with the best effects; but they must be loosened by degrees, and only when the heat and circulation is restored.

An insane patient, aged 25 years, was so weak from an attack of collapse, that when raised up, and put against his bed, he sunk as an inanimate object. He raised his languid eyes when spoken loudly to, but could not answer; his skin was cold, and covered with a clammy perspiration; bowels frequently moved involuntarily; no pulse could be felt at the wrist; at the carotids it was small and 88, and the temperature in the axilla was  $98^{\circ}$ , that of the air  $86^{\circ}$ . Neither external nor internal stimuli had any effect, and he slowly sank into the grave.

*Janu*, an insane patient, æt. 40, was in a state of exhaustion when I saw him. His surface was quite cold and dry; no pulse, and such prostration of strength, that he could not stand. There was no diarrhoea in this case. I applied the tourniquet to an arm and leg; and it was kept on for two hours, when the pulse had risen, and his body had become warm. In the evening this patient was found much better. His surface was warm, his pulse full, soft, and 68, and he had a better appetite.

Several days after I found this patient well; eating his breakfast with a relish. This is certainly a very marked example of the benefit of the tourniquet; for I know of no other stimulus that could have acted so quickly, and so powerfully.

*Cholera*.—The weak state of body rendered the insane subject to cholera, which passed rapidly into the collapsed stage, with great prostration of strength, cramps, coldness, and involuntary and constant thin, pale, liquid alvine discharges. This quickly reduced the patients to such a state of prostration, that few recovered from it. Indeed in some years none of the patients recovered, even when the treatment was commenced at a very early stage of the disease, and when it was not so fatal in the city of Dacca. On one occasion, during the prevalence of the epidemic cholera in the city, no cases appeared in the asylum; and the good food and attention to clothing in the jail seemed likewise to have exempted the prisoners from this disease, so that only one case appeared, and from its being immediately treated, the person was saved. At the commencement of the year 1849, eight cases occurred in the Insane Hospital, of whom six died. These cases early assumed the worst form, and the usual remedies were employed without success, such as heat and frictions, assafoetida pills, with calomel and opium. The tourniquets were not found so effectual, from the difficulty of keeping them on these patients, and the rapidity with which the disease passed to the collapsed stage. The greatest care was often ineffectual to prevent their loosening them, from feeling the

pressure uneasy. These and other cases, however, afforded me an opportunity of proving the great value of the stimulus, of throwing so much more blood into the system, to remove the morbid, and to set up a more healthy, action in the abdominal viscera. I have had extensive experience in the use of the tourniquet in cholera, fevers, etc., and I consider it one of the most powerful stimulating remedies we possess.<sup>1</sup>

Sometimes during the attack of fever, cholera, or other severe diseases, the mental faculties become clear; and, as the disease diminishes, clouds again envelope the mind, and the patients sink into their former deplorable state. The following is an example:—

Two large middle-aged Affghan brothers came to Bengal with some Cabul prepared grapes, etc., and appeared to have been proceeding to Sylhet, where very pretty ivory fans, etc., are made. For these trifles they wander for many thousand miles, exposed to all sorts of privations. During this visit one of the brothers died, and the other became insane, and was sent to the asylum on account of his violence; and, although apparently flabby in his muscles, like other inhabitants of the torrid zone, he proved to be endowed with great strength. He would not work, was easily excited, and was then most irascible and passionate; and still, in the “whirlwind” of his passion, he would suddenly subside into a state of meekness and respect to those over him; again launch out in the most violent bursts of passion, when not supposed to be seen by those he dreaded; threatening violence by his gestures and language, and gnashing his teeth, and spitting at his fancied enemies. He was attacked, during the cold weather of 1848, with cholera of a bad description. In his state of weakness, he earnestly desired to be bled. “It would cure him,” he said; although there were no pulse, and his extremities were cold. In this state of weakness he became more rational and communicative, and then told me that he was one of the soldiers of Akbar Khan, who was certainly the deliverer of his country from a foreign yoke, but by the most infamous treachery; and the unfortunate madman pointed to the marks of gunshot wounds, probably received while pursuing the British troops during the retreat from Cabul, and of sabre wounds, perhaps inflicted by arms almost stiffened by cold, which spared the man, but, from the privations to which he was subjected, seemed to have injured his intellect. I was curious to know more particulars, but, even when on the brink of the grave, he seemed to feel he had already said too much; and as he got stronger, he forgot the date, and every other particular I was desirous of learning.

I was one day investigating an alleged case of assault on one of the insane patients, by two of the servants of the establishment, when the

<sup>1</sup> See Observations on the Medical Effects of a Partial Obstruction of the Circulation,—M'Clelland's Calcutta Journal of Natural History, No. 28, for January 1847.

Affghan, who had now got well, soon joined the party. He declared "the charge was quite true; the patient had been beaten;" and he, grinding his teeth, and shaking his clenched fist, declared "that he saw these villains strike the poor man." "Yes, these are the rascals!" And turning to me with a patronising smile, he added, "Take my advice, sir, do as my Ameer (Akbar Khan) used to do in such a case; get a vessel of oil, put these two miscreants into it," throwing a savage look towards them, "and put a fire under it, and blow the fire, and punkah it;" and moving his hands as if they were the punkah, and in an ecstasy of maniacal pleasure, he shouted with delight; and after a pause, "Shall I go and prepare the oil;" and, tucking up his sleeves, and looking towards the kitchen fire, he added, "It is all ready." In April, he was obliged to be secured, on account of his violence, and for the first time threatened to kick me; and as I left him he poured out a torrent of abuse, among which I could distinguish, "Remember Macnaughten!"

There are usually three or four patients with *epilepsy* in the asylum; and, as the cause is more or less connected with the brain, the cure of such patients is rare. Convulsions were sometimes seen among the insane; and, from the usual danger of the cause producing them, they generally proved fatal. Other nervous diseases, as *chorea* and *catalepsy*, sometimes appeared. *Paralysis* was very rare in the asylum, and *apoplexy* equally so. In the fatal case of this latter disease, in old men, the attack was without any marked precursory symptoms.

*Ooduram Ghung*, a cultivator, aged 40, was admitted into the asylum on account of insanity, for which no cause could be assigned. Was very violent at times, when he frequently struck the other patients, on which account he was obliged to be confined and only allowed in the evening and morning to take exercise. At other times, he was silent and moody. He defaced the walls of his cell, by picking out the bricks, and destroyed his clothes and bedding. Had occasional attacks of epilepsy, which seemed to increase in violence, although great attention was paid to his general health. His chief occupation appeared to be to appropriate the blankets of the other patients to himself, several of which were generally wrapped round his own person, and others he secreted in his pillow, after removing the straw which it contained. When a blanket was thus secreted, the opening in the pillow was neatly sewn up, a work which must have engaged most of his time during the night. When advised to work, he said, "What would be the advantage of my working in the sun. I am well at rest here." He continued to show the same cunning and cautious conduct. He always left his cell with regret, and gave up his stolen blankets with difficulty; and immediately exhibited his caution and cunning in purloining others. Various remedies were in vain tried in this case, to remove the epilepsy. However, his strength and general health seemed little affected, and he may live for years in his present sad condition.



6th. The last variety of insanity is *Idiocy*, or *Imbecility* (amentia), which includes congenital imbecility, and that produced in old age.

*Treatment.*—The inexplicable nature of mental phenomena has led to the general belief among the ignorant, that the diseases are the effects of supernatural powers of a malignant nature. These are supposed to be various, which explains the peculiarities of different cases: for their cure exorcisms and expiatory sacrifices are still employed by the ignorant natives of India.

Unlike other diseases, there are always a considerable proportion of the insane who are neither cured nor die of the disease, but live for many years incurable lunatics. In this state they require very little medical treatment, and should be separated from the more recent and curable patients; to whom the care of the physician should be chiefly directed, as more cures are then accomplished.

The treatment of insanity in Bengal was either medical or hygienic.

The medical treatment consisted in the removal of any prominent class of symptoms, or of certain diseases to which the insane were subject, from the influence of the mind over the organs of the head. Local bleeding, with leeches, was employed in a few cases. It was in the more turbulent of insane patients, in whom the disease had existed for a short time, that cures are most generally obtained; a gradual restoration of the faculties occurring as the state of excitement passed off. When a patient was brought to the asylum in a very excited state, in some rare cases it was thought necessary to abstract a few ounces of blood; but in general this state was produced by a weak nervous condition, which an aperient and camphor mixture, with a generous diet, removed. In some cases, where there was a very marked determination to the head, the *douche*, or a stream of cold water allowed to fall from a height upon the head, and cold lotions, with warmth to the feet, were sufficient. When the pulse was less strong, a solution of tartar-emetic, in doses of two or three grains every three hours, with one-fourth of a grain of opium was used, and produced nausea and vomiting, a free perspiration, a soft pulse, and a cool skin. This treatment generally procured the patient sleep, which was insured by Dover's powder, or repeated doses of henbane or morphia. When these objects had been accomplished, the patient was allowed to remain quiet, until he got accustomed to the asylum, when he was encouraged to employ himself with the other patients.

This improved state required careful watching. An occasional mercurial alterative and a mild aperient were required; but as insanity is usually a disease of debility, and the patient is found weak, the skin cold, the body emaciated, and the features sunken, a more tonic treatment was indicated. In such cases much care was required to



keep them warm, to remove irritation by narcotics and tonics, and to strengthen the body by generous diet. In this state the patient was quiet and harmless, and the prominent feature of his complaint was deficiency or inactivity of mind, in which freedom of action required to be combined with constant occupation.

The *hygienic* precepts attended to in the treatment of the insane in India are chiefly directed to the improvement of the general health, by employment, so as to exercise the body and mind; and, by carefully avoiding causes of irritation, which fix the patient's attention on objects not connected with their disease.

As the patients were usually brought to the asylum in a state of squalid cachexia, much care and attention were required in giving them nourishing and easily-digested food, in keeping them clean, in clothing them warmly, and in requiring them to take as much bodily exercise in the open and fresh air as their state admitted. This was of great importance in hastening the recovery, and induced me to inspect daily the food prepared for the insane, which consisted of rice, fish, meat, peas, and vegetables. These were varied three times during the week, with dhal and rice, rice and vegetables, rice and fish, or rice and flesh. These articles were provided by the purveyor who furnished the supplies for the jail. The insane had two meals, and were allowed daily a small quantity of tobacco, and betel, in consequence of their being all habituated to their use. They had two suits of *clothing* during the year (each consisting of a chuddah, dhooty, and gamcha), and for their bedding each patient was allowed two blankets, a mat, a straw pallet, and a pillow.

In insanity, the chain of ideas or images which arise follow one another according to certain associations, over which the individual seems to have little control. We saw their characters in various degrees developed; but some impression was always found to have taken possession of the mind, and influenced the conduct, which would not affect a sound understanding, and was not corrected by facts and considerations that would, in ordinary circumstances, have removed the erroneous impressions. Reasoning, therefore, with patients on their peculiar delusions was of no use. I have often stopped opposite a raving patient, and abruptly interrogated him, on any familiar subject, so as to break the association; and as soon as the question was understood, he would stop, and, in a natural manner, answer the questions rationally. His morbid and absorbing train of thought was changed, and some well known image substituted. This experiment was the more important, as it afforded us the means of judging of the nature of the disease, and the means of remedying it.

The advantage of such a system will be understood, by considering the human system as consisting of corporeal organs, mental faculties, social affections, and moral or religious principles; and the proper use and application of these capacities in promoting the welfare and enjoyment of man. When these powers are checked

or impeded in their proper exercise, they produce insanity, which in general is a limited and partial derangement of the powers of the mind, which are weakened ; so that the individual is carried away by some absorbing train of thought, which, in the treatment, should be repressed, by calling into activity the other mental powers. But, before an opinion can be formed of the state of a person's mind, it is of importance to compare it with the mind of the lunatic himself in its natural state ; which, among the inhabitants of India, is, in general, more of a mechanical and sensual, than of an intellectual, nature. This consequently should vary the treatment. The insane patient was always employed, and was only coerced when absolutely required, for the safety of the individual, or of those around him. His faculties required to be strengthened, and the general powers exercised in a suitable and regular manner, to fortify the mental faculties, and produce a proper development of self-respect. This was best accomplished, by removing objects that recalled favourite, or morbid trains of thought ; by bodily exercise, or by the exercise of the patient's usual manual occupation. It was particularly necessary to observe that the feelings were interested in new and engaging objects, upon which the thoughts could be fixed, while the time was beneficially spent in laborious employment. This was particularly the case, from the patient's occupation being in India almost entirely corporeal ; it was consequently found the best means of alleviating the symptoms of these mental diseases, and ultimately of curing insanity. The imaginary love of self, and of ease, often baffled our efforts. But it was only when the mind was so clouded, or when we had to deal with those who were averse to any sort of restraint, or even to allow their clothes to be kept on them, that no impression could be made. Among such even, I have found example and authority of much use : and a habit was acquired, which soon proved that rest was a worse punishment than labour.

At the same time the irritating effects of restraint were avoided, as it induced the individual to use every means to gain his freedom. He viewed himself in the light of a martyr, and resisted his supposed enemies, and these efforts aggravated his disease. When patients were outrageous, and dangerous to themselves and others, restraint was necessary ; and they were immediately put into a dark cell ; but these paroxysms were generally spent in walking about, and raving. When they were impressed with the power at command, they were very tractable ; and, although generally armed with iron instruments when at work, no accident had occurred in the asylum of any consequence. In two cases, persons were struck on the head for some fancied, or real offence. In another case, a patient tried to hang another. In many cases they tore their clothes, or would not, without some attention, wear any. In other cases, they besmeared the walls of their cells with any liquid within their reach, or pulled bricks out of the walls. On such occasions, I sometimes confined them for a few hours, and found exacting a

promise to control himself on being enlarged had often the desired effect, when confidence was placed in the patient's word ; so that, while a degree of fear was not excluded from the asylum, yet the love of esteem was considered a more powerful principle ; and for this purpose those subjects were introduced in which the patients were found to take the greatest interest, and which allowed them to display their knowledge to the greatest advantage.

This humane system of treatment required the constant vigilance of the superintendents. In India this cannot always be relied on ; and we can never calculate that the patients are not neglected, or degraded by the attendants. There is a great difficulty in inducing patients, even although convalescent, to submit to any sort of labour. A rajah will pretend to be only able to act in ordering diplomatic matters, a landed proprietor in arranging accounts, a holy fukeer in contemplation, etc. ; but we have seen that the rajah may become an excellent basketmaker, a landed proprietor may be employed with advantage in carrying water, and the fukeer in cleaning the wards, in the morning. When the first sense of repugnance of the person, at such an employment, and their imaginary high rank is got over, the new occupation changes their morbid trains of thoughts, and thus acts very favourably on their diseased minds. A case occurs to me :—A middle-aged man was brought to the asylum in a state of amentia, and much weakened in bodily health. He declared he was the father of mankind ; and, as the first man, he never wore clothes. At times he seemed silent, and would not work. After attention to his general health, he was taken to the workshop, and in a few days he began to work, of his own accord, and to wear clothes. He soon became quite altered, became very industrious, inoffensive, answered questions rationally, and improved in health. There was every prospect of his speedy recovery, when he was attacked with diarrhoea, and died. Thus, by perseverance, by example, and a little indulgence, especially in diet, and dress in females, their obstinacy will in general be removed ; and an influence over the minds of such patients will be of great importance to their recovery.

The chief endeavour was that the minds of the insane should be occupied with agreeable and regular trains of thought ; in listening to cheerful music, poetry, narrative, the elementary branches of mathematics, etc., according to what custom had rendered most agreeable to the individual. In the insane asylum of Dacca there was such a large proportion of the patients illiterate, that other means were employed for occupying their attention during the day ; such as rural occupations and sports, and change of scene. These insane patients were employed as much as possible in regular and easy work, and healthful occupation, as most suited to what they were accustomed to previously, as the nature of their circumstances allowed. It was thus that the organization was adapted to the active exercise of those functions necessary to health ; and, as so large a

number of the insane had been cultivators of the soil, there were always a large proportion of the men ready to make themselves useful in digging, weeding, and watering the garden. They were ready at dawn, or as soon as the doors were opened, to proceed to work with the gardeners. Others brought water from the tank to clean the different wards and drains, and to water the gardens. Other gangs were constantly employed in bringing bricks from the city, in small hand-carts, to be broken at the asylum, and conveying them to mend roads; in bringing vegetables from the bazaar, etc. For such labours they got small sums of money, which were carefully collected to purchase mangoes, oranges, or such luxuries, which were given to them on Sundays.

The former habits of field labours of the insane patients rendered it difficult to teach them such arts as could be profitably followed without the walls of the institution.

Sometimes restraint was required when the patients refused to take food, or medicine, or anything that appeared absolutely necessary for their health. At other times they obstinately refused to take food, owing to the severity of the disease, without any reference to its ultimate effects. In many such cases they fancied great evils would follow from eating, and their caprice was often overcome by stratagem, or by exciting uneasy sensations, by the motion of a swing, etc. In other cases they appeared to be governed by the rules of contraries; in others from bashfulness, requiring that they should eat by themselves, or at a different hour from the others; and it was only in the very rare cases, in which the health would have suffered much from abstinence, that the stomach-pump was required to be employed.

A swing was found a useful amusement for the most noisy and restless, by which they were soothed; and it was used, before the patients were shut up for the night, as it made them sleep.

The introduction of manufactures into the asylum had been long tried, but was only partially successful, on account of the fickle character of the insane, and their indolent habits, which prevented their employing themselves regularly at work. This was likewise, in part, owing to the laziness, and want of method in the servants. However when I left the supervision of the asylum, there were several employed as tailors, as basket-makers, in spinning twine, in making ropes, etc.

The difficulty of finding occupation for the women was greater than for the men, as it had never been exacted from them compulsorily; and their love and habit of inactivity, prevented many from exerting themselves. I enlarged the shed, and the number of dinkies, or hammers at the extremity of a lever raised by the weight of the body, to break bricks for the roads; and all the women worked an hour before breakfast, and about four hours after. Others were employed in the morning, in washing the wards, clean-

ing cotton, etc. Spinning, knitting, sewing, and other domestic occupations, have only been partially introduced.

*Amusements.*—After their daily work was finished, all the patients were allowed to sit out on the grass or in the verandahs, and to amuse themselves. Their favourite amusements consisted of (panchese) a kind of draughts, and cards. They did not enter with animation into these games, because they did not play for money; but at their homes, where the natives play for small sums, they become so excited as often to play for days together. Among these insane patients the great desire was to cheat without detection; and it was amusing to see with what delight they performed their cunning, and often successful frauds upon each other.

Another great source of amusement was music, and it was always popular among them. The favourite hand drum (tom-tom) was always near; and when I went near, one or two immediately commenced singing one of their songs, accompanied with the drum. An old man, priding himself on being able to play on the drum, would seize it, and another sang, while the other patients were delighted, as they distinguished their favourite tunes, and joined in the chorus. On one occasion, a young man, reduced by neglect, to a walking skeleton, would throw about his arms, and his scarcely animated muscles, aping the lascivious actions of health and energy. A painful sight!

An amusement was found for some of these patients by attending on animals, such as deer, sheep, and cows; and it was interesting to observe how immediately patients interested themselves in these occupations, supplying the animals with food, and other necessities, and thus awakening and exercising the social and benevolent feelings.

The insane patients were generally soon reconciled to their condition. A very few only of the violent patients were sometimes clamorous for their release. This was likewise the case with some of the convalescents.

The improvement required in the asylums in India is chiefly in carrying out a more perfect classification, and in encouraging the more gradual employment of the faculties on the return of reason.

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ARTICLE II.—*Case of Extensive Caries of the Tarsal Bones, and of the Lower Ends of the Tibia and Fibula: Successful Amputation at the Ankle-Joint; with Remarks.* By C. BLACK, M.D., Bachelor of Medicine, and formerly Medical Scholar in Physiology and Comparative Anatomy in the University of London, Member of the Royal College of Surgeons of England, etc. etc.

W. A., æt. 19, of nervo-bilious temperament, four years ago had his foot trodden upon, from which he experienced severe pain at the



time. The pain was never afterwards entirely absent, but continued of a dull, aching, gnawing character, and was attended by some degree of swelling of the instep. By degrees complete lameness ensued, which confined him to the house. His general health now began to give way, the local disease progressed, and at length an abscess formed over the scaphoid bone. After this had opened, he was removed to an infirmary, and whilst there, other abscesses formed and opened over the tarsus and on each side of the ankle-joint.

Extensive disease of the tarsal bones having been recognised, amputation of the leg was proposed, to which the patient and his friends objected, and he was consequently removed from the infirmary. At home, for two years, the disease continued to progress, and to make gradual inroads upon the patient's system, which was ultimately reduced to the greatest degree of emaciation. On October 27th, 1851, I saw him for the first time. He was extremely pale and emaciated, his system being reduced to the last stage of attenuation. He complained of constant uneasiness in the affected foot, of loss of rest, and general debility. The right foot is considerably deformed, from thickening and induration of the soft parts of the tarsus and around the ankle-joint. In these situations five sinuous openings exist, having thick everted edges, and the surrounding integuments of a deep bluish-red appearance. From these a copious, thin, foetid pus is discharged, and through one of them a portion of the scaphoid bone protrudes, which is easily crushed between the thumb and finger. Through these different openings the whole range of tarsal bones is found to be more or less carious.

In the lumbar region there is antero-posterior flexure of the spinal column, together with considerable deposit of suspected tubercle in the lumbar muscles.

The breathing is slightly hurried, and an occasional, short, dry cough exists. Examination of the chest detects little or no flattening beneath the clavicles; no increased dulness on percussion; but the inspiratory murmur is nevertheless somewhat rough, and expiration is evidently prolonged, particularly in the upper third of the right lung. Pulse 130 per minute, small and feeble; tongue red, smooth, and glazed in appearance, owing to the absence of its mucous epithelium; frequent thirst; appetite bad; bowels regular; has never had diarrhoea, although, from the condition of the mucous membranes, this might have been expected; urine scanty, high coloured, and deposits a copious sediment of the lithates; hectic fever present.

Notwithstanding the emaciated condition of the patient, the unfavourable state of the back, and the commencement of tuberculous deposit in the lungs, it was agreed, in consultation with two professional friends, that Professor Syme's operation at the ankle-joint offered the best chance of, at least, a prolongation of the patient's life. Accordingly, on the 6th of November, I performed this opera-



tion in the usual way, whilst the patient was under the influence of chloroform.

I took the precaution not to make the lower flap too long, and this I did by carrying the incision somewhat obliquely backwards, across the sole of the foot, from malleolus to malleolus. On entering the joint from the front, a quantity of pus escaped; and on sawing off the malleoli, together with a thin slice of the tibia, the cancelli were found to be considerably opened out, and to form several large cavities, divided by extremely thin partitions, the whole being surrounded by an outward shell of denser bone.

These cavities evidently resulted from the disintegration of numerous cancellous walls, the debris of which formed a soft, gritty, pulp-like substance. This condition of parts rendered it imperative to remove more of the lower ends of the bones, with a view of compassing, if possible, the disease. Accordingly, another slice, three-fourths of an inch thick, was removed; but even here the saw cut its way as easily as through a piece of "touch-wood." Disease to some extent still remained in the shaft of the tibia; but more of the bone could not be removed, without destroying the advantage of operation at the ankle-joint. Two vessels were secured by ligature, and, after oozing of blood had ceased, the flaps were carefully adjusted by sutures and strips of adhesive plaster, and the whole was supported by a few turns of a bandage. The limb was not allowed to press on the lower flap, and thus the risk of sloughing was somewhat lessened. One-half the wound healed by the first intention. On the third day after the operation, an erythematous blush affected the skin over the whole extent of the tibia, the integuments were slightly œdematous, and a degree of soreness on pressure was felt along the shaft of the bone. Similar patches of erythema affected the integuments of the thigh.

Lint dipped in warm water, and covered by oiled silk, was applied to the whole limb; the strength was sustained by a milk and farinaceous diet, and perfect quietude observed. Under this treatment, the erythema subsided in a few days, and maturation was fully established. The ligatures came away on the sixth day; and, by the eighteenth day, the wound had so far closed, that a little pus, lodged in the sac formed by the integuments of the heel, could not sufficiently escape. The skin was consequently punctured at this point, from which pus drained until the twenty-fourth day, by which time the stump had quite healed. As the mucous membrane of the mouth recovered its epithelium, and the excitement in the shaft of the tibia subsided, solid food, light tonics—as calumba, cod-liver oil, and stimulants—were administered. The patient henceforth rapidly gained flesh and strength; and at the present time (June 10th) is in the enjoyment of good health. An ample cushion of soft parts, constituting the former heel, covers the ends of the bones, on which any amount of pressure can be borne.

*Examination of the diseased Tarsus by unaided Sight.*—Passing

by the state of the soft parts, it is important to remark, that all the tarsal bones, with the exception of a part of the os calcis, and of that portion of the astragalus which enters into the formation of the ankle-joint, were in a state of complete rottenness. They formed a mass of disease, in which nearly all trace of their normal structure was lost, and presented to the feel a soft, spongy, gritty substance, which, under the slightest pressure, broke down into a confused pulp. The different tarsal articulations were destroyed, and the corresponding cartilages removed. The head of the astragalus and the projecting portion of the os calcis were the least diseased; but even the cancellous tissue of these parts was opened out, and contained a quantity of dirty, reddish-brown, sero-sanguinolent fluid. Such was the complete rottenness of the tarsal bones, that when the foot was suspended by the head of the astragalus, the mere weight of that member tore it off at the junction of the neck with the body of the last-named bone. The synovial membrane of the ankle-joint was thickened and inflamed, and its free surface presented a delicately-fringed and velvety appearance. On the upper articular surface of the astragalus there were three small ulcers, the largest of which was five lines in diameter. Their surface was covered with pus, and for some lines around their circumference the synovial membrane was of a deeper red hue than elsewhere.

*Microscopic Examination.*—The vascular membrane covering the walls of the osseous cancelli presents numerous points of ulceration, varying from  $\frac{1}{1600}$ th to  $\frac{1}{1100}$ th of an inch in diameter. These minute ulcers are irregular in shape, and vary as to their depth,—some of them being very slight abrasions of the membrane, whilst others perforate its structure, and lay bare the osseous tissue beneath. The cells which lie directly beneath this membrane, and which, in the healthy nutrition of the parts, furnish an elaborated fluid, to be taken up by the canaliculi for the formation of bone, are studded with minute granular matter, which must seriously interfere with their particular function. A similar deposit occupies the canaliculi and lacunæ, mingled, in the latter situation, with minute oil-globules. On the surface of the minute ulcers before-named, there is a thin sero-purulent fluid, which consists of diminutive pus cells, containing from three to six nuclei, of minute globules of oil, and granules of an albuminous nature. With these bodies, blighted blood-discs are associated in the fluid contained in the cancelli of the astragalus and os calcis. Vertical sections of the cartilage of the astragalus show it to be undergoing peculiar changes on both its attached and free surfaces. Dividing the thickness of this structure into three parts, the lowest or attached third shows the cartilage cells, arranged in parallel layers, of an ovoid form, and having their longest diameter parallel to the attached border of the cartilage. They are considerably larger than those of healthy cartilage, and contain from four to nine nuclei, which they are in the process of discharging into the subjacent tissue. The layer of cells imme-

diately above these is perfect, the cells not having attained their full growth. The hyaline substance of this portion of the cartilage presents a fibrous development, the fibres next to the bone being more fully developed than those superjacent to them. The middle third of the cartilage has fewer cells, which vary in diameter from  $\frac{1}{1200}$ th to  $\frac{1}{1750}$ th of an inch, and contain from three to six nuclei, some of which are in the process of subdividing. The hyaline substance of this portion is dimly fibrous. The superficial third of the cartilage presents at its lowest part the outline of large cells, lying with their longest diameter parallel to its free surface. These have discharged their nuclei into the superjacent tissue, which, as they approach the free surface of the cartilage, elongate, measure from  $\frac{1}{3200}$ th to  $\frac{1}{2500}$ th of an inch in their longest diameter, and become identified with the fibrous tissue into which the hyaline substance has been converted. Between the free surface of the cartilage and the synovial membrane a layer of nucleated cells is spread out into a false membrane, on the upper surface of which capillary blood-vessels pursue a waving, serpentine direction. Above these again the synovial membrane is thickened, and its free surface is studded with minute tufts, which give it a velvety appearance.

*Remarks.*—This case is interesting in both a pathological and surgical point of view. Under the former aspect, it is evident that the disease was of a tuberculous nature; that it commenced in the tarsal bones, from which it propagated itself to the corresponding cartilages, destroying them, and ultimately attacking the ankle-joint, and involving all the tissues which enter into its formation. In the last-mentioned situation, disease appears to have attacked the astragaloid cartilage at both its surfaces,—at its attached surface, by direct propagation of diseased action from the bone; at its free surface, by previous thickening of the synovial membrane, and the formation of a layer of nucleated cells beneath it. This view is supported by the fact, that the uppermost and lowest thirds of the cartilage were in a far more advanced stage of disease than was the middle third.

The origin and progress of caries in the osseous tissue appears, from the above case, as well as from others which I have very carefully examined, to consist in—

1. Vascular excitement of the membrane lining the cancelli, leading to interstitial deposit and thickening of that tissue.

2. Imperfect elimination of a formative fluid by the cells which lie between this membrane and the canaliculi and lacunæ of the osseous walls of the cancelli.

3. Molecular precipitation within these cells; the inability of these molecules, from the depraved state of the blood, to attain further development; their accumulation, not only within the cells, but also within the canaliculi and lacunæ, to which latter they are, in the effort at nutrition, from time to time delivered; and

the consequent more or less obliteration of such canaliculi and lacunæ.

4. Slow progressive ulceration of the lining membrane of the cancelli, as the result of the morbid condition before named; and gradual disintegration of the cancellous walls from arrest of nutrition, as a consequence of the obliteration of their canaliculi and lacunæ.

In a surgical point of view, the above case is interesting and instructive, inasmuch as it shows that, notwithstanding the all but total destruction of the tarsal bones by carious disease, the implication of the ankle-joint, and of the lower third of the shafts of the tibia and fibula, Professor Syme's operation, although it could not remove the whole of the diseased parts, was nevertheless quickly and perfectly successful.

CHESTERFIELD, 1852.

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ARTICLE III.—*Caries of the Tarsus—Amputation at the Ankle—Recovery.* By W. CROCKATT, M.D., Dundee.

ALEXANDER SMITH, æt. 37, of a spare habit of body, and very indifferent health, applied to me about the beginning of June 1851, complaining of acute pain in the arch of the left foot, aggravated very much by walking or manipulation; could assign no cause for it. There was very little discoloration or swelling at this time; leeches, iodine, etc., were applied. This treatment was continued for some time with very little success, he continuing to walk about a little, circumstances preventing him from desisting altogether. He was then advised to lay himself up, which he did; blisters were repeatedly applied, which were thought to be beneficial at the time; but the foot became more swollen and inflamed, the pain severer, and his nights were beginning to be very restless. The swelling on the dorsum of the foot now appeared a little elevated over the scaphoid bone, and was felt to fluctuate distinctly. At this time I requested my friend Dr Monro to see it along with me, when we agreed to make an incision into the swelling, which was accordingly done; a small quantity of pus, mixed with synovial fluid, escaped. On introducing the probe, bare bone could be felt, and the probe could be pushed through amongst the diseased bones. Potassa fusa was also applied to the outer aspect, and poultices kept at it. Afterwards the foot swelled very much, and he complained of severe shooting pains all through it; large quantities of pus and synovial fluid came away, and his general health now began to suffer very much, although every kind of nourishing food was administered, and for the most part of the time he had taken cod-liver oil, quinine, etc. After consultation with Dr Monro and Mr Mathew Nimmo, it was agreed that the foot should be removed. The patient was put completely under the influence of chloroform, and with their kind assist-

ance I performed "amputation at the ankle-joint," according to Professor Syme's plan, on the 19th of November. The surgical fever that followed was very severe for some days, but gradually wore off; the flap adhered, and rapidly healed in five weeks, with the exception of a small fistulous opening on the outer side, which discharged a thin watery fluid, but which also closed in a short time.

The patient has now an excellent stump, with a good thick cushion; can walk on the bare stump without the least inconvenience; and with the boot can perform a journey of three or four miles with the greatest ease; and he says a great deal further if required. It never swells nor inflames, nor is there ever the slightest uneasy sensation. He requires no help from a walking-stick, either in walking or in ascending or descending stairs; and it would require a very close observer indeed to notice any halt in his walk, or say which foot was off, or if there was a foot off at all. His general health is better now than ever it was before.

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ARTICLE IV.—*Amputations at the Ankle-Joint, performed in Dundee Royal Infirmary.* Reported by WILLIAM MONRO, M.D., Dundee.

No. 1.—*Amputation of Foot at Ankle-Joint.—Recovery.*

JAMES STEVENSON, æt. 27, engineer, was admitted into the Dundee Royal Infirmary on the 12th September 1843, under the care of Mr M. Nimmo. Shortly before admission, while engaged on the Dundee and Arbroath Railway, he fell upon the rails, when the wheels of several carriages passed over the dorsum of right foot, crushing the bones of the tarsus and metatarsus, and inflicting extensive injury upon the soft parts. It was resolved to amputate the foot at the ankle-joint at once, which was accordingly done in the way recommended by Mr Syme, a flap being formed from the heel, the ankle-joint disarticulated, and the ends of the tibia and fibula sawn off. The extensive injuries the integuments had received, interfered to some extent in forming the flaps; they were brought together, and retained by several strips of adhesive plaster and a roller. 15th.—On undoing the dressings from the stump, it was found that no union had taken place, and that the flap looked sloughy. Latterly it became quite gangrenous, and dropt off; the wound, however, granulated and covered the ends of the bones, and ultimately cicatrised to a small piece, about the size of a shilling, when he was discharged on the 6th March 1844. He is now a porter at the railway station, and is employed cleaning and lighting the lamps, etc. He walks about with great freedom; and has a boot constructed, not only with a cushion for the stump, but also to remove part of the weight of the body off it, by support from the calf of the leg.

No. 2.—*Amputation of Ankle-Joint.*

Jane Davidson, æt. 27, winder, Wallacefeus, native of Ireland, was admitted into the Dundee Royal Infirmary on the 10th January 1845, under the care of Dr Monro. States that twenty years ago she hurt her left foot, and that occasionally ever since she has had pain in it; nine weeks ago was seized with severe throbbing pain, and swelling on the external aspect of the left heel, and four weeks since an incision was made into the swelling, but no matter was discharged. On admission, the left ankle and heel are swollen, red, and painful; and on the outside is an opening, through which bare bone can readily be detected. Had syphilis about a year ago. The wound was enlarged, and a bit of lint soaked in strong nitric acid applied to the diseased bone; and she was ordered to take, thrice a-day, two and a half grains of the iodide of potassium. No improvement on the 24th February, when the wound was again enlarged, and a portion of the os calcis freely removed by the gouge. Little or no improvement on the 16th May, when the foot was removed at the ankle-joint, in the manner recommended by Professor Syme, an incision being made from one malleolus to the other across the front of the joint; the extremities of this incision were joined by another under the heel, and the flap from this portion carefully dissected off; the joint was then disarticulated, and the extremities of the tibia and fibula sawn off; one vessel only required to be secured by ligature. The flap was brought up and retained by a few turns of a roller, and lint soaked in cold water applied. Eight hours after the operation, the bandage was undone, the wound carefully cleaned, and the lips brought together, and retained by four stitches, adhesive straps, and a bandage. 18th.—Removed some of the dressings; there is a considerable discharge of thin pus. 20th.—Dressings entirely removed. No adhesion of the flaps; the inner surface of the wound looks sloughy; a large poultice to be applied. 22d.—A thin slough separating from surface of stump and flaps, below which healthy granulations are shooting up. 24th.—Healthy granulations covering ends of bones, and the whole stump looking healthy. Brought the flaps over the ends of the bones, and retained them by a compress and bandage. June 2d.—The flaps seem to have united throughout their whole extent. 4th.—The flaps have united throughout their whole extent; the ulcers on outside of the stump have granulated to nearly a level with the skin. Health improved. 23d.—General health much improved, and is sitting up every day. 26th.—On walking from the bed to the fire, there was a discharge of blood from the stump. 29th.—On pressing the centre of the flap, some pus is discharged from the wound on the outside of the stump. 2d July.—Complains of constant pain in the stump, especially during the night. 7th.—A probe can be passed from the opening on one side of the stump, to that on the other. A solution of sulphate of zinc to be injected daily.



15th.—Still complains of a burning pain in the stump. No carious bone can be detected with the probe. 30th.—Discharge from stump less, but still much pain. August. 4th.—The stump looks better, and there is less pain during the night. 30th.—No improvement since last report. Discharge from the stump foetid. September 16th.—Not improving; general health beginning to suffer. Is recommended to go to the country. Discharged—made an out-patient.

I have been somewhat particular in giving this case, as the leg was afterwards amputated below the knee. The 30th of May terminated the period of my attendance at the Infirmary, from which it will be seen that the greater part of the after-treatment was not under my inspection; and from a careful examination of the whole history of the case, I am of opinion that what was here wanted was a free dependent opening for the escape of the matter from the hollow of the heel. Had this been made about the 29th June, I am satisfied a different result might have followed; more especially as, when the limb was removed, no caries of the bones was found to exist, but a cavity containing thin, unhealthy, foetid matter.

No. 3.—*Caries of Tarsal Bones—Amputation at Ankle-Joint—Recovery.*

James Brodie, æt. 50, weaver, native of Ireland, was admitted into the Dundee Royal Infirmary, under the care of Mr M. Nimmo, on 8th September 1846. Has suffered for four months from pain and swelling of the left foot. About three weeks ago an abscess formed on the inner side, which broke, and has continued to discharge a thin watery fluid ever since. On admission, the foot is found much swollen, with great pain and redness; there is an ulcer in front of the inner malleolus, and another on the outer aspect of the foot. On the 29th, the probe passed from the external opening downwards and forwards about five inches, where diseased bone can be distinctly felt. No bare bone can be detected through the internal opening. March 3d, 1847.—Various applications having been employed without benefit, the foot was amputated this day at the ankle-joint, the patient having previously been placed under the influence of ether. An incision was made from the centre of one malleolus to that of the other under the heel; this was joined by another across the front of the joint; the flap was carefully dissected from the os calcis; the joint disarticulated, and the ends of the bones of the leg sawn off; one artery required a ligature; the flap was supported by one strap of adhesive plaster, a pledget of wet lint, and a few turns of a bandage. Considerable difficulty occurred during the operation, in consequence of caries and softening of the os calcis, which crumbled down, and required to be carefully dissected out. 6th.—Stump dressed to-day. Adhesion by the first intention seems to have taken

place throughout a great portion of the wound. From this date the patient went on well, and the wound cicatrised firmly. The original ulcers were the most troublesome part of the cure. Numerous local applications were made to them; and much attention was given to improve his general health, which, previous to the operation, was much impaired. Ultimately, on the 31st May, he was discharged cured, able to walk with great freedom upon the stump.

No. 4.—*Amputation of both Feet at Ankle-Joint, and Excision of Clavicle—Recovery.*

Michael Hogan, æt. 19, labourer, Seagate, native of Ireland, was admitted into the Dundee Royal Infirmary on the 25th February 1848, under the care of Dr Monro. Is convalescent from typhus fever, and much emaciated. States that while labouring under the fever, the toes of both feet became livid, and ultimately black and gangrenous. On admission, the greater part of both feet are in a state of gangrene, in some parts a reddish line is beginning to show itself, indicating a limitation of the disease. Hot dressings and poultices were applied to the feet, and his strength supported by a generous diet, and a moderate allowance of wine. On the 8th March, the sloughs having shown decided lines of separation, it was resolved to amputate both feet at the ankle-joint, which was done this day. Unfortunately the heels of both feet were involved in the mortification, so that we were obliged, in the one foot, to make the flap altogether from the inner side, which was healthy, and of sufficient size to cover the ends of the bones. In the other foot the sloughing had extended so far on each side, that it required all the integuments we could save from both sides, to obtain a sufficient covering for the ends of the bones. With the exception of the source from which the flaps were obtained, the operation was performed in every other respect in the manner recommended by Professor Syme. The patient was under the influence of chloroform during the operation. From the nature of the disease for which the operation was performed, and from the enfeebled condition of his system, consequent on the recent attack of eruptive typhus, as also from the complication to be afterwards noticed, the recovery was, as might have been expected, slow. The flaps in both feet united partly by the first intention, but a considerable portion of the wounds healed by granulation; much attention was required to preserve the general health, the patient having had several attacks of diarrhoea, with great prostration. He, however, ultimately got strong and well; and when he left the infirmary on the 29th December, the feet were quite whole, and he could walk with considerable ease without any assistance, and, by the aid of a single hand staff, he walked about the house and grounds freely.

A singular circumstance in this case was the occurrence of necrosis of the right clavicle, during the recovery from the amputations. About

the middle of April, an abscess, which had formed over the acromial extremity of right clavicle, was opened, and the bone found to be denuded of its periosteum for about two inches. The necrosis extended along the shaft of the bone, until nearly the whole was involved in the disease. On the 15th May the clavicle was excised; an incision was made from the sternum to the acromion, along the course of the bone; one-half of its acromial extremity was entirely free, and disengaged from the soft parts. The sternal half, however, was firmly attached, and required great caution in dissecting it out. The disarticulation was much assisted by using the bone as a lever. There was very little hemorrhage, no vessel requiring to be ligatured. Three stitches were inserted to keep the lips of the wound in apposition, and a pledget of lint, soaked in cold water, applied. The wound healed kindly; and, before the patient left the hospital, a firm cartilaginous structure occupied the situation of the excised clavicle. The motions of the arm, and the ability to use it, had become equal to what they had ever been.

No. 5.—*Caries of Tarsal Bones—Amputation at Ankle-Joint—Recovery.*

Richard Davidson, æt. 22, seaman, a negro, native of Philadelphia, North America, was admitted into the Dundee Royal Infirmary on 19th July 1850, under the care of Dr Monro. States that, nine years ago, while serving on board an American man-of-war, and when lying in the harbour of Valparaiso, he fell from the foretop, and injured his right foot, when a part of it was removed by Chopart's operation. The wound healed, and continued well until about a month ago, when he began to feel pain in the stump, with swelling. Soon after an abscess formed, which broke, and continues to discharge matter. On admission, the stump is swollen and painful; on its outer aspect there is a considerable ulcer, through which caries of the tarsal bones is readily detected. There are also several smaller ulcerations. General good health. August 12th.—It being resolved to amputate the foot at the ankle-joint, the patient was brought under the influence of chloroform, and the operation performed in the manner recommended by Professor Syme. Nothing unusual occurred during the operation. One vessel only required ligature. The flap was brought up and retained by several sutures, a few strips of adhesive plaster, and a roller. 20th.—Wound uniting; ulcerated surfaces looking healthy. Soon after this date, the wound from the operation cicatrised; but there remained several troublesome ulcers, which healed, and broke out again repeatedly. His general health improved much under the use of cod-liver oil; and when he left the hospital on the 24th March 1851, although very much better, there were still remaining several superficial ulcerations on the stump, which prevented him using it to any great extent. Several times, when the ulcers were nearly healed,

he could bear his weight, and walk upon the stump with considerable ease; and it is my conviction that the voyage to his native country would invigorate him, so that the ulcers would soon heal, and the stump become strong and useful.

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ARTICLE V.—*Indian Hemp as an Oxytomic.* By JOHN GRIGOR, M.D., Nairn.

AT the meeting of the Edinburgh Obstetrical Society, July 1850, Dr Simpson stated that “he had been induced to try the effects, if any, of Indian hemp, during labour, in consequence of Dr Churchill stating that it possessed powers similar to those of ergot of rye in arresting hemorrhage, when dependent upon congested states of the unimpregnated uterus. In the few cases of labour in which it was tried, parturient action seemed to be very markedly and directly increased after the exhibition of the hemp, but that far more extensive and careful experiments would be required, before a definite opinion could be arrived at relative to its possession of oxytomic powers, and their amount.”

In the last August Number of “The Monthly Journal of Medical Science,” there is an article, by Dr A. Christison, on the parturient effects of Indian hemp, being a continuation of a previous one on the natural history, etc., of that medicine.

These remarks are, so far as I know, the first and last that have been given birth to on that peculiar, and as I think uncertain, effect of the *Cannabis Indica*. I could have wished that these observations had been made on a more extended scale, and the effects more particularly and individually noticed, yet I will hope that my evidence may induce some others of my brethren to try it and note its effects, so that from step to step we may at length attain a full and correct knowledge of its powers and defects as a promotor of the labour pain.

Since reading Dr A. Christison’s seven cases, conducted at the Maternity Hospital of Edinburgh, I have used the Tinct. *Cannabis Indic.* (24 grs. ext. to 3j.) in sixteen cases. In nine of these, though given to the extent of 3ij. ss. in separate doses of 25 and 35 drops at a time,—in some in quick succession, in others at longer intervals,—I could not perceive any increased uterine action, nor the slightest physiological change in any one way during labour or afterwards, with the exception of one instance of sleep (much required at the time) in a lady, far from strong, confined of her third child, and much exhausted by inefficient throes, in whom the third 3ss. dose completely arrested the pains and induced sleep, which continued for an hour, when she awakened refreshed. Labour then set in in earnest, chloroform was given, and the child was speedily born. These nine cases made good recoveries.

In the seven cases in which the tincture of hemp succeeded so well with me, five were cases of first confinement, of satisfactory though very slow labour, and phlegmatic temperament. I have noticed the contractions acquire great increase of strength and frequency immediately on swallowing the drug, and have seen four or five minutes elapse ere the effect ensued; and if none was induced within the latter space of time, I have not observed its effects at all afterwards, notwithstanding repeated doses. In these few cases, I had opportunities of giving it from the time when the os uteri would admit the point of my finger till the expulsion of the child. Judging from experience, I believe that, in appropriate cases for the use of this stimulant, and when effectual, it is capable of bringing the labour to a happy conclusion considerably within a half of the time that would otherwise have been required, thus saving protracted suffering to the patient, and the time of the practitioner.

I have not observed it to possess any anæsthetic effects. I have used it in two cases along with the inhalation of chloroform, and I did not observe that that agent interfered in any way with its action.

When the effects of the hemp were subsiding, I have been able to recall and keep up "the good pains" by the addition of ten drops given from time to time. I consider the expulsive action of the cannabis to be stronger than that of the ergot, but less certain in its effect; and it has the advantage over the ergot, of usefulness in the early stage of parturition. I believe that the previous ineffectual administration of the hemp does not interfere with the after-exhibition and full working of the ergot.

Such are my brief observations on the new and interesting use to which Bang, or the Hachisch of India, has been put. In the few cases in which I thought its administration safe, and not counter-indicated by malformation, etc., you have given the result of those in which this effect was, and was not, displayed. I cannot conclude these remarks without entering my dissent against the use of uterine medicinal stimuli in general, on account of the frequent difficulty of accurate conception of relative dimensions of parts, etc. Yet all obstetricians must acknowledge that, in many cases, such stimuli are indispensable; and to be possessed of one capable of so early application, is decidedly a matter of much importance. I would also notice that, in labour, whether the cannabis shows its peculiar effect on the uterine contractions or not, there seems, as in tetanus, etc., to be a very great tolerance of the drug—nor have unpleasant consequences, so far as I have seen, appeared afterwards; and, whilst it is acknowledged as a powerful controller of inordinate muscular spasm, it is equally, in many cases, a powerful stimulant of the uterine muscular fibre in labour, if not in the unimpregnated state.

ARTICLE VI.—*On a New Mode of Applying Leeches.* By C. F. SLOAN, M.D., Ayr.

It frequently happens that the supply of leeches is deficient, more in quality than in quantity. By the following plan I have succeeded in converting very indifferent specimens into most efficient blood-suckers.

The idea first occurred to me some years ago, while waiting the effect of a dozen, which were remaining attached to the skin, but scarcely drawing blood.

It struck me that, as leeches are remarkably influenced by atmospheric changes, it might be worth while trying the experiment of partially exhausting the air over them, and thus inducing the sensation of a low barometer. On covering them with a cupping-glass, and by means of the air-pump producing a moderate degree of rarefaction, an immediate change in the style of sucking took place; the leeches displayed an activity I have rarely seen equalled by those of the best quality when applied in the usual way; they rapidly became fully distended, and fell off. By continuing the exhausting process afterwards, which is a common enough practice, a sufficient quantity of blood was readily obtained; and I remarked that the erysipelatous appearance, which usually followed leech-bites in this patient was prevented.

It is quite possible that the increased activity under the exhausted cupping-glass may arise from the skin being rendered more vascular, and thus yielding its blood more readily; or we may even view the leech in these circumstances as an elastic sac, communicating with the circulation by a firm canula, so that the blood may flow into the animal without much exertion on its part; but whatever the cause, the result will be found satisfactory. I have never had occasion to try the effect of snipping off the tails of the animals under this plan, but possibly it would be eminently successful.

The operation is almost as quickly performed as cupping, and has this advantage, that it requires no dexterity. I think any nurse of ordinary intelligence might acquire the necessary skill in a single lesson.

In charitable institutions this plan would be especially advisable, as by it two leeches might be made, on the average, to do the work of three, thereby effecting a considerable saving, and the necessary apparatus would always be at hand. Nothing could be better for the purpose than those cupping instruments, in which the vacuum is produced by an air-pump. Increasing the vascularity of the skin by a cupping-glass previous to applying the leeches, seems to make them bite more readily, probably by rendering the odour of the blood more perceptible.

Sandgate Street, Ayr, 6th July 1852.



ARTICLE VII.—*Contributions to Obstetric Pathology and Practice.*

By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—(*Continued from Vol. XIV., p. 149.*)

## NO. IV.—MORBID DEFICIENCY AND MORBID EXCESS IN THE INVOLUTION OF THE UTERUS AFTER DELIVERY.

THE enormous increase which occurs in the parietes of the uterus during the nine short months of pregnancy has long attracted the attention of professional observers. It is a kind of physiological hypertrophy unequalled, either in regard to its magnitude or its rapidity, in any other organ in the adult human body. For, during the forty weeks of utero-gestation, the uterus enlarges from nearly 3 inches in length and  $1\frac{3}{4}$  of an inch in breadth, to 12 or 15 inches in length and 9 or 10 inches in breadth. It increases from about 2 ounces in weight to 25 or 30 ounces. The cavity of the uterus before impregnation is less than one cubic inch, while at the full term of pregnancy it is extended to above 400 cubic inches; and the surface of the organ increases from about 5 or 6 square inches to nearly 350 square inches. Before impregnation, the uterine cavity would not hold above a drachm or two of fluid; at the ninth month of utero-gestation, its contents usually weigh from 120 to 150 ounces.

The rapidity, however, with which the uterus diminishes in size after delivery is perhaps still more marvellous than the rapidity with which it increases in size after impregnation. The celerity of its involution in the puerperal state is in fact more striking and remarkable than the celerity of its evolution during the pregnant state. If the process of absorption of organs in the adult is ever studied successfully anywhere, it will probably be by making observations on the reduction or involution of the uterus in women or in the lower animals subsequent to parturition.<sup>1</sup> Whilst the human uterus takes forty weeks to attain the dimensions pertaining to the fully-developed state of pregnancy, it requires only, on the contrary, from four to eight weeks to decrease from the extreme size of the organ peculiar to pregnancy, down to the small size peculiar to the same organ in its unimpregnated condition.

But in the vital mechanism of the involution or reduction of the uterus after delivery, various pathological derangements are liable from time to time to occur. This, like every other process in the

<sup>1</sup> In the Swedish Hygieia of last year, my friend, Professor M. Retzius, of Stockholm, published some interesting observations on the process by which nature effects the reduction of the puerperal uterus. He found, in a series of anatomical and histological observations on the subject, that the process of absorption of the walls of the puerperal uterus was preceded, as absorption of other deposits is, by fatty transformation of its component fibres; and that the blood during puerperal convalescence shows under the microscope a corresponding superabundance of globules or granules of fat.

animal economy, is apt, for example, to fail, either in the way of defect or of excess. Some years ago, I endeavoured to point out to my professional brethren, that occasionally, as one of the derangements in this mechanism of involution, the uterus is morbidly slow in regaining its original dimensions—its involution becomes impeded or arrested—and the organ is in consequence liable to be found weeks or even months after parturition still so large and unreduced as at first to be readily mistaken for a tumour of the uterus or ovary. I described this peculiar condition of the puerperal uterus, under the name of “Morbid Permanence of the State of Puerperal Hypertrophy,”<sup>1</sup> and illustrated it with the following example:—

CASE I.—During the summer of 1842, I attended, along with Dr Abercrombie, a lady, who, after a premature confinement in the country, had suffered from a smart attack of puerperal fever. After so far recovering for a few weeks, she was sent from a considerable distance into town, to be treated for what appeared to be a large tumour, stretching upwards from the pelvis into the right iliac region. The tumour had not been observed before delivery, and was somewhat painful to the touch. It seemed at first sight extremely doubtful whether the mass consisted of an inflamed uterine fibrous tumour or enlarged ovary, or of one of those chronic purulent collections which are apt to form towards one or other iliac region in connection with puerperal fever or inflammation. The uterine sound, when introduced into the os uteri, passed easily and directly upwards for several inches to the superior end of the tumour, and its apex could be felt there by the hand placed externally. This at once showed the supposed diseased mass to consist of the enlarged uterus. Further examination proved that there was nothing strictly abnormal about the uterus, except its great size. In fact, it was a case where the organ had apparently remained nearly undiminished after delivery, probably from the puerperal attack arresting the usual progress of its absorption and diminution. It decreased rapidly and fully under leeches and other local antiphlogistic treatment.—*Monthly Journal*, iii., p. 1011.

In alluding to the pathological state of the uterus, of which the preceding case is a well-marked example, Dr Lever has adopted the same designation which I originally applied to the affection.<sup>2</sup> More lately (1851), Dr Snow Beck described to the London Medical Society a case of this malady, under the simple term of “A New Disease of the Uterus.”<sup>3</sup> On examining microscopically the structure of the hypertrophied uterus in this instance, no inflammatory or heterologous deposits could be detected; but the tissue of the

<sup>1</sup> *Monthly Journal of Medical Science*, for 1842, p. 1011.

<sup>2</sup> *Guy's Hospital Reports*, vol. ii. (1844), p. 17.

<sup>3</sup> *Lancet* for 19th April 1851, p. 447.

organ was, it is stated, similar in its histological characters to the tissue of the uterus at the ninth month of pregnancy, except only that its component muscular fibres were smaller in size, or like those of a uterus at the middle period of utero-gestation.

Retarded involution or reduction of the uterus after delivery is not unfrequent in its less marked degrees; especially when inflammatory or febrile action supervenes and interferes with the phenomena of the puerperal state. It is often, for example, observable both during life and after death, in women who are the subjects of puerperal fever, pelvic cellulitis, and phlegmasia dolens. Chronic hypertrophy of the uterus in any excessive degree, from morbidly retarded or arrested involution, is more rarely met with. I have sometimes, however, seen it ten or twelve weeks subsequent to delivery, in the form of an apparent tumour, twice or more the bulk of the normal uterus. In lesser, though still sufficiently remarkable degrees, it often persists for many long months, or even years, after parturition; particularly when combined, as I have repeatedly found it, with antifixion or retroflexion of the fundus uteri, or with the state of prolapsus.

The day after (July 18) the preceding remarks were written, I saw with Dr Retzius the following instance of this combination of retarded involution with retroversion of the uterus, in a lady, who came for advice from the north of England.

CASE II.—The patient, aged 28, and married for three years, was delivered of her first child two years ago. She was so well as to be allowed to leave her bed at the end of a fortnight. The lochia however, were very abundant, and continued for eight weeks. She nursed her child for seven months, and during this period of lactation the menses recurred, as they have done since, regularly, profusely, and each month somewhat prematurely. She complains of pain of the back, weakness, etc. In making a vaginal examination, the body of the uterus, which is retroverted, feels large and heavy, like a uterus at the third month of pregnancy. Its cavity is, as appears by examination with the sound, nearly an inch greater than the natural length. The cervix is large, and fills entirely the extremity of the largest-sized speculum. Its surface is red and congested, but presents no appearance whatever of abrasion or ulceration. The os is most unusually patent, and admits the tip of the finger for about half an inch. The lining membrane of the cervical cavity feels, with the os, hypertrophied, and thrown into prominent folds, and some of the nabothian glands are much enlarged. The hypertrophied body and fundus of the uterus seem quite free from fibrous tumour, or any other heterologous disease.

Sometimes hypertrophy of the uterus, from reduction or arrested

sub-involution of the uterus, follows upon abortion or premature labour. I have at present the following instance under my care.—

CASE III.—The patient, æt. 35, and the mother of six living children, had a premature labour on the 11th December 1851, under the charge of my friend, Dr Dickson, of Bathgate. The labour came on about the fifth or sixth month of pregnancy, and was in itself simple and easy. The convalescence, however, proved slow and imperfect; and she was indeed in a great measure confined to bed for three or four months afterwards. The lochial discharge stopped on the second or third day. Menstruation has recurred regularly, but with some degree of menorrhagia. There is slight leucorrhœa. The uterus feels heavy and hypertrophied when examined per vaginam, but without any organic disease in its walls. The cervix is also much enlarged, particularly the anterior lip, which is considerably thicker than the posterior. Immediately around the os there is a line or two of granular ulceration. The uterine cavity measures between three inches and three inches and a half.

The preceding instances and remarks refer to deficient, impeded, or arrested involution. But a morbid *excess* of involution or reduction in the uterus after delivery (*super-involution*) is still more rare than a morbid *defect* in it (*sub-involution*); and I am not aware that hitherto any obstetric pathologist has described the former as a diseased state of the uterus. The following remarkable instance of such super-involution, as ascertained both during life and by dissection after death, has lately fallen under my notice:—

CASE IV.—The subject of this rare pathological affection began to menstruate at the age of thirteen; and the catamenia recurred regularly every four weeks till she became pregnant when eighteen years old. Utero-gestation went on without any unusual phenomena to the full term; and her parturition was natural but tedious, a male child being born after a labour of seventeen hours. Nothing unusual occurred during her puerperal convalescence and lactation. But subsequently to delivery she never menstruated. She was, however, subject to frequent attacks of diarrhœa, which she herself believed to be generally most severe at recurring monthly intervals; and the dejections were then sometimes tinged with blood.

Two years after her accouchement, she became a patient in the female ward of the Royal Infirmary, complaining of the state of amenorrhœa, with attendant broken health. She suffered from pain in the back and hypogastrium, with a sensation of weight and pressure in the pelvic region; dysuria; a furred tongue; and a weak compressible pulse, generally beating from 80 to 90 in the minute. She was thin, feeble, and anæmic in appearance. The mammæ were shrunk and flat. For some time before admission she had

suffered much from occasional headaches and giddiness; frequent nausea and vomiting; palpitation and occasional rigors.

On making a vaginal examination, I found the uterus small and mobile. The cervix uteri was much atrophied, and the vaginal portion of it scarcely made any projection into the canal of the vagina. The os uteri was so much contracted as to admit a surgeon's probe with difficulty. It was dilated by a slender bougie being left in it for two or three days; and, when the uterine sound was subsequently used, the uterine cavity was found to be only one and a-half inches in length, or about an inch less than normal.

A variety of means were employed with the view of benefiting the general health of the patient, and of exciting action in the uterine system, but with little or no effect.

Diarrhœa repeatedly occurred during the three or four weeks she remained under my care, requiring the free use of opiates for its restraint; and as the uterine symptoms did not at the time seem to admit of special attention and treatment, the patient was transferred to one of the general wards of the hospital, where she was placed under the care of my colleague, Dr Bennett.

During the following month, the diarrhœa recurred from time to time very severely. At last, anasarca in the lower extremities and albuminuria supervened; ascites followed; and shortly afterwards her face and arms became œdematous. About a month after these symptoms appeared, delirium at last came on, the fæces passed involuntarily, and ultimately she died in a state of prolonged coma.

On post-mortem inspection, some crude tubercles were found in both lungs,—especially in the left. The liver was enlarged, and showed some fatty transformation. The kidneys presented also some stearoid degeneration; and in the right there was, in addition, a small tubercular abscess. The large intestines were very much thickened in their parietes, and contracted in their calibre; while their mucous membrane was ulcerated in various parts. Along the lower end of the ileum several large ulcerations were seen running circumferentially around the interior of the bowel. One or two ulcerations were also found in the stomach. The uterus was very small, and atrophied in its length and breadth; its size being diminished about a third below the natural standard in all its measurements; and its parietes were correspondingly thin and reduced. The whole length of the uterine cavity from the os to the fundus was not more than one inch and a-half, while the normal uterus usually measures in this direction two inches and a-half. When a section was made of the posterior wall of the organ, the thickness of its parietes at their deepest or most developed point was not above three lines, instead of the normal measurement of five or six lines. The tissue of the uterus appeared dense and fibrous, and the section of it presented the orifices of numerous small vessels. The ovaries seemed also much atrophied, and smaller than natural. Their tissue was dense and fibrous, and presented no appearance of





**Graafian vesicles.** There was no inflammatory deposit on the peritoneal surface of the uterus or its appendages; but some thick pus, or tubercular matter, existed in the distended cavity of the right Fallopian tube.

The woodcut on the opposite page presents the uterus and upper part of the vagina, the broad ligaments, and ovaries—of the exact size which they presented, and their degree of atrophy may be easily judged of by comparing the sketch with the same parts when of a normal size. The sketch represents the posterior surface of the uterus and broad ligaments, with the uterine cavity exposed, in order to show the diminished thickness of the parietes of the viscus. The whole parts represented in the wood-cut weighed only one ounce, four drachms, twenty-five grains, in apothecaries' weight.

In females, subsequently to the cessation of the menses, the uterus, along with the ovaries, undergoes a slow but marked reduction in size. The uterus in this way, in some extreme instance, recurs, subsequently to menstruation, nearly to the small dimensions appertaining to it previously to puberty; it becomes atrophied and shrunk in all its measurements and dimensions; the vaginal portion of the cervix can be specially felt flattened and reduced in size; and occasionally the canal of the cervix, especially at its upper part, is found so contracted as not to admit of the passage of a probe. This form of senile atrophy of the uterus is doubtlessly connected with the natural suspension of the functions of the reproductive organs; and in this respect is so far similar to the results which we sometimes see in other viscera, as the testes, when the functions of these viscera are arrested in the course of nature, or when they happen to be prematurely suspended by chronic inflammation, or other forms of disorganising disease.

The principal peculiarities in the instance of the marasmus, or atrophy of the uterus, which I have detailed, are two-fold:—1. The occurrence of the affection in a very young female, as a consequence of pregnancy and parturition; and 2. The excessive degree of that atrophy or super-involution,—the uterus being, as we have seen, reduced fully a third in size below its natural dimensions.

During the last few years, I have seen a number of cases of permanent amenorrhœa connected with an atrophied, or rather undeveloped, condition of the uterus. In these cases, the cavity of the uterus generally measures from one and a-half to two inches in length, as ascertained by the use of the uterine sound; the shrunk cervix usually projects but slightly into the cavity of the vagina; and the opening of the os is small and contracted. In these cases, the uterus has apparently not taken on its usual degree of evolution and growth at the period of puberty. The organ, by a kind of malformation from defective development (as teratologists would describe it), retains after the date of puberty the type and size which normally

pertain to it in the state of girlhood. But the particular case of under-sized uterus which I have described in the preceding paragraphs is quite different from these; for in it the uterus, after being fully developed at puberty, and after performing normally the several functions of menstruation, pregnancy, and parturition, returned, as it were, suddenly to the type peculiar to the organ antecedently to the commencement of menstrual life; or, perhaps, we may more correctly say, it assumed, at the early age of nineteen, by an excess of the natural involution or absorption pertaining to the puerperal state, a degree of anatomical atrophy of its structures, and physiological arrestment of its functions, such as does not occur normally till the age of forty-five or fifty; and even then, at that advanced period of life, the degree of physical reduction in the size of the organ only rarely becomes so very great as was observed in the case which I have detailed above.

The case itself affords no precise data for determining whether the atrophy of the ovaries and uterus stood to each other in any respect in the relation of cause and effect; or whether they were both simultaneous results of one common agency.

Instances from time to time occur, in which, as in the preceding case, permanent amenorrhœa follows parturition. It will not, of course, be found that all such instances depend upon excess in that process of natural absorption or involution which follows upon delivery. But the case in question shows that this super-involution of the uterus may be expected to be met with in some instances in connection with this type of amenorrhœa.

At present I see professionally, from time to time, a case in which amenorrhœa has followed parturition, and in which the uterus is also reduced below its natural size. The following are the principal points in its history:—

CASE V.—The patient, now aged 30, has been married ten years. She has borne three children. From the time of her third labour (which occurred four years ago), no menstruation has recurred. The catamenia thus ceased at the age of twenty-six. After this third labour she made a good recovery, and left her bed on the eighth day. She nursed the child for fourteen months. I first saw her about two years subsequently to the birth of her last child. She then supposed herself to be again near her confinement; but the case was only a marked example of spurious pregnancy. The uterus, instead of being enlarged, felt looser and smaller than natural; and the vaginal portion of the cervix was specially reduced below the natural standard in length and breadth. The cavity of the organ was somewhat diminished below the usual length, and did not allow the stem of the sound to pass up to quite two inches and a half. In this patient the mammæ are flat and atrophied; and she is thin, weak, pale, and impaired in health and strength.

Sometimes super-involution of the uterus follows abortion, or premature labour. During the present month, I have had placed under my care an instance of this complication, in a patient from Canada.

CASE VI.—A mother of eight living children, now aged 35, had, on the 29th July 1851, a dead premature child, about the sixth or seventh month, under the charge of Dr Campbell. Nothing occurred to impede her convalescence. Before, however, her confinement, her health was not so good as usual, and she was disappointed to find it remained so after delivery. She has only once seen the catamenia during the past twelve months—viz., in September. There is slight leucorrhœa. She is anæmic and chlorotic, with palpitation, etc. The mammæ, which she states were previously very full and large, are now shrunk and flaccid. The uterus is of nearly its natural length, but the vaginal portion of the cervix is very short and atrophied, with the lips somewhat everted. The superior portion of the cervix, above the reflexion of the vagina, can be felt small, firm, and cylindrical; and the body and fundus of the uterus, when grasped between the left hand placed above the pubes, and the two first fingers of the right hand introduced per vaginam, appears under examination unusually mobile and slender, and altogether reduced below the usual standard of size.

NO. V.—NOTES ON THE THERAPEUTIC ACTION OF FURFURINE,  
NICKEL, ETC.

For many centuries past, the principal and most important additions which have been made to the *Materia Medica* have been in the form of substances or principles derived from the vegetable kingdom. But new, and perhaps still more potent, remedies will in all probability be yet derived from other sources. Chemistry is daily multiplying upon us the already almost innumerable class of organic compounds. Some of these compounds will no doubt yet be found to possess powerful and important therapeutic properties. Chloroform is an instance to which I may venture to refer in illustration of this remark. Again, among the most potent therapeutic instruments in our present pharmacopœias are to be reckoned the vegetable alkaloids. Modern chemistry has, as is well known, found out the means of forming artificially several alkaloid substances analogous to those existing naturally in the vegetable kingdom. Upon some of these I have made experiments, but particularly upon furfurine,—an alkaloid that produces, in experiments with poisonous doses upon the lower animals, many of the symptoms of quinine; and the salts of which I have found to act as a tonic, if not as an antiperiodic, when exhibited to the human subject.

Our modern pharmacopœias contain preparations from the oxides or salts of various metals, as antimony, arsenic, bismuth, copper,

iron, lead, mercury, silver, and zinc, besides metalloids and metallic earths. Some of these possess the most decided and valuable therapeutic action upon the human economy. But all, or almost all, of these metals were already used medicinally by the ancient Greek and Roman physicians; and though in later times the list of known metals has been greatly increased by the researches of chemists, the therapeutic action, if any, of these metals has not been made a matter of research by medical men. It seems, however, *à priori*, highly probable that some of the new, like some of the old, metals, will turn out to have decided, and it may be important therapeutic properties. At all events, few fields seem so likely to yield new therapeutic results—if such results are to be obtained at all—as experiments and observations upon the preparations and salts of those metals that have hitherto not been tested medicinally.

Last year, impressed with these views, I began making various therapeutic experiments upon myself and others with different metals, as cadmium, iridium, tellurium, etc. I have had little leisure to prosecute the inquiry. But I have obtained some results which seem to me not without interest or importance. One of the new metals which I have used most frequently is nickel. As with the others, I have used it generally in the form of a salt (a sulphate), believing that, as in the case of the metals already known, the use of *one* of its salts would give a sufficient view of the generic medical action of the metal.

Sulphate of nickel has appeared to me to act as a gentle metallic tonic. I have generally used it in doses of half a grain or a grain, repeated thrice daily; and have given it in the form either of simple solution or of pill. In large doses it is liable, like sulphate of zinc or copper, to produce sickness and nausea, particularly if taken upon an empty stomach. I have generally requested it to be taken half an hour or an hour after meals. It has appeared to me, as the result of pretty numerous experiments and observations, that the therapeutic actions of the salts of nickel and manganese correspond in a considerable degree with the therapeutic actions of the salts of iron upon the economy; and that these three metals might, under many conditions, be almost used as therapeutic substitutes for each other. But they also specifically differ from each other in some respects. For example, in one most interesting case the sulphate of nickel arrested a severe form of periodic headache, which had previously defied iron in many different forms, and all other kinds of treatment that had been employed. The patient came from Italy last autumn, in order to place herself under my professional care; and for some months I was as unsuccessful as my predecessors had been in affording her any relief. But let me give the history of the affection, and the ultimate result, in the lady's own words. She drew up the following note of her case several weeks ago:—

“My headaches (she writes) came on soon after my second confinement in August 1847, and continued to return every tenth day

without intermission, up to the 1st of February 1852. During the first four years I was in Italy, and was attended by medical men of all countries—English, French, German, and Italian. I also tried hydropathy and homœopathy, the latter for six months, but all without the slightest effect. The pain came on in a small spot on the right temple, and lasted from twenty-four to thirty-six hours. After the first eight hours severe sickness followed, which continued up to the sixteenth hour. During the attacks I had violent cold shivering fits, succeeded by a burning fever. At times I was quite delirious from the violence of the pain. I have taken large doses of steel, iron, and quinine, besides many other sorts of medicines. The quinine I took at first only two days before the attack was expected. I then took six grains every day for a year and a half, but it never put off the headache a moment beyond its day and hour, nor would anything that I could do bring it on before the time. When I first came to Scotland to be under the advice of Dr Simpson in August 1851, he gave me thirty grains of quinine a-day for three days before the headache was to come on; but it returned to its hour, and as severe as ever. This was tried also with the next fit, with no better success. Dr Simpson then tried successively furfurine, beerine, and arsenic, but the headaches still continued up to the 1st of February 1852, on which day I had a most severe attack. On the 4th of February, he gave me the solution of sulphate of nickel to take; since which time, to my astonishment, my usual headaches have altogether disappeared.”

To the preceding account I have merely to add, that, if we may judge from the result up to the present time, the cure of this patient from the use of nickel appears entire and complete. And perhaps it is but proper to remark, that this result seems fairly attributable to the action of the nickel alone, inasmuch as there was no relief under the use of any of the means or medicines previously employed for years; while convalescence distinctly began from the date of the employment of the metal in question.

Further, it is perhaps not unimportant to observe, that while the disease had lasted four years without abatement, its subsidence in February could not be the result of change of climate, as the lady had already resided about five months in Edinburgh or its neighbourhood, without any noticeable amelioration in the recurrence and intensity of the headaches; and at last they disappeared under the nickel, at a period of the year—viz. the commencement of spring—at which, in our climate, headaches and other periodic diseases are known to be specially liable to become increased and aggravated.

In no kind of case is the beneficial action of iron more remarkable than in the treatment of chlorosis and amenorrhœa. I have seen nickel in a similar way apparently serviceable under the same circumstances. In the latter end of last year, I gave it in a case of amenorrhœa of ten years' duration. The amenorrhœa super-

vened at the age of twenty-two. At the same time a galvanic intra-uterine bougie was introduced, and left for some time in the cavity of the uterus. In the course of three or four weeks menstruation took place, and has recurred regularly from that period. In such a case, however, it is difficult to say how far the result was attributable to the local means used, and what share the nickel had in the restoration of the patient's health.

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## Part Second.

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### COLLOQUIA DE OMNIBUS REBUS.

COLL. III. — DE SUFFLATIONE CERVISÆ, — SUFFLATIONIBUS CLINICIS, — ET QUIBUSDAM ALIIS.

SCENA. — *Fronde super viridi, — montibus adversis, — coelo serenissimo, — tempore vespertino.* COLL. PERSONÆ. — *Editor, etc., ut antea.*

*Editor, introducing a distinguished Brewer.* I believe, gentlemen, you all know something of Mr —, whose beer has a more than European reputation. He has applied to me about a matter which I have begged him rather to submit to you in person.

*Cervisiarius.* I represent, gentlemen, the interests of Prestonpans Beer, Belhaven Beer, Edinburgh Ale, and all forms of malt, except Burton Pale Ale. My constituents are anxious to get up a grievance. We would like very much to be accused of poisoning our beers, for example.

*Medicus.* Indeed! And what then?

*Cervisiarius.* In order that, upon proof being given to the contrary, our medical customers may grant us a few certificates of skill and honesty, for public use in our trade. Allow me to read to you a lot of samples I have brought with me, to save you trouble.

*Chirurgus.* This is an extraordinary proposal. We, who use the beers you mention, and have just been drinking yours with satisfaction, know them to be wholesome, generous liquors, honestly produced from malt and hops. But, I put it to yourself, is it seemly that the members of a scientific profession should certify such things? — that their names should be spread over the land in puffing advertisements?

*Cervisiarius.* Since you put it so to me, I must say there was a time when I might have thought this a somewhat dubious proceed-



ing for gentlemen such as you. But now-a-days I see the advertising sheets of "Punch," "Bleak House," and the "Lancet" covered with beer-puffs by other medical gentlemen who hold their heads as high as you. In fact, the samples of certificates I have here for your approval are theirs,—certificates by eminent members of your profession in England. Now, we have been thinking here, that what they have done for their brewers, you might perhaps do for yours.

*Chirurgus.* Such a thing was never heard of in this quarter. But let us hear your certificates by all means.

*Cervisiarius.* Here you have a selection from a list of six-and-thirty in one advertisement about Burton Pale Ale.

1. "A close observation of its effects upon myself and upon many others to whom I have prescribed it, long ago convinced me that it contains nothing more than malt and hops [George Budd, M.D.]."

2. "My confidence in the purity of your Pale Ale remains unshaken, and my opinion of its great value in a dietetic and remedial point of view is entirely confirmed by time and experience [Marshall Hall, M.D.]."

3. "In the genuineness and salubrity of Pale Ale and Bitter Beer, as manufactured at Burton, my confidence remains unshaken [Thomas Watson, M.D.]."

4. "I am an admirer of this beverage, and my own experience enables me to recommend it as a very agreeable and efficacious tonic, and as a general beverage both for the invalid and the robust [Justus Liebig]."

5. "I have frequently recommended Bitter Ale medicinally; and when my home-brewed ale has been exhausted, I have been supplied with Ale from your brewery [Sir Charles Clarke, Bart.]."

6. "But that I am not now in the habit of drinking Bitter Beer, I should be glad to show my confidence by drinking plenty of it [R. Mortimer Glover, M.D.]."

7. "The daily adoption I witness of the future drinking of Pale Ale by former invalids leads to an additional conclusion in my mind of its value and salubrity. I am induced to believe, that Bitter Beer is an excellent adjunct to the physician in the exercise of his professional duties [George Robert Rowe, M.D.]."

8. "Your beer is the best ———."

*Chemicus.* But will you have the kindness to explain to us what occasion has arisen for all these certificates?

*Cervisiarius.* With all my heart. The story is short and simple.

You must understand that, some ten or twelve years ago, a French philosopher happened to wonder why the English got from Paris large quantities of a virulent poison, called Strychnine, which you probably know about; and so he fancied it might be used in England, instead of hops, for making beer. About a year ago, another French philosopher, recollecting his countryman's guess, expressed in his book his own suspicion on the subject; and the other day a

third Frenchman was stated, in an English periodical, to have repeated the conjecture, in his lectures at Paris, as a positive fact.

Brewers here did not much heed this nonsense. It was a natural observation for a Frenchman to make, who does not know good beer when he gets it, and cares little what he says about anything English. But hereabouts we felt pretty sure there was nobody in this country so foolish as to imagine that beer could be made with rank poison, or so ignorant as not to know what strychnine is really used for.

*Medicus.* Gamekeepers in Scotland use it largely for killing weasels, polecats, ravens, crows, magpies, and other vermin that prey upon game and their eggs. This is surely no secret. A demand for it has also arisen not long ago in Australia, for destroying the dingoes, or wild-dogs, which are very destructive to sheep there. The shepherd, trailing a lump of mutton for a *drag*, makes thus a ring-fence around his flock, and drops at intervals a bit of mutton duly seasoned with strychnia; and in the night-time the dingo, coming on the scent, runs it up, eats as he runs, and pays for his supper with his life, dying at no great distance from where he fed.

*Cervisiarius.* It is plain, therefore, that no British brewer had anything to fear from the French philosophers. But a sharp-eyed member of our trade perceived that the strychnine, which nobody in particular was accused of using instead of hops, so far from damaging *his* beer, might improve its credit. The contradiction of the charge was a glorious opportunity for an advertisement. It was only necessary to get up a chemical certificate of the purity of the beer, and medical attestations of its wholesomeness. Professor Graham and Dr Hoffmann supplied the one, and the medical profession in London seem to have competed who should furnish the others.—That, gentlemen, is the history of the beer-puff.

*Chemicus.* It has been cleverly got up, without a doubt. But, at all events, we are indebted to it for a new and excellent

PROCESS FOR DETECTING STRYCHNIA IN MIXED FLUIDS, which was devised for the occasion by Professor Graham and Dr Hoffmann. Two ounces of animal charcoal are to be shaken in about half a gallon of the suspected fluid; and the liquid is to be left at rest for a night, and then filtered through paper. The fluid is thus deprived of bitterness. The charcoal, which contains the strychnia, is then to be boiled for half an hour in eight ounces of rectified spirit; and the spirit, after being filtered, is concentrated by distillation. The remaining liquor, which is watery, is next decomposed with a few drops of solution of potash, and agitated with an ounce of sulphuric ether. The ether contains the strychnia in a state of considerable purity; and, on being evaporated, it deposits a white solid matter of intense bitterness. If a drop of sulphuric acid be placed on this residuum, and then a fragment of bichromate of potash in the resulting liquid, a beautiful violet tint appears at the points

of contact, and soon spreads over the whole fluid. This change of colour seems to be characteristic of the alkaloid strychnia.—The process now described must be one of great delicacy; for the discoverers could detect with it half a grain of strychnia in half a gallon of the pale ale of Messrs Allsopp and Son; and there is no doubt it may be applied to many other fluids.

*Medicus.* Without any desire to disparage the process, and with great respect for the eminent chemists who devised it, I submit that there scarcely was occasion for so operose a proceeding, to satisfy them that good beer did not contain strychnia. They might have proved, and might have instructed every beer drinker himself to prove, the absence of strychnia, by simply referring the question to the palate. The taste of the hop-bitter in beer, possesses eminently the quality of evanescence, which contrasts strongly with the persistent bitterness of all the substitutes which brewers have been accused of employing, such as quassia, cocculus indicus, and above all nux vomica and its principle strychnia. It is impossible that, in the finer qualities of beer and ale, such as those made at Burton, or in this neighbourhood, any man could mistake the enduring taste of strychnia for the fleeting aromatic bitterness of unadulterated hop. I am acquainted, indeed, with only one strong bitter which resembles hops in the quality of evanescence. This is the pretty *Daviesia latifolia* which [*To Chirurgus*] an old pupil of yours recognised the other day in your conservatory as a familiar friend in Australia; where it is called the “Native Hop,” and is used in making beer. I have tried a few experiments with the leaves, and find that they furnish an excellent tincture and infusion; so that this plant, by reason of its uncommonly evanescent bitterness, might prove a very convenient addition to our bitter stomachics. It does not possess, however, the aroma of the hop-bitter.

*Chirurgus.* But what do you think of the beer certificates? You have said nothing on that head; and I know you have had some experience of such things.

*Medicus.* I am entirely of your mind. I have often been assailed for certificates by inventors of new and improved sorts of food, drink, and physic; but I have invariably refused a written opinion, if not positively assured that it was not to be used for advertising purposes. Many of my friends here have followed the same practice; and I am convinced it is the correct professional rule. For, in the first place, if a physician or surgeon has some position in society, how can he, without injustice, grant one tradesman the use of his name, to the disadvantage of others not less meritorious? In the next place, if he has not yet an established name, or is in hopes of a better, does he not expose himself to the charge of taking part in an advertisement, and sharing in its benefits,—though not directly, yet not the less substantially? It is not to be supposed, for example, that the whole cloud of medical witnesses to the medicinal qualities of Allsopp’s Beer, will have their names ventilated on the

wrappers of every periodical in the country, to no purpose. Mr Allsopp, indeed, himself insinuates something to their advantage, when he says in his advertisement, that "publicity to these testimonials" is "due to the medical profession, who have so long recommended Allsopp's Pale Ale in all cases where dietetic treatment is required." But further, are such certifiers not aware that, whether they possess an established name or not, they may expose themselves to the suspicion of lending it for a direct consideration? [*Chirurgus is astonished*]. You may be surprised. It is true that Mr Allsopp's advertisements contain medical names, which are a guarantee against suspicion in their instances. But when I inform you that I have been repeatedly offered a fee for the use of an opinion for similar advertisements, you will admit it is not impossible, that the fee may not be always refused when the favour is granted. At any rate, those who know that such offers are made, may be excused for uneasy surmises respecting their professional brethren, whose names are constantly flourishing before the public in this particular shape.

*Chirurgus.* You make out even a worse case than I had anticipated against this pestilence of certificates.

*Chemicus.* It must be supposed, indeed I know, that those who grant such certificates, have often erred through ignorance. I once did so myself, long ago, when a fellow sent me a fee for a chemical analysis and opinion, and then advertised both it and me without my leave or knowledge. But now, all are forewarned; and, therefore, any physician or surgeon, who allows his name to be thrust under the public nose in this unsavoury form, whether he do so under cover of Bitter ale, British brandy, Pebble spectacles, Patent lint, or any other invention or trade conceit, must thank himself for a character. If he persist, he must accept the inference. For one may truly say of him, as they have it in Germany—"Es ist Hopfen und Malz an ihm verloren."

*Chirurgus.* This certificate-fever is not the only literary epidemic which has lately seized upon the medical profession. Pray, Mr Editor, can you inform me

WHAT IS A CLINICAL LECTURE? For such a multitude and variety of lucubrations appear now in the weekly periodicals under that name, that one is at a loss to think what some people understand by the title.

*Editor.* In many instances a Clinical Lecture is—A new way to advertise medical pretensions.

*Chirurgus.* That seems to be often its Final Cause, certainly. But what I meant to ask was,—What is the nature of a Clinical Lecture?

*Editor.* Judging from those which are perpetually appearing in print, it is sometimes—A tritcal, medical essay, not necessarily delivered, and unnecessarily published.

*Chirurgus.* I mean, of what does it consist?

*Editor.* Frequently—An abridgment of an article in Watson's "Practice of Physic," or the "Library of Medicine," and commencing with "Gentlemen."

*Chirurgus.* What else?

*Editor.* Or,—A catalogue of cases in the author's private practice, read, or supposed to have been read, to an audience, none of whom ever saw one of them.

*Chirurgus.* Well! well! Then tell me what a Clinical Lecture ought to be.

*Editor.* A commentary on hospital cases, addressed to students who visit them. But that is an old-fashioned sort, delivered now chiefly in Edinburgh, I believe, and not always seen in periodical literature.

*Chirurgus.* We have got at the right definition at last. Then what is to be said of a "Clinical Lecture on Diseases of the Rectum," in which the lecturer states in his exordium—"I shall first read the histories of a series of cases of gradually increasing severity," and then fills up an entire lecture on "Ulcerations and Excoriations," but principally on "Fissure of the Anus," with the details of the cases of—1, "A barrister about 35 years of age;" 2, "Mrs —, a lady;" 3, "Mr —, a gentleman about 60;" 4, "Mr H., master of a grammar-school;" 5, "Mr —, aged about 35, a tradesman in large business;" and, 6, "Mr P., aged about 35, a farmer of robust frame"? There is no mention made of a single hospital case, nor of an hospital reason for commenting on the subject of Anal Fissure, nor indeed of an hospital at all, except in the title; and yet this is published as a Clinical Lecture delivered at University College Hospital, London, by the Professor of Clinical Surgery. Every case tells of the cruel sufferings of the patient, the slight cause that produced them, the promptitude of the surgeon in relieving them,—and this by a trivial incision of the mucous membrane merely,—an operation long and now familiar, which he claims to have been contrived by himself upwards of twelve years before he thus, for the first time, appropriates it.

*Obstetricus.* Have I not seen it clearly described in Syme's Principles of Surgery, A.D. 1842, p. 334? Why write more about it?

*Chirurgus.* Mr Quain does not deign to make that acknowledgment, but refers to an observation attributed to Mr Copeland by Sir Benjamin Brodie, as the origin of the operation. Nevertheless, this Clinical Lecture, delivered without clinical occasion, published without literary necessity, is headed by the Editor, "Original Lecture."

Next week the same periodical presents another "Original Lecture" of the like denomination. In this "Clinical Lecture on Diseases of the Tongue, by Langston Parker, Professor of Anatomy and Physiology, Queen's College, Birmingham, and Surgeon to

Queen's Hospital," the lecturer starts with the intimation, that "two or three formidable cases which have occurred lately give me an opportunity of devoting one of my summer clinical lectures to the consideration of these diseases." But it turns out that his students had not seen one of these cases. For they are,—1, "A temperate gentleman;" 2, "A surgeon;" 3, A syphilitic "gentleman, who had been treated for five years by the most distinguished surgeons in France, Germany, and England;" 4, "A respectable married female;" and, 5, Another "surgeon, aged 47." [*To the Editor.*] These, I think, come under your second last definition.

*Editor.* It is a still greater perversion of the name of Clinical Lecture to apply it, as is done in other instances, to mere general descriptions of disease, not relating to any special case at all, written apparently in the closet, and very possibly never delivered to any listeners.

*Obstetricus.* Of course, however, you do not mean that a Clinical Lecturer has nothing to do with general descriptions of disease, or with his opportunities of private practice.

*Editor.* By no means. A lecturer of experience may often illustrate his clinical observations with great effect by reference to his private practice, and must often preface them by an exposition of general principles, or a summary general description of disease. But two qualities are inseparable from the essence of a Clinical Lecture. It must be substantially a commentary upon cases seen by those to whom it is addressed; and it must be primarily conceived and delivered with the pure and simple intention of instructing those to whom it is addressed—viz., students. If it be got up with any other view—as for the purpose of subsequent publication—it will seldom prove an honest, downright, clinical lecture. A clinical lecture, if truly such, must contain so much matter, new and important indeed to students, but trite to practitioners, that in general it cannot fructify beyond the lecture-room and hospital wards. Now and then, no doubt, it will stand publication, from the novelty of the materials, and the ability with which they are discussed. But there cannot be many such in a clinical course, if the lecturer studies to adapt himself conscientiously to the level of his hearers. A clinical lecture, delivered for the purpose of publication, is almost certain to become an essay; and the real design of the author is as certain to be betrayed to an experienced reader. Accordingly, many of the so-called Clinical Lectures, with which the weekly periodicals now teem, are manifestly no Clinical Lectures at all. Some are such as I have endeavoured to characterise in answer to *Chirurgus'* request for a definition. Others are imitations, "sed magno intervallo," of the excellent "Lectures on subjects connected with Clinical Medicine" of Dr Latham, who acknowledges that these "Lectures" were sometimes delivered only in substance, sometimes not even delivered at all, and who, therefore, studiously avoids calling them "Clinical Lectures."



*Medicus.* It is 105 years since the first Clinical Lectures on Medicine in Britain were delivered by Dr John Rutherford in the University of Edinburgh; and it is just half a century since the first Clinical Lectures on Surgery were commenced, also in the same University, by Professor James Russell. But great and manifest as was the improvement thus effected in medical education, it is only within these few years that the example has been followed in the schools of London. One cannot wonder, therefore, that the true scope of clinical teaching should be sometimes misunderstood there. One thing, however, is certainly surprising: How the hospital physicians and surgeons of London and England generally have had their eyes opened all in a moment to its importance—how they have been seized so suddenly with so great a fury of clinical teaching as appears from the weekly medical press. It is not because they receive the usual honorarium in return for this new application of their professional time and talents. For, strange to say, this is the only department of tuition which, in London at least, is proffered to the students without a fee. Some entirely new object surely must have been found to be attainable by clinical lectures.

*Chirurgus.* And it is this. Formerly, Clinical teachers delivered lectures for students. At present, in London, they deliver them for the medical public, who do not hear, but read them—just as a tiresome member of the House wastes his oratory upon his Honourable friends, not on their account, but for the reading of his constituents in next day's newspaper. Until lately the rule was, that Clinical teachers brought practice and experience to aid them in their lectures; but in London they now make their lectures help them to experience and practice. This is an illegitimate application of teaching; and such being its aim, it can seldom prosper.

*Editor.* The teacher, in short, produces a Clinical Puff, as well as a Clinical Lecture.

*Chirurgus.* And, meanwhile, what becomes of the poor student, whose interests seem entirely a secondary concern?

*Editor.* He is in the condition of the horse's stomach, which M. Bouley has been writing about the other day,—absorbing nothing, but letting all things pass unappropriated into another organ predestined for their reception.

*Chirurgus.* Videlicet,—the *cloaca* of medical literature, the Weekly Medical Press.

*Medicus.* What are these researches, which I see M. Bouley read in the end of June last to the Paris Academy of Medicine, on the horse's stomach?

*Editor.* He has reconciled a number of previously puzzling facts and discrepant inquiries, by proving a very startling proposition,—that

THE STOMACH OF THE HORSE DOES NOT POSSESS THE FUNC-

**TION OF ABSORPTION.** A controversy having been lately revived, whether absorption has any dependence on the nervous system, and more especially, whether absorption in the stomach depends upon the integrity of the pneumogastric nerves,—M. Bouley undertook an experimental inquiry into the subject at the Veterinary School of Alfort. He first found that the division of these nerves in the horse completely prevented the poisonous action of extract of nuxvomica. A dose of 494 grains, which killed in an hour and a half another horse not so prepared, had no effect whatever on the former in thirty-one hours,—although the contents then found in the stomach acted with violence on dogs. Nevertheless, it appears that this result is not at all owing to absorption being thus prevented in the stomach. M. Bouley afterwards ascertained, that a mere ligature on the pylorus has the same effect as division of the pneumogastric nerves. In a horse, with the nerves entire, and a ligature on the pylorus, retaining in the stomach the usual dose of nuxvomica extract, no effect whatever took place for eighteen hours. But when the ligature was then withdrawn, the animal, in the course of fifteen minutes more, perished in tetanic convulsions. Hence he came to the conclusion, that the cause of the non-action of the poison in the case of section of the pneumogastric nerves was merely arrestment of the muscular movements of the stomach, by which its contents are propelled into the intestines; that the lining membrane of the stomach cannot absorb; and that poisoning cannot take place till the poison reaches the duodenum.

*Obstetricus.* That must prove a convenient property for some stomachs, and accounts for some anomalous facts as to the influence of poisons on the horse. If he can keep them there till the other contents of his stomach decompose them, he is safe. But, does M. Bouley give a satisfactory explanation of this singular want of function?

*Editor.* Simply, that the inside of a horse's stomach is rather a skin than a villous coat. The dense impermeable "pavement epithelion" of the gullet, which gives place, at the cardiac orifice of the stomach in man, the dog, and many other animals, to the usual delicate, permeable epithelion of the mucous membranes,—extends in the horse to the middle of the stomach in the form of a dense tegument, as thick as the human cuticle, and probably even less penetrable. And in the pyloric half of the organ, the epithelion, though more delicate, vascular, and capable of secreting with activity, is still a "pavement epithelion," too dense to allow of any great absorption taking place. Thus, the stomach of the horse is a digesting organ entirely, and not an absorbing one. And it is probable that some other animals, especially among Ruminants, are similarly organised.

*Obstetricus.* Is there any printed record, from which one may form some idea what a Clinical Lecture was made of in the days of Dr John Rutherford? If not, a sad blank has been un-

accountably left in Scottish Archæology and British Medical Literature.

*Medicus.* There is none. But, by an extraordinary chance, it is not yet too late to fill up the blank. Here is a manuscript volume of apparently very exact notes of his Clinical Lectures in the Royal Infirmary in 1749, being the third session after he instituted the Course. The volume was lately presented to me by Mr Philp of Kirkaldy, who could not trace it, however, beyond its last possessor, and he was not a professional man. The notes are full, clear, and apparently exact,—so much so, that they may be printed *verbatim*. If you have any curiosity to know what the first parent of Clinical instruction in Britain said to his pupils,—your great-great-grandparents, perhaps,—I shall play the part of the old Trojan, and deliver to you his Second Lecture of January 19, 1749.

*Omnes.* By all means.

*Medicus.* But remember to forget 103 years of pathological and therapeutic discovery since that time. You cannot otherwise do justice to

A CLINICAL LECTURE IN THE MIDDLE OF THE LAST CENTURY. By-the-bye, his pupil has forgotten the exordium. Or perhaps, so downrightly straight-forward is the Professor's humour of prelection, that I should not wonder if he had none, but began his lecture abruptly thus, as I find it written :—

Ever since I heard of electricity, I had a notion that its aura might be of service in diseases of the head and nerves, as there would seem to be something analogous between the rapid motion of it and that of our nervous fluid. Upon which account, I design to-morrow to begin some experiments, to make tryal of its effects upon some of the patients in the house ; which you may depend shall be done as accurately as lyes in my power, to determine what may be the effects of it.

*A Mucous Catarrh.*—John Macdonald, aged 50. This man by trade is a smith, and was much exposed to heat and cold. After catching cold, he contracted a pain in his head and back, and a dry hard cough ; from which I take his disease to be a catarrh.

He was not very feverish. His disease is quite different from the *peripneumonia notha* ; which, as it is caused by a viscid matter obstructing the pulmonary artery, creates a greater oppression of breathing, anxiety, and stifling. Yet the cough is not so violent ; which, as it is but an effort of nature to remove something from the lungs, would avail nothing here, except it were to propel the viscous matter farther in the pulmonary artery by the concussion it causes.

The seat of catarrh I take to be in the villous mucous membranes lining the bronchiæ, or in the cells or glands,—viz., when a greater quantitie than usual ouzes out from the parts. This humour at first is very clear, soapy, and very viscid, whence proceeds the dryness of the cough, by which nothing can be brought up until this viscid liquor turns white and freer, and is what is called concocted. Neither have I observed in my whole practice any medicine which could produce

this concoction; which can only be effected by time, and lying for some time exposed to the heat of the heart.

If this liquor indeed be in great quantity, it may compress the pulmonary artery, and cause a peripneumony.

Macdonald's cough at first was dry. His lungs were oppressed, until by time, and the use of the *spiritus mindereri*, the matter was concocted; for he now spits up very freely, and his lungs are relieved. However, I must tell you, that nature did a great deal more than art, in carrying off his fever by a looseness, so that he had no fever when he came to the house.

Catching cold is nothing else than having perspiration stopt, either in our skin or lungs, by the contraction of the vessels by cold. That perspired by our lungs is equal to what is perspired by all the rest of our body. Nature casts out all the useless acrid humours in our bodies either by the skin or kidneys; so that, when any of these evacuations is stopt, a fever must be kindled both by the quality and quantity of this acrid humour retained within the body, except nature throws it off some other way—which it does not very often by the kidneys. Hence it is that the thick urine may commonly be observed in colds, if nature makes use of this way; but if nature does not make use of this way, they generally get gripes and a purging, by which this retained acrid humour is expelled. Hence this man's fever was lessened in proportion to his looseness; but his lungs were not relieved by it, as the matter was neither concocted nor expelled.

By it he has pain in his left side. But as he has no sign of any inflammation, I take it not to be an acute pleurisy; though I think it must be either some of the membranes—viz. the pleura, or that investing the lungs; or that it is a beginning adhesion between the two membranes, without which there are very few people in this part of the world who live to the age of forty or fifty. This at first gives some pain until the fibres are soupled and stretched; which may be his case. However, as there was no contra-indication, I ordered a blister to be applied to the part affected.

*Hæmoptoe*.—Archibald Wright, aged 25, about two years ago had a fever along with a cough. The fever lasted about three weeks, and was probably of the peripneumonick kind; but we have nobody that can give us a distinct account of it. This cough still sticks by him.

In a great many fevers there is a metastasis made to the capillary vessels of the lungs; for when the lentor is so far attenuated as to be pushed through the arteries, it passes into the veins. The first place it can cause inflammation at is the pulmonary artery; so that a person might be surprised that, as in most acute diseases, there is no metastasis made there; but this is prevented by the structure of these parts, whose arteries are very expansible and capacious, according to the pressure made upon them. By this extensibility of theirs, they allow gross humours to pass them, which may cause obstructions in other parts of the body. Hence it is that often pus absorbed from ulcers, after having passed the lungs, falls upon some of the abdominal viscera. But when this dilatibility in them is wanting, the liquor stops here; which seems to have been the present case. The fever had a crisis, which fell upon the pulmonary artery, and there caused obstruction; so that the whole mass of blood had to pass through a less number of canals.

Hence a greater friction and celerity ; so that, the momentum of the blood being increased, the vessels were dilated gradually, and at last ruptured, so as to produce the hæmoptoe under which he labours at present.

Although I have not examined him sufficiently to know whether he has a phthisis or not—however this readily follows an hæmoptoe, as the blood gets in among the cellular membranes, and there it stagnates, corrupts, and by its acrimony corrodes the neighbouring parts.

In the forenoon I ordered him to be blooded, which I take to be the principal remedy in such cases.

The blood moves in the pulmonary artery with greater celerity, and in greater quantity, than any other artery in the body. Yet people seldom die of the hemorrhage from a branch of the pulmonary artery bursting in this country,—though they do frequently in warm climates,—but from the consequence rather of the hemorrhage ; for after this disease, most commonly a *phthisis pulmonalis* happens.

I have ordered him a milk diet ; and if his belly be bound, he must have a laxative glyster.

[*Case continued, January 26.* He has had a tumour in some of the viscera, immediately under the *cartilago ensiformis*, which falls over to whatever side he lies on. Where this can be, I cannot determine,—whether stomach, colon, mesocolon, omentum, mesentery, or pancreas, though its being in the last seems most probable. He has been bled for his hæmoptoe, and uses a balsamic electuary and pectoral decoction. His hæmoptoe is gone ; but his cough remains.

The prognosis for him is not very favourable, as he had two diseases which give contrary indications. The country air, a milk diet, etc., would do best for his lungs, whereas the house would be very proper for dissolving that obstruction. The juice of some of the mild saponaceous plants, as the *Fumaria*, *Taraxacum*, *Beccabunga*, etc., would help to dissolve this glandular obstruction, by continuing in the use of them for some time. But that would bring back the hæmoptoe by dissolving the blood.—*January 29th.* He had six ounces of pus discharged from a vomica burst in his lungs. This I suspected all along, from a quickness in his pulse. I don't think anything can be done for him here ; so that, dismissing him to the country with proper directions will be the best method. He worked in the glass-house ; so that his lungs were probably much injured, before the distemper seized him, by the sudden changes of heat and cold, and the suffocating fumes that generally arise there.]

*Phthisis.*—John Williamson, aged —. This lad I take to be in a consumption. He contracted a catarrh by walking in a cold night. After a hard cough had remained a considerable time, he had a diarrhoea, which I take to have been symptomatic, and to have proceeded either from an error in the non-naturals, or a new cold. He had this when he came in here ; but now it is turned into a colliquative diarrhoea. He has a hectic fever, and sweats in the morning. Within these few days he has begun to spit up pus ; so that his prognostick must be very unfavourable. However, if it were a proper season of the year, and he was removed to the country, where he might enjoy fresh air, a milk diet, and exercise, he might have some chance ; but as circumstances stand at present, I own my having no hopes of recovering him.

There seems, from his breathlessness, to be a great many of the vessels of his lungs obstructed, which, compressing the pulmonary artery, have caused inflammation and consequent suppurations, which, upon this account, must be particular little ulcers in a great many different parts of the lungs. Which kind of suppuration I have often seen in dead bodies, though they never had spat up any pus; which was because no large branch of the bronchial vessels was ended by it.

This kind of phthisis is worse to cure than one proceeding from a large vomica in any particular lobe of the lungs; upon the breaking of which, we would have a great efflux of pus. But, then, it would be easier discharged and deterged than this kind,—the pus of which partial suppurations, a part being retained, becomes acrid, erodes, and produces inflammation and suppuration, and is resolved into the mass of the blood; where, by its acrimony, it stimulates the heart and vessels, and produces a fever, and lessens more efficaciously than any thing I know the crasis and coherence of the blood. Hence proceed colliquative sweats and diarrhoea.

There can be little done here on account of the contra-indications. He has little blood left already; so that his sweatings and diarrhoea should be first stopt; which if I should attempt by astringents, as the *Cortex Peruvianus*, which would effect this,—his expectoration would be stopt, and by oppression of his lungs he would be choaked. His expectoration is not very free at present, nor his breathing,—which if we were to mend with any of the pectoral gums, as ammoniac, etc., they would increase the diarrhoea,—and if by balsams, as copaiva, etc., these are so heating, that they would increase the fever. However, as something must be done, which will rather be palliative than any thing else, I design to give him the *Balsamum Lucatelli*; which, as well as its balsamick virtue, has somewhat astringent in it. But, as I fear it will run to the gutts, it shall be in small quantity, viz., *R. Balsami Lucatelli semunciam, Conservæ Rosarum rubrarum unciam: misce. capiat magnitudinem nucis moschatæ bis in die.*—There is another medicine, which, though much abused, might be of service to him. I mean tarr-water; of which let him take a gill [four ounces] twice or thrice a-day, as his stomach will bear it. This will increase expectoration, will not increase his diarrhoea, and, as it contains the acid of the tarr, will be of service, and likewise, containing the medicinal part of the tarr in so much water, will go a great way through the blood.—In place of his Paregoric at bed-time, or Laudanum, which hinders expectoration, I shall order a form which I have always found not to be so heating or drying, and consequently not to stop expectoration, though at the same time it produces all the good effects of the opium. *R. Caputum Papaveris albi unciam; coque leni igne ex Aquæ Fontis libris duabus ad libram; sub finem coctionis adde Radicis Iridis Florentinæ semunciam; coletur, et adde Syrupi Diacodii unciam.* Let him take three ounces or more, according to the laudanum he got. I don't pretend to say that this is quite free of the bad effects of opium,—only that it contains them in a less degree. The poppy-heads require long boiling. The Iris I have added, and the Syrup, because they make it more pleasant, and promote expectoration.

[Continued. Jan. 22.—He is dead sooner than I expected, though I had no hopes of his recovery. His dying so soon however I impute to mismanagement wholly. Last when I left you, as I went through the ward, I saw him at stool,



quite naked, with his bare feet upon the cold stones. He had the colliquative diarrhoea and sweat ; so that he could not miss to catch cold, as the patients told me he had done the same thing upwards of a dozen times that day. This cold stopt his expectoration.]

*Dropsy.*—Thomas Fergusson, aged 46. Served a brewer in this town, and had a good state of health until July last, when he and two of his comrades fed upon salmon, which disagreed with them. The other two it purged, and they recovered; but it remained in his stomach and congealed, as he says, like a stone. After this he had frequent pains and thirst, and drank a great deal, which he never passed. But he continued at his work till, overheating himself one evening, he fell asleep in a cold house, and when he awakened, he found his legs swelled. They had begun to swell before now, but they were not near so great. This swelling proceeded gradually, until now he labours under an universal dropsy,—none of the cavities being free except those of the brain.

This effect of the salmon I observed last year in a girl, viz., purging and vomiting; which I impute to their way of carrying the salmon, viz., in matts wherein tobacco was carried,—which we know not only to have those effects, when taken inwardly, but likewise those of poysonous gripes, cholics, inflammations, etc. It was remarkable in this girl's case, that it was only such as eat the parts near the surface of the salmon were affected.

This man's dropsy I take to proceed from a quantity of cold drink he took, and which he never passed either by stool or urine. This never mixes with our blood rightly, and so passes easily off by the lateral vessels into the cavities and cells of the *membrana adiposa*, where, by macerating the vessels which open there, the disease is increased. The perspiration, which was stopped, would go off the same way, and have the same effect.

He has the common symptoms of a cough, with which he gets nothing up; which proceeds from the water pressing the lungs. His pulse will strike two or three times regular enow; then four or five times so fast, that you will not be able to count them, they are so fast and quick; in short, it is like a dog's pulse. This I imagine proceeds from a *Hydrops pericardii*, by which the motion of the heart is impeded.

He has a continual vomiting, which I take to proceed from the same cause, viz., the weight of the pericardium irritating the diaphragm by the cold or weight; for he has neither pain in his stomach, nor any other symptom of that viscus being affected; so that, unless it be this, or any other irritation on some of the muscles of the abdomen, I don't know what it may be. His thirst is caused by the thinner parts being carried away. This I take to be unfavourable for the patient, as he has gripes, which by Hippocrates are reckoned to be a fatal symptom in dropsies, and are often observed to attend scirrhus or obstructed viscera.

When he came here he got some purges of jalap and mercury, which always relieved him. However, in the main he has all along been turning worse. But I never reckon a case desperate until all remedies are tried without producing any effect. He got a few of the mercurial pills, which were intermitted as he turned worse. But this could not be imputed to them, as he got but a few of them, and his vomiting continued after they were intermitted; for which he got

opium, and was not better ; and then the theriac, afterwards the strong nervous medicines, as assafoetida and castor, but all without any effect,—which still convinced me the more as to my former opinion of the cause of it. I therefore had recourse to speedier remedies than mercury.

In this case I gave him a vomit, which, by its shock, might promote the absorption ; and next morning I gave him a hydragogue purge of *Tinctura Jallapæ* one drachm, and *Syrupus de Rhamno* an ounce. I gave it him in a liquid form, as it would operate sooner.

In obstructions of the liver, Jallap, in substance, never answers so well ; for its resinous parts, without the help of the bile, can never be dissolved ; so that it will not operate so well. I don't know, however, if this be his case. The tincture of Jallap suspends the parts of this root in an ardent spirit ; which, upon the mixture of any watery liquid, will precipitate it. If this happened in the stomach, it would form flakes, tough and tenacious to the sides of the stomach ; which would create gripes, etc. But, being mixed with the saponaceous liquor,—which sugar, and consequently syrups, are,—this inconveniency is prevented. Rubbing a little powder of sugar with jallap, or mixing a little sugar with jallap as you are powdering it, will prevent its griping. The proportion of sugar may be one ounce to three ounces of jallap.

His vomiting is abated, and he got a Paregoric at bed-time, and shall have a hydragogue to-morrow ; for in hydropical cases they must be given soon after another, otherwise you can gain no ground.

[Continued. February 2. Since the 29th of January he has got a jaundice ; which is worse than if it had appeared before, as it shows it to proceed from a worse cause. He has a dropsy, with all the bad symptoms which can attend it. Hydragogues answered well at first, but they failed afterwards. Then I tried emetics, which did not answer either. For the vomiting he was so much troubled with, I gave him a little dose of opium. For his drought I ordered him tamarinds in his mouth ; and he had a strong diuretic decoction, which produced no effect ; and in place of the hydragogue he has got a ptisan of senna and tamarinds with *Syrupus de Rhamno*, by which he has found himself much relieved. After that, at bed-time, he gets three grains of opium, otherwise his belly would be as much swelled by flatulence after the water as before the purging. The ptisan brought away a great deal of water.—February 9. Neither vomits, purges, hydragogues, nor diuretics have produced any great change upon him. I dare not tapp him for fear of bringing on a mortification in the intestines, some of which are probably obstructed. The pressure made upon them by the water keeps up a kind of an equable circulation in them. They are much weakened by maceration ; so that if this pressure was removed, the blood would rush into their relaxed vessels, there stagnate, corrupt, and produce very great evils. Therefore, yesterday, I ordered him a very strong diuretic decoction of *Spiritus Mindereri*.

*Dropsy*.—Robert Hunter, aged 19. This boy has a universal dropsy. He was a postilion ; and having little cloaths to cover him, went a journey of seventy miles to the west in rainy weather, and was obliged to be in wet cloaths all night. Before he came to town, his legs began to swell, afterwards his belly, etc. : and his disease proceeds from an obstructed perspiration. By the circu-

lation being lowered, the serous parts of the blood were easily separated from the rest.

As from an uncommon oppression at his breathing, and a very frequent cough, I conclude his having the *Hydrops pectoris*,—for a slight degree of both these may be caused by an *Ascites*.—I tried him with emetics, as they are so much recommended by Sydenham in this disease, and said to evacuate both upwards and downwards. But I never found them to answer in any case more than in this, where they were of no service. Then I tried Hydragogues, which did very well, until unluckily an *Hæmoptoë* obliged me to give them up as too acrid and stimulating in his case. As this was abated, I gave him a purgative ptisan yesterday, by which he thought himself relieved much. It was of Tamarinds, Senna, and Glauber's salts. After it he gets 2½ grains of opium.

His penis is swelled to a monstrous size, that it has burst in some places, by which the water, in very small quantities, ouzes out; but the swelling is not lessened by it, and he has an excessive pain by the existence of it. I could easily in a person that was dropsical let out the water by puncture, or incision, or a seaton. But in such a case the wound would certainly not suppurate, but mortify; which would very soon spread. Therefore I ordered him a decoction of Scordium, Rue, and Wormwood, with a little Camphorated Spirit of Wine to foment it with; and not being the better of this, I caused him to foment it with *Spiritus vini camphoratus* alone; but this gave him great pain, and made some of the bursted parts bleed. I intended to have strengthened the fibres by this, and repell the stagnating lymph, but was obliged to desist, and apply a poultice of lint-seed made with a decoction of the following (?) plants, by which he finds himself easier, although the swift method would be by purgatives, if he could bear them.

The best thing about him is probably that he has no obstruction in any of the viscera, although indeed we can't be certain of that as long as his belly continues so much swelled. However, we can expect to be of little service to him, as he has not strength to mix the separated parts of his blood, although he were freed of his present load. Besides, this is one of the worst diseases to prevent a relapse in the vessels that have been so long macerated in warm water.

[Continued. Feb. 9. His medicines have very little effect on him. His penis is now excoriated, and in great danger of mortification; to prevent which I ordered the bark in decoction, as lighter this way than any other. He is an ill-natured wilful boy, so that I can neither get him to take medicines nor care of himself. As he had no cloaths, I got him a flannel shirt, than which I know nothing better or more useful in leucophlegmatics, as they promote sweat much. Yet I never see him but naked. The pressure on his lungs still continues the *Hæmoptoë*; so that he must either use acrid purgatives or diuretics. His pulse is weaker, his strength is wasted, which by Hippocrates was observed always to precede the patient's death, when coming on towards the latter end of the disease;—and reasonably, as it proceeds from a laxity of the vessels allowing a greater quantity of liquid to pass through them.—Feb 16. He is dead, according to our prognosis. Some days before his death, he had a delirium and stupor, caused by the disease seizing his head, and water gathering in the ventricles of

the brain. The liver was found scirrhus, likewise the pancreas. The gall-bladder was distended; neither could any bile be got out by pressure. The duct was surrounded with a steatomatous kind of substance, by the pressure of which, and of the liver, the passage through the duct was in a manner concreted. Hence proceeded his jaundice some time before his death,—viz., from these obstructions. By the vomits he took his heart was as large as any ox's; so that it seemed so high that it caused a kind of trembling in the jugular veins, which some of you took for a pulsation of these veins; and indeed it is very like that, but was only caused by the pulsation of those neighbouring parts. The auricles and ventricles were found very large; and the entry of the left auricle into the left ventricle was found very much shortened by a kind of long excrescences surrounding it. And hence another cause of his dyspnoea.]

*Obstetricus.* No one can doubt that *this* lecture was delivered,—and to students,—and with a single eye to their instruction. The Professor's frank confessions of embarrassment in his diagnosis,—his perplexity in the choice of treatment from contradictory indications,—his lamentation that "neither vomits, purges, hydragogues, diuretics," nor any thing else, did any good to Thomas Fergusson,—his disgust that John Williamson should die within three days after the belief expressed, that "he might have some chance with a milk diet, fresh air, and exercise,"—his consolatory discovery that the lad's death was owing to his having sat upwards of a dozen times in one day, stark naked, on a close-stool,—the mortifying pathological display of Robert Hunter's scirrhus liver, diseased mitral valve, and "heart as large as any ox's," after the Professor's assurance that there was "probably no obstruction in any of the viscera,"—these are genuine clinical incidents, proving the genuineness of the lecture and the sincerity of the lecturer.

*Chirurgus.* And widely different from what you find in some so-called Clinical Lectures in the Weeklies; where everything turns up as the lecturer prophesies, and conveniently when he desires it.

*Medicus.* There is a sort of clinical essence even in its somewhat rambling fashion. And no one can fail to see in it many valuable practical precepts for students of that day.

*Chirurgus.* An old and esteemed Professor and friend of ours used to say, that it is a good lecture which contains a dozen good points; and that no lecture ought to contain more, because no student could carry off more. Try Dr Rutherford's lecture by that test.

*Medicus.* There is, 1. The advice never to despond so long as any remedies remain untried. 2. The doctrine that, in bronchitis, the fever may cease, and yet the local inflammation continue as before. 3. That the fever of acute bronchitis having ceased, an easy expectoration cannot be forced on by remedies, but should be left to nature, time, and the heat of the heart. 4. That when hæmoptysis once occurs, it is a difficult matter to make sure in any case that

phthisis is not present. 5. That, alarming though hæmoptysis may seem, patients in this country seldom die of the hemorrhage, but of the consequences,—that is, according to modern pathology, the consequences of its cause. 6. That the frequent expectoration of small quantities of pus is generally of worse augury than a single discharge of a large quantity. 7. That pleuritic adhesions are seldom wanting in people who reach middle age. 8. That salmon sometimes acts after the manner of an acrid poison,—though he mistakes the cause. 9. That when hydragogue cathartics are given to evacuate dropsies, they are of little use unless given frequently, with brief intervals. 10. That incisions into parts excessively distended by anasarca are apt to cause sloughing. 11. That sugar tends greatly to prevent the griping caused by some acrid cathartics, such as jalap. And 12. He might have wound up his pathological discoveries in the dead body of Robert Hunter, with a corollary on the uncertainty of physic.

*Chemicus.* Of human knowledge, if you please, but not of physic, in particular. It is incomprehensible to me that medicine should have been so often singled out for uncertainty, when we have the uncertainty of the law constantly staring us in the face,—or of politics,—or of war,—or of almost any other science except the pure sciences. But in these days medicine is actually made to bear even the burden which belongs to others,—to law, for example.

*Medicus.* I thank you for the correction. It is clear to me, for instance, that many anomalies and absurdities in the practical working of the law in regard to insanity are imputed to medicine, which are really owing to the state of the law, and the partial views of lawyers.

*Chemicus.* Let me give a late specimen of

**THE UNCERTAINTY OF THE LAW.**—The other day, two prisoners escaped from the hands of justice in a most extraordinary manner; and the public voice ascribes their escape to a medico-legal error, when, in truth, they owe it purely to legal uncertainties.

*Editor.* Was there not some flaw in the medical evidence?

*Chemicus.* Not at all,—not there, wherever it was. An old man in Ross-shire was suspected to have been poisoned with arsenic by his wife and son. They were brought to trial for the crime at last Inverness Circuit. There was no question as to the cause of the man's death. The stomach, liver, etc., were all full of arsenic. There was as little question as to the moral and general evidence. There was a correspondence between the prisoners, in which the son encouraged the mother to become a widow as soon as possible, and the mother said she would get on well in the world if she were one; and these letters were seized by the authorities, and produced in evidence against them. Then there was the opportunity of administering poison to the old man. And, to conclude,

both of them were proved to have purchased arsenic, the poison of which he died.

*Chirurgus.* Upon what plea, then, did they get off? I should have thought that, with such a mass of evidence against them, they would hardly have got a verdict of Not Guilty.

*Chemicus.* Neither did they. They had a verdict of Guilty returned against them by the jury.

*Chirurgus.* And yet I saw by the newspapers that they were let off the other day scot free—"dismissed simpliciter" from the bar of the High Court of Justiciary. How came that to pass?

*Chemicus.* It is a most remarkable story. You will please to bear in mind that in all criminal trials in Scotland, the law requires that the prisoner shall be furnished, some days before trial, with an accurate list of all the persons, papers, or articles, which are to be produced in evidence against him. The purpose of this, of course, is to enable him to conduct his defence. If he can discover any inaccuracy in this list, or "inventory of productions," he takes an objection to it, so as to set aside the disputed article or witness, and so get rid of all the relative evidence. In the present instance an objection of this sort was taken, and proved the groundwork of all the subsequent proceedings in the case. I told you that both prisoners had been proved to have purchased poison. It was of course essential, in order to connect either of them with the death of the old man from arsenic, to prove that this was the poison which they had purchased. There was no dispute about this as regarded the woman. She purchased, or at least got another woman under her directions to purchase, arsenic itself. But the poison which the son had bought, was an article sold in packets, under the name of "Butler's Gloucestershire Vermin-Killer;" and, in order to ascertain what it was, a packet similar to that purchased by the male prisoner, was procured from the same shop, and sent for analysis to the chemical expert employed in the case.

*Medicus.* And that turned out to be arsenic, sold under so imposing a name?

*Chemicus.* Exactly so.—*Arsenicum fuliginosum*; arsenic, coloured with soot, according to Lord Carlisle's Bill.

*Editor.* But what was the objection to all this? Does the soot act as a counter-poison? Charcoal does, if there be plenty of it.

*Chemicus.* The objection lay in a very different quarter. In order to preserve the identity of the packet of poison, purchased and sent to Edinburgh for analysis, it was sealed with two official seals, and marked on the printed wrapper by the signatures of the officers who procured it. The chemical witness to whom it was sent, in opening it to get at the contents, left the seals entire, according to rule and custom, and got into the packet by cutting the paper round them. Having made his analysis, he returned the packet to the Crown authorities, for the purpose of having it produced in evidence. Of course this packet had to be notified on the list of productions to be



brought in evidence against the prisoner; and in the list it was described as "a sealed paper wrapper, or other piece of paper, containing a small quantity of powder, the paper being dated on the one side, 'Tain, 19th November 1851,' and subscribed 'Thomas Flint, H. M. Taylor, Geo. T. Munro.'"

*Editor.* One would think there could be no dispute about the identity of that packet.

*Chemicus.* You never were more mistaken in your life. A prisoner's counsel will dispute anything. Accordingly it was argued that the packet produced in court was not that described in the indictment, inasmuch as it was not "a sealed packet," but an opened packet with seals on it. And of course it was contended upon this ground, that neither the packet itself, nor the chemical report as to its contents, was admissible as evidence.

*Editor.* Was such an objection listened to?

*Chemicus.* Yes, listened to; because it is imperative, in all criminal proceedings, that the forms of law shall be closely complied with; and no objection is more fatal than a clear deviation from them. The objection therefore was listened to, but it was not sustained. The court held that the description was sufficient to establish the identity of the packet, and the evidence connected with it was received. The trial then went on, and the jury found both prisoners guilty.

*Editor.* Then why were they not at once sentenced?

*Chemicus.* The Circuit Judges thought it their duty, when the lives of two prisoners were involved, and an objection had been taken to the admission of one very important piece of evidence, to report the case to the High Court of Justiciary at Edinburgh, for the opinion of the other judges.

*Medicus.* Was it supposed that the High Court might sustain this objection?

*Chemicus.* No. I should hardly think the objection would have been sustained. But that we can never now know; for the High Court never got so far forward. The case took quite another turn. In remitting it for final adjudication by the High Court, it was merely ordered that the prisoners should be brought up for a final judgment there; but no particular day was fixed for proceeding against them. Accordingly, when they came up before the High Court for trial, it was objected by their counsel, that as no day had been fixed upon, and intimated to the prisoners, that—

*Editor.* What? Another objection? Surely it was time enough to fix a day, when it could be known that the High Court would be ready to try them.

*Chemicus.* Not at all; and the reason for this is an excellent one. In Scotland, as you know, all criminal prosecutions are conducted in the Sovereign's name by the Lord Advocate, whose business it is to bring to justice all offenders against the laws. Now, unless the law afforded ample protection, a functionary exercising such authority might convert his power of prosecuting into one of persecuting. It

is theoretically, though scarce practically possible, that an arbitrary Lord Advocate might accuse a person of a crime, refuse to bring him to trial for it, and keep him in prison for an indefinite period, as King Bomba is said to do with his political enemies at Naples. Now, in order to prevent this, the law of Scotland, in the first place, enables a prisoner to force on his trial if he chooses, and secondly it enjoins that all criminal proceedings shall be "peremptory,"—that is, that a day shall be fixed for the trial, and duly intimated to the prisoner; and if no proceedings are taken on this day, the case is held to be at an end. The argument therefore now was, that as in the remit from the Circuit to the High Court, no day had been fixed for the trial, the case had died a natural death, and was no longer valid against the prisoners; and this objection was found good, but only by the deciding voice of the Lord Justice-General, the other Judges on the Bench being equally divided in opinion—three to three.

*Chirurgus.* And thereupon I suppose these people were let loose.

*Chemicus.* No, not yet. The Crown lawyers made another attempt to bring the law to bear upon them, and petitioned the Court to have them tried over again; but now another difficulty was encountered. It is also a principle in criminal law, that no person can be tried twice for the same criminal act; and therefore it was argued for the prisoners, that having already sustained the verdict of a jury, or, as the Scotch phrase is, "tholed an assize," they could not be again indicted on the same charge; and the Court unanimously found this objection relevant. And so at length they were set at liberty.

*Obstetricus.* That seems to be a true case of miscarriage of justice.

*Medicus.* True. Themis was in want of Lucina; but after all, though it is to be regretted that two prisoners found guilty by a jury should escape punishment, it is at least well to know that it was not from any defect in the medical evidence; and that, even in so peculiar a case, the law, as it stands, is administered impartially.

*Obstetricus*—[*To Chemicus*]. I think you said that part of the arsenic used on this occasion was sold in accordance with Lord Carlisle's Bill. Does that act answer its purpose?

*Chemicus.* Very imperfectly. The permission to use soot to colour arsenic renders the act futile. All that is required to be added amounts only to six per cent.; and as half a drachm of arsenic, which is more than enough to give any victim his quietus, contains thus little more than two grains of soot, it can easily be mixed with anybody's porridge without being perceptible to the senses. Moreover, as soot may at any time accidentally fall into a pot on a fire, it would, even if tested, hardly attract attention. The indigo, which the Sale of Arsenic Prevention Act also allows to be used to colour arsenic, is not open to this objection. As the colour is powerful, and none of our articles of food are blue, a very small quantity may be observed.

*Editor.* A blind person might be poisoned with such arsenic, how-

ever. Would it not be well to put in something which should have odour as well as colour?

*Chemicus.* I have always thought that if we are to follow this plan of preventing the sale of arsenic for felonious purposes, by giving it peculiar sensible properties, it ought to be mixed not only with indigo, but with oil of anise, or with oil of rhodium, especially as these oils are said to be attractive to rats, which are generally made the excuse for purchasing arsenic.

*Editor.* What is oil of rhodium?

*Chemicus.* Oil of rhodium is got by distillation from a climbing shrub of the Canaries, which at one time was supposed to be a kind of broom (*Genista canariensis*); but it appears to be a convolvulus (*Convolvulus scoparius*). The woody rhizome is known in France by the name of Bois de Rhodes, and in commerce as Lignum rhodium; but it has nothing to do with the Isle of Rhodes, and only means Rosewood, or rather Rosy wood, from its smell, and of course has no connection with the Rose wood of English commerce, which is used for making furniture. A very few drops would flavour a considerable amount of arsenic.

*Medicus.* It is most attractive to rats, and; except on account of its high price, would be a most important article in

THE ART AND SCIENCE OF RAT-CATCHING. You know there are two ways of getting rid of rats—by poisoning them, and by taking them alive. The great secret, in either way, is to collect them all together, and extirpate the entire colony by a *coup d'état*. It is easy to accomplish this by poisoning them. But unluckily a rat, like a human being, has a fancy for dying in his bed—makes for his hole when sick, and, dying there, he becomes a greater pest to society dead than living. Where his odour for a month or two don't signify, however, here is the way to proceed: Feed the colony for five nights with porridge made with roasted oatmeal, putting all other food out of the way, and, on the sixth night, mix arsenic with the porridge. Not a rat will be seen alive again. I have thrice recommended this cheap and simple plan to my friends, and always to complete extermination.

*Chirurgus.* I for one have to thank you for the hint. My colony got a supper of porridge—for a fortnight through an oversight. The dish was licked quite clean every night. The gardener then spiced the dish with a whole pound of arsenic. That night half the porridge was left; but not a rat has been seen since.

*Medicus.* Of course this plan is no novelty; and yet I have no doubt it is at least equal to any of the new-fangled secrets and patents for the same purpose,—such as phosphorus in fats. Phosphorus is said to have the advantage, that it may be safely put in stables and cow-houses, and even along the back of mangers, without removing the lawful occupants, because they will not touch fatty substances. But this goes on the supposition that rats can be extirpated

in detail. You may wage perpetual war with them in that way, but scarcely to their extermination; as undoubtedly they are rendered wary by previous deaths in the colony. The other way is to attract them to a great trap by some odoriferous substance, such as oil of rhodium or anise. But I shall tell you a third way—a secret I got from an experienced rat-catcher in the north, who has been dead for some years. He consulted me for diseased heart, and a new rat-poison. He informed me there could be no doubt that rats might be taken alive in great numbers by anise-oil, but, above all, by oil of rhodium, which has an extraordinary attraction for them, but is too expensive for so humble a profession as his was. He did not quite approve of my arsenical plan; and I suspect his reason was not so much its inefficacy, as the utter annihilation of the animals by which he won his bread. But he told me he had tried many odoriferous substances, poisonous and otherwise, and had found that the common prussic acid was the most attractive of all,—that he had watched the creatures crowding, and squeezing, and running over one another's backs to get at it,—and that *they never reached their holes again*. His only objection was its volatility, on account of its price; for he knew no other mode of purchase than by obtaining it from a London druggist at the cost of a shilling an ounce; and in trying to make it in a small out-house, this being his first essay in the chemical manufactures, he narrowly escaped dying, like his subjects, in his hole. I told him how he might safely make it for a few pence a pound; and, as fee, I got a promise of as many live rats as I chose, for my “philosophical experiments.”

*Chemicus*. Returning to the anti-arsenical act, in the first place, I have no doubt that a very few drops of oil of rhodium would sufficiently flavour a considerable amount of arsenic. It has always appeared to me, however, that too much confidence has been placed in these flavourings; and that the other and more important provisions of the act for tracing the purchasing party, and letting him understand that he is marked, are very insufficient.

But,—there is another thunder-storm coming over the Pentlands:—Let us take up the Act another day:—*Sauvons-nous*.

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## Part Third.

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### CLINICAL REPORTS, LECTURES, ETC.

#### CLINICAL MEDICINE.—PROFESSOR BENNETT.

##### DISEASES OF THE LIVER.

Notwithstanding the obscurity which still rests upon the functions of the liver, the progress of histological pathology has tended to make us better acquainted with the minute changes which occur in many diseases of the

organ. The nature of fatty enlargement, of cirrhosis, and of the disintegration of cell-texture following obstruction of the bile-ducts, is now understood. Doubtless much research is still necessary, and more especially a careful comparison of the structural changes observed in the liver after death, with the clinical history and observation of symptoms during life, is now what is greatly desired to advance our knowledge of hepatic diseases. This, however, can scarcely be anticipated, until medical practitioners, and especially such as practise in the East, become efficient histologists. It is the therapeutics applicable to these diseases, however, and a correct appreciation of the class of remedies called cholagogues, which in the present state of medicine so much requires to be determined. Such an investigation necessitates physiological, histological, and chemical knowledge, added to good powers of clinical observation. But of all the subjects of research now open to the young investigator, I know of none in which patience and exactitude, based on a rational rather than a traditional system of inquiry, is likely to yield more useful results.

**CASE I.—*Impaction of Gall-Stone in the Common Bile-Duct ; Jaundice ; Death.***

Mary Duncan, æt. 36, married, admitted November 24, 1851. She has lived in India some time, and returned only a few months since. Three weeks ago, when recovering from a severe attack of lumbago, she experienced great pain in the epigastric and right hypochondriac regions. This was ushered in by rigors and feverishness, and lasted three or four days. Its severity then diminished ; but jaundice appeared, which has since become more intense. On admission, the whole integumentary surface presents a deep yellow colour. Pulse 100, full. The tongue is dry, with a dark-brown coat. There is a disagreeable taste in the mouth, impairment of appetite, but no nausea or sickness. The liver on percussion presents the normal dulness of four inches on the right side. Pressure in the neighbourhood of the gall bladder elicits pain, and there is permanent soreness diffused over the anterior portion of the liver and epigastric region. The pain is not spasmodic in its character, or more severe at one time than at another. The bowels are generally costive ; skin hot and dry ; urine like porter, staining linen yellow, and becoming green and then red on the addition of nitric acid. The abdomen is enlarged. She has had a child previously, and says she is now six or seven months pregnant. The treatment consisted of purgatives (*Pil. Rhei. c.*) ; leeches to the tender spot over the liver, and diuretics of acetate of potass and *sp. æther. nit.* December 3d.—The bowels have been kept open by purgatives, and the stools have been well coloured with bile. Leeches have been applied twice, and the hepatic pain has been much relieved. She has also been taking small doses of tartrate of antimony, and muriate of morphia. The skin, however, continues dry, and is now more deeply tinged yellow than on her admission. The urine also is still loaded with bile. To-day vomiting came on, and she rejects her food and medicine. The lips and teeth are covered with sordes, and she complains of great languor and depression. *Diuretics to be continued ; a blister to the right hypochondriac region ; and a powder, containing four grains of calomel and one-third of a grain of opium, to be taken every hour for six doses.* The mercury produced no physiological action, although continued in smaller doses and at longer intervals for several days, assisted by mercurial frictions over the right hypochondrium. December 11th.—There was slight diarrhoea, which was checked by an aromatic cretaceous mixture. Mercurials were suspended. On the 13th she was evidently worse ; the skin assumed a greenish hue ; she is very feeble, and passes her stools in bed ; pulse 120, small. On the 15th, the skin assumed a tawny green colour ; the stools are passed in bed, are of a dark colour, and of a very offensive cadaveric smell ; great prostration of strength ; urine still loaded with bile ; low delirium at night. Died on the 18th.



*Sectio Cadaveris.*—Thoracic organs healthy. On opening the abdomen, bands of recently-exuded lymph are found firmly uniting together the peritoneal surfaces of the gall-bladder, anterior margin of the liver, and a portion of the omentum, over an extent the size of the palm of the hand. In separating these adhesions, the gall-bladder and omentum were found so firmly united, that an aperture was formed in the former the size of a pea, through which a quantity of dark-green bile escaped. The liver was of its normal size, and presented externally a dark olive-green colour. On cutting into its substance, the gall-ducts were everywhere dilated and thickened. Some were distended into elongated cavities, above half an inch in caliber, and they were all filled with thick dark-green bile. The tissue of the liver throughout was unusually soft, readily breaking down under the fingers, and uniformly of the same olive-green colour as the external surface. In the common bile-duct, about half an inch from its duodenal extremity, a hard light-yellow gall-stone, the size of a small hazel nut, was firmly impacted, the duct both above and below being somewhat thickened and dilated. No other gall-stones could be anywhere discovered. The uterus and rectum were adherent, and in separating them about a tea-spoonful of yellow pus escaped. The vagina was shortened and constricted about two inches from the vulva, so as scarcely to admit a common quill. About an inch in the vagina, on its inferior wall, was a round aperture, the size of a shilling piece, and surrounded by ragged edges, which communicated with the rectum. And on the superior wall of the vagina, about half an inch from the clitoris, was another rounded opening, about the size of a sixpenny piece, into which the point of the little finger could be passed and pushed into the bladder. The natural meatus urinarius was occluded.

*Microscopic Examination.*—On crushing a small piece of the liver between glasses, and examining it under a power of 250 diameters linear, it was found to consist of a multitude of fatty molecules and granules, with larger globules of loose oil. Many of the cells seemed to be broken down and disintegrated, but such as were entire were more or less distended with bile pigment.

*Commentary.*—The symptoms present in this case on admission were indicative of obstruction in the common bile-duct connected with some inflammatory action going on in the liver or its neighbourhood, as indicated by the local pain, the rigors, and fever. Hence an antiphlogistic treatment, with the topical application of leeches, was adopted. As the blood and urine were evidently loaded with bile, diuretics and purgatives were also ordered to assist the excretion of that product. These remedies proving of no avail, and the constitutional symptoms increasing, mercury, conjoined with opium, was actively administered, but failed to produce its physiological or any useful therapeutical result. After death, peritonitis surrounding the gall-bladder and common duct was discovered; but she evidently died from the system being poisoned from the absorption of bile, the excretion of which was prevented by the firm impaction of a calculus in the common bile duct. The benefit of mercury in such cases, though strongly recommended as a means of altering the constitution of the bile, appears to me very doubtful; for, supposing it to possess the effect ascribed to it, and to act as an alterative and cholagogue, its action in obstruction of the gall-ducts must be to distend them still further, and thus increase the pressure on the hepatic cells, and consequently the disintegration of the hepatic texture. Most of the examining class were in favour of the trial of mercury in this case; and, considering how uniformly it is recommended by the most experienced practitioners, I did not think it right to deprive the patient of any chance which might arise from the use of this popular drug. At the time, I expressed my want of confidence in its virtues, an opinion the progress of the case fully justified. In the present state of the science and art of medicine, there is no one point in therapeutics which so urgently requires thorough re-investigation as the real value of the medical properties attributed to mercury.

The effect of the long-continued over-distension of the gall-ducts on the liver, was a partial disintegration of its cell elements, and an accumulation of



bile in such of the cells as remained perfect. This lesion is remarkably well described by Dr Budd, in the third chapter of his work on the liver, when treating of fatal jaundice. It admits of question, how far this destruction of the hepatic cells may not, by impeding the secreting power of the organ, at length induce that condition described by Dr Alison, where the biliary principles are not eliminated. It must, I think, be certain that jaundice, produced primarily, as in the present case, by a mechanical obstruction, must be kept up by the altered condition of the cell-structure afterwards induced.

This case was instructive to all who observed it, with regard to a supposed pregnancy she laboured under. The abdomen was certainly somewhat prominent; but the investigation of the existence of this state was never gone into, for the simple reason, that it in no way affected the diagnosis or treatment. When the woman was dying, however, the husband applied to me, with a view of ascertaining whether it might not be possible to save the child. On this point I requested the opinion of Dr Simpson, who, on examining the woman, declared her not to be pregnant. This circumstance, then, is an illustration of how women who have previously had children may be deceived as to the existence of a subsequent pregnancy, and how important it is for the practitioner, as a general rule, to satisfy himself of the reality or falsity of such a state in all cases. When formerly delivered in India, she said instruments were employed, and that she sustained some injury. This account is rendered highly probable, by the existence of the recto-vaginal and urethro-vaginal fistulæ, and the remarkable vaginal stricture, found after death.

*CASE II.—Hypertrophy of Liver and Spleen; Jaundice; Dilatation of Arch of Aorta.*

James Kerr, æt. 28, a labourer. Admitted July 22, 1852. This man first had jaundice, with swelling of the abdomen, between four and five years ago; and since then he has been several times in the Infirmary. The treatment has consisted of various alteratives internally, with the occasional application of the Tr. Iodinii, blisters, and leeches externally. The swelling, however, has continued to increase very slowly, and for the last two years he has been incapacitated from working. The blood for some time has contained an excess of fibrin, and a slight increase of the colourless corpuscles; and he has been troubled at intervals with attacks of epistaxis and occasional diarrhoea. For two years past there has been an increased impulse, with a rough blowing murmur, loudest under the manubrium of the sternum. On admission the hepatic dulness commences an inch below the right nipple, and extends down to the umbilicus, measuring twelve inches vertically. The splenic dulness reaches from the lower margin of the fifth rib to a transverse line drawn an inch below the umbilicus, and measures two inches vertically. The liver presents a protuberance anteriorly, which extends in the form of a ridge, four inches broad, from the epigastrium backwards towards the false ribs. The girth of the abdomen over this ridge is 42 inches. The inferior borders of the spleen and liver can be distinctly felt through the parietes of the abdomen. The heart's action and sounds are normal. An increased pulsation is distinctly visible at the root of the neck, and can be felt above the clavicles and under the top of the sternum. Here there is a loud rough murmur synchronous with the systole of the heart, and accompanied by a distinct impulse. There is slight cough, with scanty mucous expectoration, but auscultation and percussion of the lungs reveal nothing abnormal. Urine rather scanty, high coloured, spec. grav. 1026, contains some biliary matter, and deposits on cooling a pretty copious pinkish sediment of lithates. The integuments and conjunctivæ are still tinged of a light bile yellow colour. There is considerable tenderness over the liver in the right hypochondrium. He says slight epistaxis returns about once a week. The bowels are open daily; no diarrhoea. After walking or unusual exertion œdema of the legs comes on. On examining the blood microscopically, the colourless corpuscles are not so numerous as when he was last in the house. *Four leeches to be applied to the right hypochondrium.*

*Commentary.*—The lesion which I presume to exist in the liver of this man is a simple hypertrophy, a disease frequently associated with enlarged spleen. This hypertrophy must occasion some obstruction to the perfect excretion of bile, as the jaundice, though slight, has been uniform for more than four years. There has also been epistaxis and frequent diarrhoea, symptoms very common in connection with enlarged liver and spleen, although the *rationale* of their production is by no means obvious. He suffers no other inconvenience from the abdominal swelling, except from its bulk and occasional tenderness, unless indeed dyspnoea be also considered, which may partly be connected with the aortic aneurism.

### CASE III.—*Fatty Enlargement of the Liver.*

James Grant, æt. 29, blacksmith, admitted October 14th, 1851. His occupation consists of watching an apparatus worked by steam, in an elevated temperature; he has no heavy labour, though constantly standing on his feet; he drinks whisky to a large amount. Since September 1849, he has been three times in the house for various periods, from which he has been dismissed relieved. The liver began to enlarge two years ago, and has been very slowly increasing ever since. On admission, his digestive functions were well performed, with the exception of looseness in the bowels, having had two or three stools daily for several weeks past. He has, however, a dull heavy pain in the abdomen, extending to the lumbar region. The belly is evidently enlarged at its upper part, where a firm tumour exists, forming a protuberance in the epigastric region. The girth of the abdomen at this place, during expiration, is 34 inches. The hepatic dulness extends from two inches below the right nipple, down to a transverse line drawn one inch above the superior spine of the ilium. The whole of the right and part of the left hypochondriac regions are dull on percussion. The tympanitic sound of the stomach is audible in front, the organ being evidently pushed forwards by the enlarged liver behind it. The whole surface of the tumour feels smooth, and presents no tenderness. The splenic dulness measures  $5\frac{1}{2}$  inches vertically; skin dry; no oedema of the legs; general appearance pale and cachectic; occasionally he has frequent desire to micturate, but the urine has always presented its normal characters; considerable breathlessness on exertion, but the lungs and heart, on examination, were apparently quite healthy; other functions well performed. He was ordered a mixture containing the *Iodide of Potassium*, six grains of which were to be taken three times a-day. Frictions with the *Unguent. Iodini* were also to be employed daily. Towards the end of October, the bowels became regular, and his general health was somewhat improved. Frequent micturition, with discharge of pus in the urine now came on, which subsided in a few days. From this time, although the size of the liver underwent no diminution, his bodily strength gradually improved. He occasionally had slight return of looseness in the bowels, which was checked by appropriate remedies. The difficulty of breathing after exertion, also slowly left him; and he was dismissed greatly relieved, January 26th, 1852.

*Commentary.*—Fatty liver is now well known to depend on the secretion of a large quantity of oil, which is stored up in the hepatic cells. These cells are under such circumstances frequently enlarged, and contain oil varying in amount from a few granules to a large mass, which occupies the whole of their cavities. Not unfrequently livers, which to the naked eye appear healthy enough, may still be demonstrated with the microscope to contain an unusual number of fat granules, and there can be little doubt that considerable variations may exist in this respect quite compatible with a state of health. Almost all stall-fed animals that do not labour, possess a large amount of fat in their hepatic cells. It is only where the organ is much enlarged, altered in colour, and presses upon neighbouring viscera, that its fatty degeneration can be said to interfere with the vital processes.

In man, fatty degeneration of the liver has been observed to be very common

in two kinds of cases—1st, in drunkards; 2d, in persons labouring under phthisis pulmonalis. Drunkards are continually taking alcoholic liquids, which abound in carbon, and which being too large in amount to be excreted from the lungs as carbonic acid, and from the liver as bile, is stored up in the latter organ in the form of fat. In phthisis pulmonalis, the excretory power of the lungs is diminished, and the excess of carbon in the tissues and food is thrown upon the liver to be excreted. Under these circumstances, it is converted into fat and stored up in that organ.

The manner in which the livers of geese are prepared at Strashurg, is by following a process somewhat similar. They are confined in close cages, in a heated atmosphere, and largely supplied with food. Want of exercise and heat diminish the respiratory functions, and cause that of the liver to be disordered, and the result is enlargement of the organ from accumulation of fat. In the case before us, such exactly seems to be the cause of the disease. A man is kept stationary watching a steam-engine, in an elevated temperature, whilst he is consuming his usual food, and exceeding in alcoholic drinks.

This view, however, has been objected to on the following grounds:—1st, That the connection between fatty liver and disease of the lungs is not general; 2d, That there is no evidence that a fatty liver does not excrete bile as usual; and 3d, That as a considerable portion of bile is absorbed into the blood to be excreted from the lungs, the liver must be considered as preparing material for these organs. Hence it is argued that it would be a strange compensation if the functions of the liver were to be increased, while that of the lung is diminished by disease (Budd). But if fatty liver be not always conjoined with diseased lung, it will be found associated with some circumstance which diminishes the function of that organ, in relation to the work it is called upon to perform; for instance, the separation of carbon from the alcoholic fluids taken by the drunkard. Again, want of exercise from various diseases, and especially phthisis, whilst, in order to support the strength, wine and nutritious diet are given liberally, may frequently be seen to be the cause of fatty liver. Further, although it be granted that the liver may in health prepare carbonaceous matters for pulmonary excretion, it must be clear that if the lungs cannot accomplish this function, such matters must be thrown back or retained in the liver, and constitute a powerful cause of fatty degeneration of that organ. On the whole, therefore, we must regard excess of carbonaceous matters in the system, and the diminution of pulmonary action, as the chief causes of derangement in the functions of the liver; a view which has the merit of pointing out to us as remedies a diminished diet, a temperate climate, appropriate exercise, and an endeavour to promote the functions of the lungs and of the skin.

There is another structural alteration of the liver, which, from the colour and general appearance so like bees' wax it assumes, has been called "waxy" and sometimes "brawny" liver. This disease has been confounded with fatty liver, although an examination of its minute structure will show that the hepatic cells present a very different character. Instead of being enlarged and filled more or less with oil globules, they are colourless, shrunken, and for the most part destitute of contents, while the nucleus has disappeared. The lesion seems to me to be a further stage of the fatty degeneration, in which the oily matter is absorbed, and the cell-walls are left behind and aggregated together; but further researches are required to determine this point.

#### CASE IV.—*Cirrhosis of the Liver.—Ascites.*

John Harper, æt. 28, farmer, from Caithness. Six years ago, after recovering from measles, his health was greatly deranged. He was weak, and perspired profusely at night, or when performing any unusual exertion. A short time afterwards, he was exposed to cold and wet, and he observed that the abdomen gradually enlarged, and dyspnoea supervened. On two occasions paracentesis abdominis was performed; and at the first operation a quart, and at the

second a pint of fluid was removed, without producing much relief. He has had considerable pain in the epigastric region at times, and latterly the appetite has been diminished, and the bowels costive. On admission, the abdomen is slightly swollen, and evidently contains fluid. Round the umbilicus, it measures 39 inches. No anasarca. The hepatic dulness extends three inches downwards on the right side, commencing two inches under the nipple. Tongue is furred; appetite diminished; no epigastric pain or uneasiness; bowels irregular, but at present costive. There is slight dulness on percussion under the right clavicle, with harsh inspiration, prolonged expiration, and increased vocal resonance; urine scanty, depositing lithates. The other functions are well performed, and he appears to be a strong, well-nourished person. The treatment consisted of diuretics (*Sp. Æther Nit.*, and *Tr. Digitalis*) and sudorifics (*Pulv. Doveri*); but, feeling the confinement of the Hospital to disagree with him after his usual active occupations, he insisted on going out, which he did July 6th.

*Commentary.*—The diminished extent of the hepatic dulness, the ascites, and the chronic nature of the disease, point this case out to be one of cirrhosis. This morbid change in the liver consists of hypertrophy of the fibrous element between the lobules of the organ and its subsequent contraction, whereby its volume is diminished, and the secreting cells compressed and atrophied. As a further result, the large venous trunks are also compressed, and their commencing ramifications so congested that effusion into, or dropsy of, the peritoneal cavity is induced. The nutmeg liver is an incipient condition of cirrhosis, in which the portal system of vessels in the organ is congested. In both conditions, the hepatic cells are more or less fatty and atrophied. The fatty degeneration in nutmeg liver may be seen to commence at the circumference of the lobules, whereas in the advanced stage of cirrhosis, all the cells are more or less diseased, some loaded with fat, and others with yellow pigment. Notwithstanding the great organic changes which are frequently observed in this disease, danger is not so much to be apprehended from interruption in the functions of the liver, as from the ascites, induced by the constriction of the large abdominal veins, which, by distending the abdomen and compressing the lungs and liver, so interferes with those important organs, that death is occasioned.

The treatment in cirrhosis must be purely palliative, and directed to diminishing the ascites, by means of diuretics and diaphoretics. The question of drawing off the fluid by paracentesis is one which may arise, in case the swelling is very great, and the embarrassment to the pulmonary and renal organs extreme. Even then, although temporary relief may be obtained by the operation, there is every reason to believe that, in the majority of cases, life is in no way prolonged.

#### CASE V.—*Diabetes.*

Allan M'Clemont, æt. 32, labourer. Admitted 7th June 1852. About three weeks ago, on recovering from a general rheumatic attack, he found himself much reduced in strength, and somewhat emaciated. He experienced great thirst, and passed a large quantity of urine. These symptoms have rapidly increased. On admission, tongue moist and clean, appetite increased, thirst excessive, bowels rather costive, skin dry, urine very pale and slightly turbid. On heating a portion of the urine with an equal portion of *Aq. Potassæ*, a deep brown colour is produced. He has passed during the last 24 hours, 380 oz., spec. grav. 1030, having drank 460 oz. of water in that time. Other functions performed normally. His weight was 11 stone 8 lbs. Ordered pills of *Aloes and Ipecacuan.*, and a mixture of *Inf. Quassia* and *Tr. Aromat.* On the 10th June, he was ordered the following diet: 3 cakes made of bran, butter, and milk, weighing half a pound; 3 eggs; 4 oz. steak for breakfast, 12 for dinner, 4 for supper; 1 cabbage; 3 bottles of soda water; 8 oz. of lime water; 3 oz. of wine. To have a warm bath every third night. On 15th June the amount of urine passed was diminished to 120 oz. in the day, of density 1036, and he drank during that time 150 oz. His weight was 11 stone. On the 22d, he was

ordered 4 oz. of steak additional, and another bran cake. From this time the amount of urine fluctuated from 160 to 190 oz. daily; but on the 5th July it was reduced to 150 oz., spec. grav. 1034, and his drink was 167 oz. He then weighed 11 stone 2 lbs.; but, being wearied of the treatment, he insisted on going out on the 6th.

*Commentary.*—The excretion of sugar in large quantities by the kidney, has for a lengthened period excited the attention of pathologists, and given rise to abundant speculation. It having been shown by Mr Macgregor of Glasgow, that sugar was formed in the stomach from the digestion of food, while that principle was subsequently detected in the blood by the same observer, as well as by Ambrosiani, Maitland, and Percy,—the view of Rollo was, on the whole, considered the correct one, and the treatment he proposed has been, in its main features, followed by subsequent practitioners. This theory supposed that the sugar found in the stomach and alimentary canal, from the starchy and saccharine principles of the food, instead of being rapidly converted into other compounds, as it was supposed to be by Prout, was absorbed into the blood, and excreted by the kidneys. The treatment based upon this theory was, therefore, directed to keeping up nutrition from substances incapable of being converted into sugar; and it is worthy of remark, that such treatment has greatly diminished the excretion of sugar, without, however, suppressing it, and has also ameliorated the other symptoms. The researches of M. Bernard have introduced other views as to the origin of sugar in the animal economy. He admits that sugar may be formed in the process of digestion, and that a certain amount of it may, as a result of absorption from the alimentary canal, find its way into the blood. But he has shown that, in dogs fed entirely on animal food, sugar may be demonstrated to exist in the liver and in the blood of the hepatic vein, while it is absent in the portal vein. Moreover, he has shown that sugar is a normal secretion of the liver in all animals, from man down so low in the scale as the mollusca; and that, moreover, it is secreted by the liver of the foetus. He has proved, experimentally, that this function is increased, and diabetes produced, by irritating the eighth pair of nerves at their origin in the fourth ventricle; while, on the other hand, section of these nerves destroys its formation. I have seen M. Bernard perform these experiments, and repeated them myself in this city, and have no doubt regarding the accuracy of the results. That sugar does not exist normally in urine and blood drawn from the arm, is explained by its rapid decomposition in a state of health, and its excretion by the lungs. But when so increased that the lungs cannot secrete the whole of it, then it passes off by the kidneys, and hence diabetes. It follows that this disease is of hepatic origin, and is capable of being excited artificially, by irritation of the eighth pair of nerves, although in man this and other causes have not yet been determined.

This theory of M. Bernard explains why Rollo's treatment diminishes the excretion of sugar, by cutting off all that enters the blood through the alimentary canal, and further explains why such treatment does not cure, because it fails to affect the hepatic organ. It appears to me also capable of throwing light on the good effects of opium, from its power of diminishing nervous irritability—effects which are universally recognised in this disease. No other practical results, however, are as yet derivable from it, inasmuch as we are completely ignorant of any therapeutic means capable of diminishing this secretion, when once found in excess. Further researches are required on this point, so that the attention of practitioners, no longer *exclusively* directed to the digestive organs, may, by new efforts, endeavour to control this singular disorder.

The diet ordered in this case is one which admits of very slight formation of sugar in the alimentary canal, and, together with opiates and the occasional use of the warm bath, is the best which has hitherto been adopted. Its good effects were well manifested, although it proves, in conjunction with the confinement of an hospital, very irksome to the patient.



## Part Fifth.

### MEDICAL NEWS.

#### EDINBURGH OBSTETRICAL SOCIETY.

MEETING III.—*January 14, 1852.*—DR SIMPSON, President, in the Chair.

##### PRICES OF CHLOROFORM. BY DR SIMPSON.

As the price of chloroform was one potent reason, particularly in pauper and country practice, for not using it so extensively as it might be employed, Dr Simpson remarked, that he had found many practitioners unaware of the cheapness with which the drug was now furnished. In his original pamphlet upon chloroform as a substitute for sulphuric ether, Dr Simpson had observed:—"I believe that, considering the small quantity requisite, as compared with ether, the use of chloroform will be less expensive than that of ether; more especially as there is every prospect that the means of forming it may be simplified and cheapened." These anticipations were now so far fulfilled that chloroform was at the present time furnished by two or three of our principal manufacturing chemists in Edinburgh at six shillings per pound weight, when taken in large quantity; and was sold generally to the profession in smaller quantities at eight-pence per ounce weight. If the Government would allow (of which there was some prospect a few years ago) the alcohol used in medicine and for chemical purposes to be employed free of duty, chloroform would then be made considerably cheaper than it even is at present. The price charged for chloroform in some towns on the Continent was very high; and yet the manufacturers should be able to make it cheaper than in Britain, as their alcohol is much cheaper. In Edinburgh it was now generally made of the specific gravity 1500. He had seen several foreign specimens, not only very high in price, but very impure.

##### IS ANÆSTHESIA A PREVENTIVE OF PUERPERAL MANIA ?

*Dr Simpson* gave an account of the use of chloroform in patients predisposed to puerperal mania. He stated the particulars of three cases in which chloroform was used in patients, who in former confinements had been the subjects of puerperal mania. In none of these cases did the disease return. One of them had born several children previously; and after each labour, had been attacked with puerperal mania, but had entirely escaped from any recurrence whatever of the disease after the last labour, in which he had employed chloroform.

##### SOUND HEARD DURING THE THIRD STAGE OF LABOUR.

*Dr Simpson* stated the result of M. Caillant's investigations on a supposed stethoscopic indication of the separation of the placenta; but, whilst describing and admitting the sound, Dr S. added various reasons for dissenting altogether from the explanation of the sound offered by M. Caillant. Dr Simpson believes the sound is produced by the mere physical compression of the placenta, as it is being expelled from the uterus; and it could be imitated with a placenta, after its expulsion from the body, by pressing the placenta through an aperture such as that of the cervix uteri.

MEETING IV.—*February 11th, 1852.*—DR SIMPSON, President, in the Chair.

##### CASE OF PUERPERAL CONVULSIONS. BY DR KEILLER.

A. W., æt. 40, unmarried. Was admitted into the Edinburgh Royal Maternity Hospital, December 1, 1851. She stated that her delivery was not expected until



the end of the month; but on Friday the 12th, the labour began at twelve P.M. At noon of the following day, the os was dilated to the size of a shilling. The membranes ruptured shortly after, and the first stage terminated at seven P.M. Chloroform was given in small quantity during the latter end of this stage. The head presented in the first position. Up to two A.M. on the Saturday, the head remained in the cavity of the pelvis, without in the least degree altering its position. The short forceps were then applied, and the delivery thereby readily accomplished. The child, a male, weighed 8 lbs. 2 oz., and subsequently did well. The mother had retention of urine, requiring the use of the catheter twice daily for a few days; otherwise she seemed to be doing well, and had every prospect of a speedy recovery. Upon the 16th, four days after delivery, the house-surgeon was suddenly called to see her, when he found her in a state of insensibility, convulsed, with stertorous breathing. The convulsions were strong and universal. There was deep turgescence of the face; features much distorted; jaws violently clenched; mucus issued from the mouth; the hands were firmly clasped, with the thumbs and fingers bent inwards; and the urine and fæces had been expelled involuntarily. The pulse could not be felt at the wrist during the paroxysm, but returned as the paroxysm subsided. Similar attacks, lasting from five to ten minutes, continued without intermission for four hours, the patient remaining perfectly comatose the whole time. She was cupped behind the ears, had sinapisms applied to the legs, feet, and breast. The head was shaved, and cold applied. Croton oil was administered, and a castor oil enema given. She continued much in the same state during the whole of that night,—both the enema and the oil acting freely, but without any apparent relief. On the following day, it was observed that her right side was paralysed,—the left arm and leg tossing about, accompanied with blowing respirations; at times, however, she lay as if in a quiet sleep. The heart sounds were normal; pulse small and weak on the left side, but on the right much fuller. A blister was applied to the scalp; and a turpentine and castor oil enema, given in the afternoon, brought away a large quantity of black feculent matter. Mercurial ointment was rubbed over the blistered surface, and sinapisms were applied along the course of the spine. The pupils obeyed the stimulus of light, and consciousness temporarily returned. Her pulse was now 132,—respirations twenty-six in the minute. She continued much in the same state as described above; was allowed stimuli in the shape of wine and beef-tea; had a turpentine enema repeated once or twice; but gradually sunk without any alleviation of her symptoms, and died on Saturday morning, the 20th. At the autopsy, the brain only could be examined; and with the exception of a very slight vascularity of its membranes, and some subarachnoid effusion, no abnormal appearance was discovered. During her illness the urine was repeatedly examined, but at no time did it present any abnormal appearance. After death it was ascertained that the patient had received a letter from the father of the child, on the day on which she was taken ill, denying the paternity, and refusing to provide for the child, which communication had caused her considerable mental suffering.

*Dr Simpson* exhibited a preparation of an atrophied uterus, and made some remarks on this subject. (See Article in our present Number, p. 127).

CASE OF FETAL MALFORMATION, WITH EXTROVERSION OF ABDOMINAL VISCERA.  
BY G. H. HONEY PATERSON, ESQ., SURGEON, ERROL.—(COMMUNICATED THROUGH DR SIMPSON.)

Mrs B., æt. 18, of ordinary stature, and well formed, began to feel ill on the 21st of June 1851, while in the eighth month of her first pregnancy, and continued more or less till the following morning, when, about five A.M., a violent pain was felt; immediately after which a large quantity of liquor amnii was discharged; on the occurrence of which, a message was sent off to request the attendance of Dr P. On his arrival, half an hour after, he found the os uteri

dilated with a substance closely resembling the placenta detached, lying over the cervix, but no hemorrhage had taken place. He now cautiously endeavoured to pass his hand beyond it, for the purpose of ascertaining more particularly the nature of the presentation. Having done so, and finding it preternatural, an effort was made to get hold of the feet; on attempting which, however, the uterus contracted strongly and forcibly, during which the nates presented. Another attempt being made, the nates were found wedged firmly in the pelvis. After administering to the woman a gentle stimulant, and allowing her a short interval of rest, the crotchet was introduced, and hooked over the lumbar vertebræ close to the sacrum. At this period the pains became much stronger, and ere long the delivery of a dead child was speedily effected, and the mother did well. The foetus presented the following appearances:—Apparently long since dead in utero, and highly decomposed; head anencephalous, and greatly enlarged, which, on being removed a little from the mother, appeared to be retained. The cause of the retention being traced, the umbilical cord was found attached to the protuberance of the occipital bone; and, from the shortness of the funis, it had given way without the necessity of dividing it; in consequence, the water contained in the head instantly gushed out, leaving it in the form of a cone, and flattened at the sides. On examining the natural situation of the cord, nothing remained of it except about two inches shrivelled up and contracted. The nose was unformed, and the nostrils were on a line with the eyelid; the upper lip was divided by a single fissure; right arm wanting, with the rudimentary form of a hand affixed to the shoulder joint; chest distorted towards the left side, and much contracted; the viscera, conglomerated into one mass, obtruded at the sternum,—except the heart, in a state of atrophy, which remained in the cavity of the thorax; left leg shortened to some extent, the heel drawn up and the toes turned inwards, so as to represent a combination of the talipes equinus with the talipes varus.

CASE OF LARYNGISMUS STRIDULUS. BY DR ROSS, GOLSPIE.—(COMMUNICATED THROUGH DR SIMPSON.)

*Dr Simpson* quoted the following extract from a letter by *Dr Ross* to *Professor Alison*:—"Mr Reed's child was seized for the first time with *Crowing Inspiration*—one fit being so severe and so long, that she almost expired before the glottis opened. When I got to her, about two in the morning, she was very fretful, but still inclined to sleep; as soon as sleep came, the laryngeal spasm returned and awoke her. After using the hot bath and giving a turpentine enema, I was at a loss what to do; but that evening, at home, I had read *Dr Simpson's* case, which appeared in the last Number of the "Monthly Journal of Medical Science." Having brought chloroform with me, I began it and kept her about an hour under its influence, but I was afraid to push it much farther. However, after this she was much better; and after a dose of the musk mixture, to which I added two drops of *Tr. Opii*, she fell asleep, and slept quietly for nearly six hours. Since this time she has been much better; every day she had two or three attacks of the laryngeal spasm, but they never amounted to complete closure of the glottis."

CASE OF ANENCEPHALOUS MONSTER. BY DR FENTON OF ALYTH.—(COMMUNICATED THROUGH DR SIMPSON.)

Mrs L., æt. 36, of a sanguine temperament, the mother of four children, two boys and two girls, all very healthy. Her husband called on *Dr F.*, on Tuesday night, the 13th January last, desiring him not to go from home, as Mrs L. was in labour. He was called to visit her on the following morning about six o'clock. On entering the house, one of the women who was attending her during the night informed him that the membranes had ruptured last night about eleven o'clock, and a very great quantity of water had followed,—the pains coming regularly every fifteen minutes. On making an examination, he found the os uteri dilated about the size of a shilling, having a thin and smooth edge. He could feel

with the finger an irregular edge of bone, and a substance of a soft nature, very unlike anything he had ever felt before in labour cases. He remained about an hour without the child advancing, for the pains were weak, and only coming every twenty minutes. Feeling a little uneasy at the unusual presentation, he went home to consult some works on midwifery, but found nothing to assist him in forming an opinion of what was coming. After breakfast, he returned, and found the patient walking in the room. She had some pains since he left, but very few. He ordered the nurse to give her a cup of tea, and put dry clothes into the bed. During the time the nurse was doing this, he embraced the opportunity of seeing a few patients in the neighbourhood. On returning (being absent about forty minutes), he found the patient in bed, and the pains coming every five minutes. On examining, he found the os uteri now fully dilated, and could feel the eyes, nose, the bony portion above the eyebrows, and a considerable portion of the unossified part of the head.

From the free motion of the head in the pelvis, and the posterior part being unossified, he felt perfectly satisfied that a great portion of it was wanting. The pains were now strong and quick, advancing the child each time considerably; and the unnatural head was brought into the world about twelve o'clock. A few minutes after, the upper extremities, trunk, and lower extremities, followed with ease. For about a minute, the child had the appearance as if dead; but the heart began to palpitate, the nostrils expand, the ribs on each side were drawn in, so that the chest had the appearance as if it had been crushed between two bodies; the eyelids opened, the tongue protruded a considerable way beyond the lips, a slight gurgling was heard, but no crying; the upper and lower extremities moved a number of times, and the head was raised a considerable way from the place that supported it; the heart continued to flutter for about fifteen minutes, when all motion ceased.

The placenta was of a dark colour. The mother made a good recovery. Since the delivery, Dr F. learned that Mrs L., in the first months of pregnancy, had removed a weighty box from one part of the house to another, and felt great pain for a number of weeks afterwards in that part of the abdomen where the box pressed.

*Dr Sidey* communicated a case of Rupture of the Ovary, simulating in every particular pelvic abscess.

MEETING VI.—*April* 14, 1852.—Dr MYRTLE in the Chair.

CASE OF PLACENTA PRÆVIA. BY DR A. THOMPSON LOUNE.—[COMMUNICATED THROUGH DR GORDON.]

Mrs S., a stout full-bodied woman, of sanguine temperament, aged 34, had been married six years. Her first, second, and third pregnancies were attended by abortions, each of which, according to her friends' statements, was followed by profuse hemorrhage, and the first by some form of abdominal inflammation. Being again pregnant, she was delivered, at the full time, of a boy, now living, and made a good recovery. In June 1851, she became again pregnant; catamenia were observed for the last time during the second week of that month, and she continued to enjoy good health, with the exception of a chronic cough, attended with asthmatic paroxysms, up to January 29, 1852, when Dr L. was first summoned, at two in the morning, to attend her. On his way to her place of abode, he was informed that she had been in a state of great excitement the preceding evening, occasioned by her husband coming home in a state of intoxication, that she had gone to bed in that condition, and during a fit of coughing a profuse uterine hemorrhage had occurred. He found the sheets soaked with blood; several large clots were lying around her. There was a pail by the bedside filled with saturated napkins; and a small stream of blood was trickling towards the door. There was great anxiety and alarm, and slight tendency to syncope; but the constitutional effects were by no means so striking as he should have expected from so profuse and sudden a hemorrhage. Upon examination, he found

the os so high up, and directed so much towards the sacrum, that he could barely reach its anterior lip, and was therefore unable to ascertain its condition. By placing the head low, ventilating the apartment, removing the bed-clothes, and the plentiful application of vinegar and water to the hypogastrium and thighs, the hemorrhage and its more immediate effects were quickly controlled. A very slight oozing of blood continued for about an hour, when it wholly disappeared. The patient quickly recovered from its effects, and in a few days nothing but slight pallor remained. She continued well until February 12th, fourteen days after the first attack, when, at the same hour, he was again sent for, as the hemorrhage had re-appeared, and without any apparent cause. This time it proved but slight, and was readily arrested by application of cold. During the following day, one or two small coagula were passed; and on the following night, he was again called in, as about three ounces of blood, and a pretty large coagulum, had been suddenly ejected. He now began to feel very anxious about the case, and mentioned to Dr Gordon his suspicions, that it would prove one of placenta prævia, when Dr G. very kindly offered his assistance, if such should prove the case; and as Dr L. greatly feared, from the amount of blood already lost, farther hemorrhage could not but suddenly and seriously affect the system, he cautioned her friends to summon him immediately upon its re-appearance. During the 14th and 15th, there was no hemorrhage; but during the night of the 16th, a slight oozing took place, and this being quickly followed by rather a profuse gush, he was, at three in the morning, again called. Upon his arrival, it had entirely ceased; but this comparatively slight loss of blood had produced constitutional symptoms of so decided a character, as clearly to indicate that the time for non-interference had passed by. Upon examination, he found a change had taken place in the position of the uterus, the os being sufficiently low to enable him to determine the nature of the case. It was patent to the extent of about half an inch, somewhat rigid; and the placenta could be felt adhering all around, except posteriorly, where he thought it rather extensively separated. As there were no pains present, the hemorrhage had ceased, and the constitutional symptoms not being urgent, there was time to seek farther counsel. Dr Gordon quickly arrived, and upon examination, confirmed Dr L.'s opinion as to the nature of the presentation. They determined to administer stimulants, watch the condition of the os, and to give ergot, to induce uterine contractions, with a view to lessen the danger of post partem hemorrhage. After three doses had been taken, slight contractions ensued, and the os began slowly to dilate. At seven, although there had been little if any further hemorrhage, yet the patient was evidently much weaker; and as they feared they might have to turn before the os could be considered in a fit condition, although it was assuming a more favourable aspect, they thought it better to avail themselves of the experience of Dr Zeigler. Upon that gentleman's arrival, he considered it necessary to interfere; and as the membranes were unruptured, and the os did not offer any considerable resistance, turning was effected without difficulty, and a dead child extracted, not, however, without the use of considerable force. The placenta was separated, and removed by the hand. The uterus contracted, and neither during the extraction of the child nor placenta was there any blood lost. After delivery the patient was much exhausted. Brandy and beef-tea were freely administered, warmth applied to the feet, etc. For some time she remained in a very precarious state, but towards evening the system rallied. A dose of morphia was given at bedtime.

*January 17th—Morning.*—Condition favourable; has slept during the night; is perfectly composed and sensible; complains of nothing, but that the beef-tea had turned acid in her stomach, and produced uneasy sensations in that organ; pulse 130, of better strength; temperature of body normal; tongue moist and clean; urine has been freely passed; lochial discharge normal; pains occasionally present; expression of countenance improved; mucous membrane of mouth and lips very pale; expresses desire for food. *Evening.*—Favourable condition continues; bowels have been freely relieved; there is no abdominal tendency or pain; lochia continue. Morphia at bedtime.

18th—*Morning*.—Has passed a restless night, but after ventilating the room had an hour's quiet sleep; the skin is hot and dry; tongue dry and furred, and there is great thirst; pulse 140; lochia continue; breasts are slightly distended, and a small quantity of milk passes from the nipple; there is neither abdominal pain nor tenderness; uterine tumour normal in size and hardness. These febrile symptoms being contemporaneous with distension of the mammæ, he ascribed them to secretion of milk; but Dr Zeigler, who saw her in the afternoon, thought there was slight tympanitic distension, and that phlebitis had set in.

19th.—Has passed a bad night; slightly delirious at times, but this morning says she is much better; desires food, and declares herself free from all pain; pulse very variable in strength and frequency during the day. *Evening*.—Was seized with a pretty severe rigor at two this noon, which recurred several times during the afternoon, but with decreasing intensity; has vomited twice since last visit; lochia are scanty, dark-coloured, and rather foetid; bowels open. To have vagina syringed with warm water, and flannel, sprinkled with turpentine, applied to the abdomen.

20th—*Morning*.—There has been no recurrence of rigors during the night, but occasional vomiting persists; the countenance is much fallen; no milk; and lochia have disappeared; two dark-coloured offensive stools; no abdominal tenderness or pain, or abnormal hardness of uterus; there is slight tympanitic distension. A mustard plaster was applied to the epigastrium, calomel and James's powder were given, and afterwards calomel and opium; turpentine and flannel to abdomen. *Evening*.—Symptoms the same, except vomiting, which has ceased. Calomel to be discontinued, as it is passed off by stool.

21st.—Has had a very restless night, with occasional delirium; has passed two or three loose stools; pulse scarcely perceptible, and very quick; respiration hurried and laborious; teeth covered with sordes; abdomen slightly tympanitic; patient is quite collected; says she has no pain. An astringent was ordered, and brandy, beef-tea, milk and water—the former every half hour, the latter frequently. *Evening*.—Apparently moribund; lies on her back, with laborious respiration.

22d—*Morning*.—Still living, but unable to speak; is apparently quite conscious, and frequently indicates a desire to drink; breath cold; countenance hippocratic. On raising her fingers to her mouth, and protruding the tongue, she was lifted up, and swallowed some brandy and water. On repeating the same movements half an hour later, it was again held to her lips, but she was unable to swallow. Shortly after, the eyes became fixed, the pulse ceased to be perceptible, and after a few sighing respirations, and a slight convulsive movement, she expired six days after her confinement.

After much difficulty, the relatives yielded to the repeated solicitations of Dr L. and Dr Gordon, and they obtained a post-mortem examination, which was kindly conducted by Dr W. T. Gairdner, who furnished the following notes:—

*Sectio Cadaveris*—*Drs Loune and Gordon present*.—Female, aged apparently 45. Post-mortem lividity and suggelation over veins of neck, otherwise great pallor; corpulent, upwards of one inch of external fat. In the pericardium about an ounce of sero-sanguinolent fluid; heart flabby, pale; cavities natural, blood well coagulated, but with much serum. Lungs very pale throughout, a little distended anteriorly, otherwise normal; no adhesion of pleura. Peritoneum normal everywhere; intestines distended with flatus, unexamined. Liver soft, pale, many of the cells containing very large oil-globules. Spleen large, probably weighing sixteen ounces; perfectly diffuent, flowing out of the capsule in the form of a grumous pulp. Kidney soft, perfectly opaque in tint, and yielding an opaque juice abundantly from cortical substance; no decided appearance of disorganisation, but under the microscope most of the cells present fine molecular shading with some larger fatty granules. Uterus flabby, imperfectly contracted; length about eight inches; internal surface covered with olive-green foetid pus and organic debris; small shreds of gangrenous matter attached to various points of the interior; many of the veins in the uterine walls contain coagula mixed with pus of



the same character as on the internal surface of the uterus; uterine tissues otherwise perfectly normal. The ovarian and hypogastric veins contained some air, but their blood appeared normal; vena cava inferior contained a small, well-formed, apparently normal clot. Blood examined microscopically, nothing abnormal.

ON THE ESQUIMAUX FEMALE PELVIS. BY DR JAMES STRUTHERS, LEITH.

*Dr James Struthers* exhibited the pelvis of an Esquimaux woman which he had received from Mr George C. Pirie of Dundee, who obtained it last summer while acting as surgeon to a whaling vessel. It was found near Cape Hooper, on the coast of Davies' Straits, in latitude 68.6 N., longitude 64.36 W., under a cairn of stones,—the usual mode of burying there. As a specimen of the pelvis of this division of the Mongolian race, it is interesting from its rarity, but chiefly from the unusually large dimensions of the brim, cavity, and outlet.

In its form it approaches the square, the distinctive character, according to Weber, of the Mongolian pelvis; the transverse diameter exceeding the conjugate by  $1\frac{1}{2}$  inch.

The brim is  $16\frac{1}{2}$  inches in circumference, being 2 inches more than in a large European pelvis. The diameters of the *brim* are:—

Transverse,	-	-	6 inches
Conjugate,	-	-	$4\frac{5}{8}$ ...
Oblique,	-	-	$5\frac{5}{8}$ ...

The first being  $\frac{5}{8}$ , the second  $\frac{5}{8}$ , and the third  $1\frac{5}{8}$  inch larger than in the standard assumed by Ramsbotham.

The diameters of the *cavity* are:—

Transverse,	-	-	$5\frac{1}{8}$ inches.
Conjugate,	-	-	$5\frac{1}{8}$ ...
Oblique,	-	-	$5\frac{1}{8}$ ...

An increase over the standard of 1 inch in the antero-posterior, and of  $\frac{1}{2}$  inch in the two other diameters. The *outlet* is also very capacious; the transverse diameter being  $5\frac{1}{8}$  inches; the conjugate about the same; while the pubic arch is unusually wide. The depth of the cavity is much the same as in the European pelvis.

This being the only specimen of an Esquimaux pelvis that Dr S. has had an opportunity of examining, no general deduction of course can be drawn as to the pelvis of the Esquimaux being larger than that of the European. It is worthy of observation, however, that parturition among the Esquimaux, whatever be the cause, is by all accounts much more rapid than with us—so rapid, indeed, that after making all due allowance for the child being smaller, which is the case, and for the half savage condition of the natives, we can scarcely avoid the inference that something may be due to the maternal passages being wider in the one race than in the other. Crautz, in his History of Greenland, states that parturition among the Esquimaux is both rapid and easy; and Mr Pirie, during his residence there, received many accounts to the same effect. Mr Pirie also collected some curious information as to the manner in which the accouchements are conducted, the leading feature of which is that the whole process is left entirely to nature. Till within a few hours of labour, the woman goes about her usual duties. As soon as she feels unwell, she retires to a skin hut built for the purpose, which is so small as to confine her almost entirely to the recumbent posture. No one is allowed to be present while labour is going on; and the woman, it is stated, generally makes her appearance within the hour, carrying the child in the hood attached to her dress.

It is the general belief that the child is not separated from the placenta until the latter is expelled, and that the division is effected by the mother gnawing through the cord. The Greenland women, according to Crautz, are not very prolific, seldom having more than three or four children, between each of which there is generally an interval of three years. When told of the fecundity of Europeans, they compare them contemptuously to their dogs. Miscarriages, twins, and monsters are very rare; and scrofula and rickets are almost unknown. The



children subsist for three or four years solely on the mothers' milk; there being no other nourishment suitable for them. If another child be born before the mother has ceased nursing (not an uncommon occurrence), the latter frequently dies in consequence of being deprived of its supply of milk; and should the mother die while the child is young, it, as a necessary consequence, does not long survive her.

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### EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING VIII.—*Wednesday, May 5, 1852.*—Dr BEGBIE, P., in the Chair.

(Continued from p. 94.)

#### CAUTERISING THE DENTAL NERVE.

Dr W. A. Roberts read the following communication, "On the Application of the Actual Cautery for Destroying the Dental Nerve, by means of Electricity:"

Mr President,—I must crave the indulgence of yourself and this Society, in bringing forward a subject apparently of more interest to the dentist than to the medical practitioner—namely, the application of the actual cautery, by means of electricity, for the purpose of destroying the dental nerve.

It has frequently been a cause of much mortification to myself, and, I doubt not, likewise to others of my professional brethren, to be under the necessity of extracting teeth, after the patient use of all the means usually employed to deaden the dental nerve, and thus enable us to stop the decayed teeth, or place artificial teeth on or over the remaining roots. The importance of this may be readily understood, if we suppose the case to be that of a young lady, in whom one or more of the incisors have decayed so far as to change the expression of the face and affect the speech, thus requiring them to be artificially replaced, both for the sake of appearance and articulation. Here, however, so great may be the sensibility of the dental pulp, that it is impossible to pivot the tooth, and we are compelled either to extract the fang, which might otherwise have been valuably employed in supporting a new crown, by means of a pivot made of gold, or else we must have recourse to a gold plate, which, in a young mouth especially, it is always most important to avoid, if possible. In some cases, a severe result has followed the pivoting of a tooth, such as abscess, and even erysipelas, in which there had been but a trifling amount of sensibility at the time of its insertion. In other instances, when even there had been no sensation manifested, yet in the course of a few hours only, we have seen the suffering become so great that extraction of the root was the only resort. On the other hand, in those roots where the pulpy portion is either absorbed by time or completely destroyed, there is not a simpler, neater, or safer method of supplying any deficiency that may occur in the incisors—of the superior maxillar especially—than by pivoting, while the roots frequently maintain their position for many years.

As, in cases of stopping decayed teeth, we have first to remove the carious portion thoroughly, by means of small instruments made for the purpose, it sometimes happens that the tooth is so very tender that the slightest touch causes so much agony, that it would be madness, if not something worse, to persevere while it is in this state. In such cases, the usual practice is to delay procedure for a time, and to endeavour to reduce the irritability by leeching, nitrate of silver, chloride of zinc, combined with morphia, etc. All of such measures, however, at times fail to obtain the desired effect, compelling us at last to the use of the forceps.

In America, the dentists in general speak highly of the use of *arsenic* for the destruction of the dental pulp; I have frequently tried it, and with various results. In some instances, even the irritation excited by its use seemed to be worse than the disease. Combined, however, with the muriate of morphia, arsenic certainly gave less pain, but still I quite agree with the opinion of Mr Tomes of London, who, in his work called "Dental Physiology and Surgery," says: "I

think arsenic should be struck off our list of dental remedies, seeing that we have other escharotics that are just as effective in destroying the pulp, and, if swallowed in the minute quantities we use, can work no harm. But, independently of these injurious effects, there is great danger that, in employing a stopping of this poison, it may escape from the cavity of the tooth, and be swallowed." In proof of this, I may instance a case recorded in the "American Journal of Dental Science," volume for 1843-4, of death following this application. The symptoms were decidedly those of poisoning by arsenic. The quantity *said* to have been used in this case was only one-twelfth of a grain, mixed with kreosote, so as to form a paste; but there is either cause to suspect that the quantity was much greater than this, or that the unfortunate patient (a Dr Walcott of Lychfield) had a peculiar idiosyncrasy.

Taking into consideration, then, the importance of preserving the teeth for the purposes of mastication, appearance, and articulation, surely the conservation of such a valuable apparatus is more meritorious than their extraction, however dexterously performed, if they can be preserved for a few years only;—how much better and more serviceable a tolerable tooth of one's *own* is, compared to the best artificial tooth ever produced!

In bringing this subject before the Society, I do not pretend to claim the merit of a new mode of practice, as the use of the actual cautery is by no means a modern application. But the employment in dental surgery of the cautery heated by electricity has not, as far as I am aware, been proposed or practised by any one, and this I submit is deserving of attention as a more *certain*, and, at the same time, a less startling mode of applying it. Almost every dentist must have met with patients who have wished the nerve destroyed, and who would submit to almost any amount of pain for the moment, for ultimate relief, but on no account would allow the offending tooth to be extracted. I have occasionally been asked by the patients to apply the cautery, or to burn the nerve (as they call it), and have at times complied; but a great drawback to the successful performance of this operation has always arisen from the necessary fineness of the wire required, which, before it can be inserted into the foramen of the tooth becomes too cold, and the consequence is, violent inflammation, like that following a burn, is set up, instead of effecting the immediate and total destruction of the pulp or dental nerve.

Among the many methods made use of by the profession of applying the actual cautery, I think the coolest, in one sense at least, is that adopted by Mr Koeker, and recorded in his work, entitled "Principles of Dental Surgery," published in the year 1826.

"I require," he says, "for this, the following apparatus,—1st, a small iron wire fastened to an ivory handle; the extremity of this wire I file to the size of the exposed surface of the nerve, and bend the wire in such a direction as to enable me to touch the exposed part of the tooth or mouth; 2d, a thick tallow candle with a large wick; I direct the patient to discharge all the saliva he may have in his mouth, then to incline his head backwards against the head-support of my operating chair; I put the candle into his left hand, and direct him to hold it in such a position that the flame of it may be on a level with his mouth," etc.

Some dentists recommend that a sharp wire should be thrust down into the root of the tooth, and then rapidly twisted round so as to tear out the nerve. There are advocates for this plan, who declare that there is not much pain in so doing, but that it is an operation rather pleasant than otherwise. I doubt much if many will coincide in this view of the matter,—more especially the party operated upon. When the actual cautery is considered necessary in cases of alveolar hemorrhage, the importance of this instrument, heated by the agency of electricity, I think will be felt, as the point of the bleeding vessel may be touched with a wire at a white heat, while no other part of the mouth is.

The method of applying the actual cautery, which I have now to explain to the Society, was suggested to me by my friend Dr Strachan of Dollar, who had seen the article in the "Lancet," vol. i. for 1851, page 546, in which a proposal was

made for excising tumours, etc., by means of wire brought to a white heat by electricity. I have followed up the idea; and the instrument on the table is the result. After several experiments, I found that "Grove's" battery answered best, being more convenient than "Smee's"; it is much smaller, and consequently more portable and more easily kept out of sight. By this one pair of plates I can produce a more decided result than I could arrive at with six pair of "Smee's." In the porous jar I have a mixture composed of two parts of nitric, to one part of sulphuric, acid. In the glass jar there is a mixture of four parts of water to one part of concentrated sulphuric acid. At the end of each of the conducting wires, you will observe a fine platina wire brought to a point. This wire is fine enough to enter the foramen of any tooth, and if required merely to cauterise the larger cavity of a decayed tooth, it can be easily rolled up to act as a small ball. When the wire is to be heated, the communication is made by gently pressing upon the ivory knob while pressing down the spring; the contact is made instantaneously; by taking off the pressure, the current is broken off as quickly. I found some difficulty at first in operating, from the want of elasticity in the wires, as the thickness necessary to convey sufficient quantity of the electric fluid to heat the platina wire to a white heat, rendered them very unwieldy. This has been got over very ingeniously and satisfactorily by a plan of my son's, which consists in filling two small India rubber tubes with quick-silver, the ends of the copper wires being inserted into the mercury at both ends, and tightly tied; by this means, the communication is rendered complete, and allows of free motion. Since this was done, we find now full freedom can be obtained by having a bundle of very fine wire tied together instead of a single thick one.

The advantages, then, to be obtained by this instrument are, its easy application to the desired spot in the mouth, and that perfectly cold, instead of alarming the patient by holding a red-hot iron before his face. Its being at once raised to the requisite heat, and no more than the mere point of the wire used being hot. Its being at once cooled on simply removing the finger from the ivory knob; and lastly, there being no appearance of heat to alarm the patient.

When to be applied for the purpose of arresting hemorrhage, or the deadening of the dental nerve, the cavity should be first well wiped out with a piece of lint, and then the desired spot should be rapidly touched, so as not to come into contact with other parts. This can be repeated if necessary. The platina must be at least red hot, as it then acts instantaneously, and with little or no pain, otherwise it would cause much pain, and subsequent inflammation. I need not say with timid persons the inhalation of chloroform should be resorted to. I have used this form of the actual cautery with and without chloroform in many cases. I may instance one case without the aid of chloroform. A young gentleman, of about nineteen years of age, came to me to have the front incisor of the upper jaw stopped; and while engaged in scooping out the decayed bone, from the great pain he was suffering (although he tried to hide it) he fainted. I made him call next day, applied the actual cautery; he felt, he said, almost no pain. The following day I stopped it with gold, and he is now quite free from any pain or uneasiness.

With these few remarks, Mr President, I will now conclude, and proceed to show the battery in action, which I trust will be thought more available than that of the thick tallow candle of Mr Koeker, or the instrument employed by a Cambridge friend of Mr Tomes of London, who, it seems, was dreadfully tortured with pain in a carious tooth, and one day, quite worn out with suffering, he broke off all the prongs but one of a dinner fork; the remaining one he heated red hot, and in that condition thrust it forcibly into the hole of the aching tooth. He says that the pain ceased in a minute, and that he has not, from that time to this, a period of nearly thirty years, had a single twinge of toothache.

I may repeat, that in every case where the cautery has been made use of by this method, I have not failed once in giving relief; and instead of having to

extract the teeth as requested, they have been stopped, and are now giving no trouble.

It is unnecessary to state, that where suppuration has taken place, the cautery will not be applicable. We must, therefore, treat the case as circumstances point out as being most proper.

*Mr Nasmyth* remarked, that an important consideration deserved attention in the first place, and that was, whether or not the application of the actual cautery was an advisable measure in the treatment of diseases of the teeth. At an early period *Mr Nasmyth* had used it to a considerable extent; but not having found it satisfactory, he abandoned it, and had not met with a case for many years past where he could have recommended its adoption. When used for the destruction of the nerve and vessels of an aching tooth, it certainly kills the pain, and destroys the sensibility of the tooth so as even to admit of its being filled with gold, or other stopping; but the tooth is then so disorganised, that sooner or later you generally have irritation, pain and swelling about the roots of the tooth, frequently of an indolent nature at first, but sometimes very acute.

The arrangement *Dr Roberts* has produced is certainly very ingenious and convenient, and similar in principle to what was announced last June by *Mr Harding* in the "Lancet."

*Mr Nasmyth* was perhaps not altogether warranted in making these observations, as he had not tried this new method of applying the cautery; but he could give the Society the evidence of one who had given it a fair trial. His friend, *Dr Dewar*, of Aberdeen, in a letter received from him the other day, says: "I availed myself of the opportunity which my last visit to London afforded me, of making myself familiar with the mode of applying intense galvanic heat to the exposed nerve of a tooth. I provided myself with the suitable apparatus, and have applied it a sufficient number of times to convince me that it is nearly useless in all cases,—as I have found, what with the difficulty of applying the apparatus to the exact spot efficaciously, and also taking into consideration the resources already at our command, nothing, absolutely nothing, has been gained when real efficacy was required. I have therefore abandoned the use of this new cauterising medium."

*Dr Roberts* fully agreed with what had been stated by *Mr Nasmyth*, and he (*Dr R.*) had referred to that point in his remarks just read. The want of success, and often disagreeable consequences following the application of the cautery by the old method, arose from the difficulty of getting the wire applied sufficiently heated. But, by the present plan, this difficulty was overcome, and the results had been most successful; out of fourteen cases where the patient was suffering greatly, almost instant relief had been given, and in no case had any unfavourable symptoms arisen,—the cases being under his observation at short intervals.

MEETING IX.—*Wednesday, June 2, 1852.*—*Dr SELLER*, V.P., in the Chair.

*Dr Scott* read the details of a case of aneurism of the descending aorta, opening into the left bronchus, and giving rise to many of the signs of pleuritic effusion. Death occurred suddenly by hemoptysis. The aneurism was not larger than a walnut.

*Dr W. T. Gairdner* read a communication relating to two cases of aneurism. In one of these cases, an aneurismal sac, arising from the innominate artery, interfered with the right recurrent nerve, and produced laryngeal dyspnoea, simulating disease of the larynx. The aneurism was small, and was not positively detected, although its existence had been suspected. It produced death by hemorrhage into the trachea. In the other case, there were two aneurisms of the descending aorta, which gave rise to extreme spasmodic dyspnoea, but of the asthmatic, not of the laryngeal, type. Death took place by orthopnoea. The expectoration was stained with blood, but there was no profuse hemorrhage. The aneurism was in this case also without positive physical signs; but the symptoms,

together with the signs of obstruction in the left lung, had led to a correct opinion as to the nature and seat of the disease.

*Dr Bennett*, in the course of some remarks on the cases of aneurism submitted to the Society, detailed at some length the particulars of a case which had been under his observation for upwards of two years, in which aneurism existed both in the thoracic and abdominal aorta. The latter, which was the only one detected, had been treated by the method of Valsalva, with apparent good effects; but the thoracic aneurism appeared to have made constant progress, and by extensive caries of the vertebræ, and pressure on the cord, had led to paraplegia. Death had occurred suddenly, in consequence of poisoning by aconite, which was swallowed by the patient (perhaps intentionally), having been given him in the form of a liniment for external application.

*Mr Syme* made some remarks on the medical treatment of aneurism by blood-letting, in reference to one of *Dr W. T. Gairdner's* cases.

*Dr Andrew Wood* suggested, that in cases of laryngeal spasm occurring in the course of aneurism, chloroform might be found useful in allaying or removing the spasmodic symptoms; and thus, that the necessity for tracheotomy might in some cases be avoided.

## EDINBURGH PHYSIOLOGICAL SOCIETY.

MEETING XIII.—May 8, 1852.—Professor BENNETT, P., in the Chair.

(Continued from Vol. XIV., p. 580.)

4. *Mr Barlow* exhibited a portion of large intestine (cæcum) taken from a young horse which died after being some weeks affected with profuse diarrhoea. The mucous surface of the gut was thickly covered with dark-looking elevations, the largest of which were about the size of a large pin-head. Their apices were blunted, had no apparent orifices, and were entirely composed of the semi-transparent epithelial layer. On opening the elevations, a brown coloured worm, varying in length from one-eighth to half an inch in length, was found spirally coiled up in the interior of each. When demonstrated under the microscope, its structure could be pretty clearly made out, and seemed to be that of a species of oxyurus, but on this *Mr Barlow* would not insist. The space or cavity containing the worm, is formed in the substance of the epithelial coat, sometimes between this and the muscular layer. These parasites are most commonly met with in young horses out at grass in autumn and spring. They cause in the first place great emaciation; this is succeeded by an intractable, most exhaustive, and not unfrequently fatal diarrhoea. On opening the horse after death, we frequently find ulceration of the intestines, which appears to commence in and extend around the sacs after the worms have left them. The worm is evidently retained in this nidus during a period of early development, as the same animal, in a more advanced stage of growth, from an inch to two inches long, is found free in the intestinal cavity. Its common habitat is the cæcum and commencing part of the colon. *Dr Knox* has very cursorily described and figured this entozoon in the xlii. vol. of the "Edinburgh Medical and Surgical Journal." This, so far as *Mr Barlow* knows, is the only recorded notice of the parasite, and is founded on two specimens sent in 1835 and 1836 to *Dr Knox*, then of Edinburgh, by *Mr Dick* of the Veterinary College, who was the first to discover them in this country.

5. *Dr Murchison* laid before the Society a description, with drawings, of the contents of the cyst in a case of ranula evacuated by *Mr Syme*, March 29th 1851. These, to the naked eye, presented the appearance of a transparent, glairy, slightly viscid fluid, not unlike the uncoagulated albumen of egg. This, on microscopic examination, was found to consist of nucleated cells suspended in a structureless fluid. The cells were of very delicate structure, of an oval or rounded form, and with a diameter varying from  $\frac{1}{800}$ th inch downwards. They seemed to be in various stages of development. Each cell contained a



single rounded or oval nucleus, rather smaller than a human blood-corpuscle, with a central dot or nucleolus. Between the nucleus and cell wall there was a greater or less quantity of a very fine molecular matter, and in many of the cells a number of minute oily particles, in some cases completely obscuring all appearance of the nucleus. The action of diluted acetic acid partially dissolved the molecular matter, rendering the nuclei more distinct.

#### 6. ON THE CONTENTS OF A LARGE SANGUINEOUS CYST.

*Dr Murchison* detailed the particulars of his examination of a large sanguineous cyst occurring in the left axilla of a little girl aged six years. The tumour had first made its appearance when the girl was only two years of age, and had gradually been increasing in size until the patient's admission into the Royal Infirmary in December 1850, when it was about the size of two fists, and of a globular form. On the 16th of December, Mr Syme punctured the cyst with a small trocar, and drew off by the canula twenty-four fluid ounces of a dark bloody fluid. The cyst was injected with tincture of iodine with the effect of completely obliterating it.

The contents of the cyst, after standing some time, did not coagulate, but separated into two parts,—1, a dark reddish-brown, finely granular sediment, forming about one-sixth of the whole; and, 2, a turbid whitish supernatant fluid.

Microscopic examination of the fluid, before the deposition of the sediment, showed it to contain very numerous blood-corpuscles, either solitary or cohering together in rolls. A few of these had their edges serrated or more or less irregular, but the majority were quite unaltered. Along with the blood-corpuscles there were a few rounded transparent vesicles, with a diameter varying from  $\frac{1}{1000}$ th to  $\frac{1}{3000}$ th inch. None of these contained a nucleus, but in the interior of each there were several (1 to 10) minute rounded globules, of a brownish colour, and strongly refracting the light. The fluid also contained a considerable quantity of yellowish-brown molecular matter.

From the examination of the fluid, *Dr Murchison* concluded that most of the blood had been effused into the cyst but a short time prior to the operation.

#### MEETING XIV.—May 22, 1852.—Professor BENNETT, P., in the Chair.

1. *Dr Murchison* read the particulars of a case of cancerous disease of the uterus and adjacent organs, and likewise of the liver, in which cerebral symptoms became prominent before death. On post-mortem examination, there was found, besides unequivocally cancerous masses in the above situations, a lesion of the brain, resembling to the naked eye some forms of cerebral softening, but on closer examination, especially with the aid of the microscope, developing peculiarities of structure which identified it with cancer. The softened parts yielded a very peculiar creamy juice, which contained large nucleated cells, of highly complex character, and resembling those found in the decidedly cancerous tumours of the liver, uterus, etc., in this case. The juice containing these structures was in the largest softening surrounded by a delicate vascular wall, inseparable from the cerebral substance; and in the others, the vessels were distributed through the disorganised cerebral matter, giving it exactly the appearance of the so-called "red softening." There was no trace of fibrous tissue in any part of the cancerous substance, which was of a yellowish-white colour, and pulpy consistence. The case had occurred in the female clinical ward of the Royal Infirmary, and the post-mortem examination was made by *Dr Gairdner*. *Dr M.* pointed out the resemblance between this case and another case of cancer of the brain, recorded by *Dr Redfern*, in the "Monthly Journal of Medical Science," vol. xi., p. 510, which had also been remarkable for containing no fibrous tissue. He concluded by making a few remarks on the asserted essentiality of fibrous tissue to the constitution of cancer.

*Dr Bennett* stated, that he had been of opinion that nucleated cells, embedded in a fibrous stroma, constituted the best definition of cancer, until the publication



of Dr Redfern's case. He still believed that the detection of such a fibro-cellular structure would prove most useful in enabling us to separate cancerous from fibrous, epithelial, and other growths. The observation of Dr Redfern, together with the present case, the cancerous formation in the brain of which he had carefully examined, proved that nervous texture constituted an exception. This was the first decided case of encephaloma in the brain that had come under his notice.

*Dr W. T. Gairdner* said, that this was a remarkably satisfactory case, as illustrating the nature of the assistance furnished by the microscope in the investigation of cancer. In performing the dissection, he had been struck with the peculiar character of the softenings, and the creamy juice contained in the largest of them; and considering their occurrence in connection with cancer of other organs, and without the usual accompaniments of cerebral softening—such as disease of the vessels—he had been led from the first to believe the cerebral lesion of cancerous origin, and had afterwards found all doubt on the subject removed by the very peculiar microscopic structure of the softened parts of the brain. It so happened, also, that a similar conclusion was arrived at, quite independently, by Dr Murchison, and likewise by Dr Bennett; while there could be no question that, but for the microscopic examination, it would have been impossible to have formed a clear idea as to the character of the lesion; and anything less than a most careful and minute examination would have been very apt to lead to its being recorded as an ordinary case of cerebral softening.

2. *Dr Cobbold* showed a dissection of the gemmeous dragonet (*Callionymus lyra*).

3. *Mr Barlow* gave an abstract of some observations which he had recently been engaged in making regarding the structure of the kidney in the higher mammalia. Dr Bennett, Dr Gairdner, and Mr Struthers, were appointed to examine Mr Barlow's preparations of these organs, with a view to the subject being again brought before the Society.

4. *Mr Struthers* read a paper by Mr Greig, on a case of malformation of the heart and blood-vessels of the foetus. (This paper was published in the last Number of the "Monthly Journal of Medical Science.")

5. *Dr Murchison* exhibited a microscopic demonstration of acicular crystals of urate of soda, taken from gouty concretions in the joints of the fingers.

#### MEETING XV.—June 19, 1852.—Professor BENNETT, P., in the Chair.

1. *Dr Bennett* showed a morbid preparation, taken from the body of a man, who recently died in the Royal Infirmary. The case (one of thoracic and abdominal aneurism) was published in the last Number of this Journal, together with a figure of the preparation exhibited to the Society.

2. *Dr Cobbold* laid before the Society a brief account of some investigations into the embryogeny of *Orchis*, *Gesneria*, and other phanerogamia. These observations, together with a preliminary account of the labours and opinions of Schleiden, Amici, Brown, Geraud, Griffith, Hofmeister, Meyen, Mirbel, Mohl, Dickie, and about forty others, formed the subject of an essay, written in the summer of 1849. Dr Sanderson, who at the same time investigated this subject, has since published, in the "Annals of Natural History," an admirable memoir on the embryogeny of *Hippuris vulgaris*, the facts there recorded being strikingly confirmed by what the author of this paper observed as occurring in the above genera. From a review of the whole matter, the following conclusions are to be drawn:—

1st, That, prior to impregnation, the ovule always contains an embryo sac.

2d, That the embryo sac is commonly formed at the apex of the nucleus.

3d, That in the interior of the embryo sac there exists a fluid, more or less granular.

4th, That the sac frequently protrudes beyond the exostome (ovule tube,—Griffith).

5th, That in the interior of the sac, prior to impregnation, one or more cytoblasts, or embryonic vesicles, are formed.

6th, That their formation takes place by the aggregation of molecules (Amici, Meyen).

7th, That the cytoblasts, or embryonic vesicles, also contain a fluid more or less granular (globulo-cellular cambium,—Mirbel).

8th, That the pollen is always necessary for fertilisation (apparent exception given by Smith).

9th, That the pollen, when applied to the stigma, sends out one or more tubes (prolongations of the intine), which contain granular matter (fovilla).

10th, That in most cases the union of the pollen tube with the apex of the embryonic sac constitutes the very act of impregnation.

11th, That the result of this union is the formation of an embryo.

12th, That this formation takes place, either by the metamorphosis of one of the pre-existing germinal or embryonic vesicles under the dynamic influence of the fovilla (acting catalytically ?) ; or, as is more probable, by the union of the contents of the pollen tube with that of a germinal vesicle, similar to what occurs in the conjugation of confervæ. When two or more vesicles exist, as in orchis, one only becomes fertilised, the remainder abortive.

3. *Dr Dobie* exhibited under the microscope a preparation of the human spleen, in which arterial twigs were seen passing through the Malpighian bodies, in the same manner as they had been previously demonstrated to the Society by *Dr Sanders*, to pass through the shut sacs in the sheep's spleen.

4. *Mr Drummond* exhibited to the Society a specimen of certain bodies occurring in the sclerotic coat of the cod's eye, and also in the bones of the skull of the same animal. They consist of cell-like bodies, about the  $\frac{1}{3000}$ th of an inch in diameter, round or oval in shape, and flattened like a bi-convex lens. They contained two small bodies, holding the relation of nuclei. They are very distinct in their outline,—not unlike the *torula cerevisiæ*, but less delicate. These bodies are included in cysts, and chiefly occur in the cartilaginous part of the sclerotic coat. Treated with chemical re-agents, they behave in the following manner:—1. Alcohol and æther renders them turbid and granular, apparently from coagulation of their fluid contents. 2. The vegetable acids do not produce any alteration upon them. 3. The mineral acids, even when concentrated, produce little or no effect on the cell-wall, but render the contents obscure and granular. 4. When boiled in caustic potash or ammonia, they are dissolved, and gradually disappear. They seem, from these re-actions, to consist of an external membrane, composed of a substance analogous probably to chitine, containing albuminous fluid.

5. *Dr Sanders* showed Helmholtz's speculum for examining the retina of the human eye, and explained the theory of its operation. (This paper was published in the last Number of the "Monthly Journal of Medical Science.")

## JURY TRIAL.

MR LIZARS, SURGEON, v. PROFESSOR SYME.

Monday, 26th July 1852.—Lord Justice-General, presiding.

A Jury having been balloted, and the Issues read,

Mr ROBERT MACFARLANE: May it please your Lordship—Gentlemen of the Jury, I have the honour to address you on behalf of the pursuer, Mr Lizars, who, as may be known to many of you, has for many years practised as a surgeon in this city. He has been under the necessity of bringing the present action in vindication of his character against the defender, Mr Syme; and I think you will be satisfied, when you have learned the nature of the case, that he had no

alternative but to do so. I think you will be satisfied that a most unjustifiable libel has been published against his professional character and position in society—a libel which it was impossible for the pursuer or any man to remain under, if he had any respect for himself, or if he wished to be respected by others.

Gentlemen, the circumstances are exceedingly simple, and I believe can be laid before you without detaining you many minutes. There appeared in one of the July Numbers of last year (1851) of the "London Medical Gazette," a periodical published weekly in London, an article in the following terms. It is under the head of correspondence in that Journal, and there appears as a letter addressed, I presume, to the Editor or Conductor of that periodical, by the defender Mr Syme. It is published and set out in this Journal in these terms:—"Edinburgh, June 26, 1851.—SIR,—I have only to-day happened to see your Journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner. You say, 'a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,' but you must, or at least ought to know, that I have not addressed a single word upon the subject in question to the so-called 'professor.' \* \* \* \* Within the last eight months I have performed this operation nine times in the Royal Infirmary of Edinburgh, in presence of the largest class of surgical clinical students in her Majesty's dominions. These gentlemen can testify that in no instance has there been bleeding, extravasation of urine, or any other unpleasant consequence, and that all the patients speedily and completely obtained the relief which they desired. As you say that 'something more than the guarantee of Mr Syme's reputation is wanting to assure the surgeon that he would be justified in having recourse to the proposed operation,' I beg to inquire if you think the evidence thus afforded sufficient, and if not, what further proof you deem requisite to establish the safety and efficiency of my operation?—I am, Sir, your obedient Servant, JAMES SYME. The Editor of the London Medical Gazette." And then there is added a statement in these terms by the Conductor of the Journal:—"Certain parts of this letter, which would fall under the English law of libel, have been omitted." Now, gentlemen, in some respects that letter of Mr Syme, the defender, was, to say the least of it, somewhat offensive, in so far as Mr Lizars is concerned; but probably he would not have thought it worth his while to have taken any notice of it, if the matter had stopped there; and you must suppose that, after the significant warning which Mr Syme, the defender, received, that some portions of his letter so addressed to the Editor or Conductor of the "London Medical Gazette" would have fallen under the English law of libel,—I say, after so significant and plain a warning as that, you could hardly have supposed that he or any candid man not desiring to injure his professional brother could have disregarded this warning, and, after weeks of reflection, have proceeded to get the libellous passages, so rejected by this periodical, published in another medical journal, which was more immediately under his own control. One can imagine a person under the irritation of the moment—whether there are sufficient grounds of irritation or not—but labouring under excitement immediately created, may do a thing that he would be sorry for immediately afterwards. But it is not very often, fortunately, that a gentleman in the position of the defender, Mr Syme—a man of European reputation, and occupying the highest status of his profession,—could—after weeks of reflection, having ample time to consider—could deliberately and wilfully proceed to libel his professional brother, and after such a manner. But he did so. And accordingly he then proceeded to have his letter, containing the libellous passages which had been rejected by the "London Medical Gazette," set out and published in another periodical, published here and in London, called the "Monthly Journal of Medical Science." He takes the opportunity of having his letter containing the libellous matter published in the August Number of that periodical, of which, I see from the title page, he (Mr Syme) is one of the Conductors. Now, gentlemen, you have the whole letter set out in the issue before you; and allow me to read it along with you. You see the issue is an admission to the effect, that there was published in the

"Monthly Journal of Medical Science" for August 1851, the following statement:—"The London Medical Gazette." One of the Conductors of this Journal (that was Mr Syme himself) lately felt it necessary to address a letter of remonstrance to the Editor of the 'London Medical Gazette,' who published it in an imperfect form, under the pretext that the matter excluded would have been subject to the English law of libel." This was offensive even to the Conductors of the "Gazette." "Two results have followed; in the first place the letter is rendered meaningless; and secondly, the author is made to appear to have used libellous language. In order that our readers may judge how far this conduct was warranted, we now place before them the letter in its original form—the omitted portion being enclosed within brackets."

(Here follows the letter, as published in the Monthly Journal for August, 1851, p. 198. The portion omitted by the London editor was as follows:—"[\* \* regarding him as long placed beyond the pale of professional respect and courtesy.]")

"In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Mullar, are well-founded; but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth.]"

Now, gentlemen, such then is the nature of the libel of which the pursuer Mr Lizars complains; and I put it to you, as I think I would put it to any intelligent man, whether or not it is not plain on the face of this written and published statement of Mr Syme, that it is a clear and direct attack and libel on the character and professional position of the pursuer, Mr Lizars. Just put the case to yourselves, and that is the true way of judging of the matter. Whatever your particular business, trade, or position in life—if any one occupying the same position, profession, or business, were to publish or do what the defender has published so deliberately of the pursuer,—that he regarded him "as long placed beyond the pale of professional respect and courtesy,"—I would say, gentlemen, does it not necessarily, plainly, and directly follow, that this is a gross and unjustifiable libel; and can there be any doubt as to the answer you must return to the question put under those circumstances—"Whether the whole or any part of the said article or statement is of and concerning the pursuer, and falsely, calumniously, and injuriously represents and holds out the pursuer as a person of disreputable character in his profession, and as neither respected nor entitled to respect in the medical profession, to the loss and damage of the pursuer?" I think, gentlemen, it is only necessary to read the libel to do that. Why, gentlemen, it is all the worse that it is a condemnation of Mr Lizars in general terms. If he had stated the reasons why he considered Mr Lizars as neither respected nor entitled to respect in the medical profession, then the readers of this statement would have been enabled to judge for themselves; but you see perfectly that such a statement as that may cover anything. It is impossible for any man merely reading that, not to know but what Mr Lizars had been guilty of something most atrocious in his professional conduct and character; and it is published to the world. I daresay most of the medical profession of the country, in all parts of it, and many parts out of it, take an opportunity of looking into this "Monthly Journal of Medical Science," and may have seen that, and what would they think of it—or any member of the public, whether professional or not—what would he think if he read such a statement in regard to any man whatever? I think it would be suggested to his mind that there was something very bad. Was it possible for Mr Lizars to remain silent and not wipe off the stain? I will not indulge in any more observations at present—you will be addressed afterwards on the subject. Again, can you doubt that such a libel as that was calculated to do Mr Lizars very great injury and damage? It is impossible to tell what injury it may have already done. No man can tell what injury it may do until the end of his professional career. Think you, if a party who would otherwise, and without any hesitation,

be disposed to call in Mr Lizars to have recourse to his professional aid—after he had seen this statement without knowing that it had been cleared away, and the character of Mr Lizars completely vindicated—think you one moment that he would carry his intention into effect, and call Mr Lizars into his family, either to aid himself or any member of his family—the man who, according to the statement of the defender, had been long placed beyond the pale of professional respect and courtesy? And I say this—take his own account of his position, and who is Mr Syme?—Professor of Clinical Surgery in the University of Edinburgh, and who lectures to the largest class of surgical clinical students in her Majesty's dominions. It adds to and aggravates the measure—I was going to say, the dangerous and injurious effect—of such a libel as this, that it comes from one who stands so high, and who lectures to the greatest number of clinical surgical students—he says, the largest class in her Majesty's dominions—and I daresay every one of his students, to the end of his life, looks with interest on every publication of his. So there are students, the rising profession in all parts of the kingdom, and closely attending with interest to everything that proceeds from him. They, for aught we know, may, one and all, have formed their own conclusions to the serious and palpable injury of Mr Lizars in his character and professional status. Now, gentlemen, if then I am right that this was a libel, and plainly it is a libel, the law of libel is much the same here as in England. Mr Syme was told that his statement would bring him under the law of libel. There a warning was given by a party perfectly neutral: he disregarded that, and deliberately reviewing the article, proceeded to publish it on his own ground. It is a libel here, and it is for you to say what reparation Mr Lizars is entitled to from Mr Syme. Down to this moment this gentleman has made no retraction, independently of injury or damage otherwise to Mr Lizars. You can perfectly understand how painful it must have been, painful to his feelings at the time, and down to the present moment, until entirely cleared away. It is not for me to detain you as to the amount of reparation. That is entirely in your hands; and after you have heard the evidence, and truly and deliberately considered all the circumstances, you will make that reparation to Mr Lizars in the only form he can have it, and which a man in his position deserves, and which I believe will be completely substantiated to your satisfaction.

*Pursuer's Evidence.*

*Dr Alfred Swayne Taylor examined by Mr Deas.*

Q. Dr Taylor, you are professor of Medical Jurisprudence in Guy's Hospital.  
—A. I am.

Q. How long have you been in practice in that hospital?—A. About twenty-nine years.

Q. You conducted the London "Medical Gazette" for some years?—A. I did, from 1845 to 1851.

Q. That included part of 1851?—A. In fact, the whole of 1851.

Lord Justice-General.—1851 inclusive—what No. of process do you now show him?—Q. The number of the London "Medical Gazette," 4th July 1851. Is that the number of the London "Medical Gazette"?—A. It is.

Q. You see a letter there from Mr Syme?—A. I do.

Q. Bearing what date?—A. 26th June 1851.

Q. To whom is it addressed?—A. It is addressed to the Editor of the "Medical Gazette."

Q. And you received it in your capacity of Editor?—A. I did.

Q. That letter in the outset refers to certain statements—observe what it says—"I have only to-day happened to see your journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner,"—and then he goes on to make a quotation from that Number?—A. Yes.



Q. Does that lead you to know the particular article in your journal of 16th May 1851, that he alludes to?—A. It does.

Q. You know Mr Lizars of Edinburgh, by name?—A. I have known him long by name.

Q. Was that article in your journal of May 16th, there alluded to, written by him?—A. No, sir; it was not.

Q. He had no connection with it, I presume?—A. No, no knowledge of it.

Q. You know the writer?—A. I do.

Q. His name?—A. Mr Kesteven, a London surgeon.

Q. The article in the journal of May 16th, was a review, I think?—A. It was.

Q. A review of some particular book?—A. It was.

Q. Had you requested Mr Kesteven to review that book?—A. I had.

Q. Of your own motive?—A. Yes.

Lord Justice-General.—For the “Gazette”?—A. Yes, my Lord.

Q. Does the letter, as it appears there, contain all that the letter itself did?—A. It does not.

Q. When you received the original letter, what did you do?—A. I sent it to Mr Kesteven, as he wrote the review, with a request that he would read and return it to me with any comments he might have to make upon it. He returned it to me without any comment. I read it over carefully, and struck out one or two passages which I thought very offensive.

Q. I ask him, my Lord, to look at the “Edinburgh Monthly Journal” for August 1851, No. 11 of process. Do you observe the passages within brackets?—A. I do.

Q. These were the omitted passages?—A. Yes, they were.

Lord Justice-General.—That is, the passages that you omitted?—A. They are, my Lord.

Q. In what respect did you think those passages objectionable?—A. I thought them highly objectionable as reflecting on the character of a medical gentleman, and that they would involve the publishers of the “Medical Gazette” in an action for libel.

Q. Do you remain of opinion that those passages convey a meaning injurious to the character of a medical man?—A. I do.

Lord Justice-General.—What is the injurious meaning?—A. The injurious meaning, my Lord, I consider is, that it would prevent another medical man consulting him, or recommending any patient to consult him.

Q. What would these words lead you to think, supposing them true—that his character, as a professional man, was reputable or disreputable?—A. Certainly disreputable.

Q. Were you personally acquainted with either Mr Syme or Mr Lizars at that time?—A. No.

Q. You see a note annexed to the article in the “Gazette”—an editorial note?—A. I do.

Q. Did you write that?—A. I did.

Q. I understood you to say, that all you did in that matter was done without any communication with Mr Lizars, or any one on his behalf?—A. I communicated with neither—the only person was Mr Kesteven.

Q. Was your attention called to the letter in the “Edinburgh Monthly Journal of Medical Science”?—A. Yes.

Q. What was the impression made upon you when you saw the whole article there?—A. That it would certainly be very injurious to Mr Lizars’ reputation.

Lord Justice-General.—You say your attention was called to the letter—do you mean in the course of your reading, or by some one?—A. By some one. I only saw it yesterday morning. I had given no thought to the matter, and did not see it before to my recollection. I mean the republication of it entire.

Lord Justice-General.—You had seen the original letter, but not as published



in the "Journal"?—*A.* Yes, I had seen the original letter, but not the republication in the "Journal."

*William B. Kesteven*, examined by Mr Deas.

*Q.* You are a member of the College of Surgeons in London?—*A.* I am.

*Q.* You were some time surgeon to the dispensary at Holloway?—*A.* I was.

*Q.* You are now practising in London?—*A.* I am.

*Q.* You have been practising in London for about fourteen years?—*A.* Yes.

*Q.* Did you write reviews for the "London Medical Gazette" for some years when under the charge of Dr Taylor?—*A.* I did.

*Q.* Look at this Number of the "Medical Gazette" (No. 38 of process),—do you see a letter there from Mr Syme of Edinburgh; and in the outset you see it alludes to an article that had appeared in the "Medical Gazette" of May 16th. You know what that article is?—*A.* Yes.

*Q.* Who wrote it?—*A.* I did.

*Q.* You reviewed the book referred to in it at the request of Dr Taylor?—*A.* I did.

*Q.* Had you any knowledge of Mr Lizars personally at that time?—*A.* None at all.

*Q.* Do you remember of Dr Taylor showing you that letter after he read it?—*A.* He sent it to me.

*Q.* And you read it?—*A.* I did.

*Q.* Is the whole letter there, or some passages omitted?—*A.* Some passages are omitted.

*Q.* Look at this Number of the "Edinburgh Monthly Medical Journal,"—do you see the omitted passages there in brackets?—*A.* Yes, I do.

*Q.* Could you tell me what impression the letter in its original state made upon you?—*A.* I thought it very offensive towards Mr Lizars.

*Q.* Offensive in what respect?—*A.* In those remarks he made use of as regarding him as placed beyond the pale of professional respect and courtesy, and that the statements of Mr Lizars and Dr Mullar were utterly devoid of truth.

*Q.* Look at these words "regarding him as long placed beyond the pale of professional respect and courtesy," What meaning did they convey to your mind?—*A.* That he must be in very bad repute among his medical brethren here, if those words were true.

*Q.* Did it lead you to think that his character in the profession was reputable or disreputable?—*A.* Disreputable.

*Q.* Did it lead you to think that he was respectable in the profession, or the reverse?—*A.* The reverse.

*Q.* Did it lead you to think that he was entitled to respect in the profession or the reverse?—*A.* The reverse.

*Q.* What impression did you form as to the effect of those words on medical men in leading them to consult, or not to consult, with Mr Lizars?—*A.* Why, that, if they were true, medical men could not consult with him.

*Q.* Did it lead you to think, supposing them true, that medical men would, or would not, advise patients to consult him?—*A.* That they would advise them not to consult him.

*Q.* And not to employ him in his profession?—*A.* Yes.

*Q.* Am I right in thinking, that when you read that letter, you considered these expressions calculated to be very injurious to Mr Lizars?—*A.* Very much so indeed.

*Q.* Did you think they ought to be published, or ought not to be published, in the "Medical Gazette"?—*A.* That they ought not to be published.

*Q.* Do you retain that opinion as to the injurious nature of those words?—*A.* Yes, I do.

*Q.* Had the "Medical Gazette" an extensive circulation?—*A.* It had.

*Q.* It does not exist now?—*A.* No.

Q. Was it extensively read by medical men throughout the whole of Great Britain and Ireland?—A. I presume so.

Q. Both in this country and abroad?—A. Yes.

Lord Justice-General.—It is presumed to be read if extensively circulated, perhaps not to the extent of the gentleman who got two copies of a newspaper from Edinburgh, and read them both.

Cross-Examined by Solicitor-General.

Q. When you received that letter of 26th June 1851, of which you published a part, had you understood that a fierce paper war had arisen between two Edinburgh professors, Mr Syme and Mr Lizars?—A. I saw it from the books placed in my hands, and from what I had seen in other journals.

Q. What did you understand Mr Syme to be professor of?—A. I do not remember.

Q. What did you consider Mr Lizars to be professor of?—A. To the best of my recollection, anatomy.

Q. In what establishment in Edinburgh?—A. I do not recollect.

Q. You observe the expression in that letter of 26th June, "I have not addressed a single word upon the subject in question to the so-called professor"?—A. I do.

Q. "Regarding him," What do you understand by that—who regarded him?—A. The writer of that letter.

Q. "Professional respect and courtesy,"—What are the words there that you attach the meaning you do to?—A. The meaning I attach is, that his professional talents and veracity are impeached—that he cannot command the respect of his brethren in Edinburgh and elsewhere.

Q. You think that refers to veracity?—A. I suppose it includes it.

Q. Where is the impeachment of veracity in that part?—A. I take that with the other.

Q. Suppose veracity left out, what else do you consider impeached there?—A. Professional ability and acquirements.

Q. What is the word that you conceive impeaches his professional ability and acquirements?—A. In being long placed beyond the pale of professional respect and courtesy.

Q. What has "courtesy" to do with it?—A. As resulting from his want of courtesy.

Q. Do you think a man of no ability might not be entitled to courtesy?—A. I do not say so.

Q. Has courtesy anything to do with ability?—A. In a professional light.

Q. Explain, if you please.—I think you said that a man of no ability might still be entitled to courtesy?—A. I think that a man in the position of Mr Lizars would be entitled to some courtesy.

Q. You have said that the mere want of ability may not entitle a man to the want of courtesy?—A. I say, that I think a man with great ability and other qualifications should command courtesy.

Q. Suppose a man of great ability had insulted and defamed you, would you consider him entitled to courtesy from you?—A. No.

Q. Suppose he had written a book to abuse you, would you view him with respect?—A. No.

Re-Examined by Mr Deas.

Q. When you said, that if a man who wrote a book to abuse you, you could not view him with respect, I presume you meant to say that you did not deserve the abuse?—A. Precisely.

Q. When you say that a man holding a certain position, if he had no ability, would not receive the same courtesy as if he had—Do you mean your position requires the necessity for ability?—A. Yes.

Q. Might a man be placed beyond the pale of professional respect and courtesy,

in respect of his conduct, apart altogether from his ability?—*A.* He might be.

*Q.* Is there anything in those words “regarding him as long placed beyond the pale,” etc., as showing whether they refer to conduct, or both to conduct and ability, except conjecture?—*A.* No, nothing.

*Q.* Then it would depend on the mere reading what inference he would draw as to what was meant?—*A.* Yes, because the grounds are not stated.

*Q.* Exactly. They might mean something a great deal worse than deficiency of professional ability?—*A.* Certainly.

*Dr Renton, examined by Mr Macfarlane.*

*Q.* Dr Renton, you practise in Dalkeith?—*A.* Yes.

*Q.* You are a member or licentiate of the College of Surgeons of Edinburgh?—*A.* Yes.

*Q.* You graduated here?—*A.* Yes.

*Q.* How long have you practised in Dalkeith?—*A.* Upwards of twenty-five years.

*Q.* I suppose you will be the oldest practitioner there?—*A.* Dr Graham and I are the two oldest.

*Q.* Do you know Mr Lizars?—*A.* Yes.

*Q.* And also Mr Syme?—*A.* Yes.

*Q.* You have been frequently in consultation with Mr Lizars?—*A.* Frequently.

*Q.* And have also frequently sent patients to him?—*A.* Frequently.

*Q.* In surgical cases?—*A.* Yes.

*Q.* Do you know the periodical called the “Monthly Journal of Medical Science”?—*A.* Yes, I do.

*Q.* Are you a subscriber to it?—*A.* I get it regularly.

Lord Justice-General—And read it sometimes?—*A.* Yes, my Lord.

*Q.* Do you remember the article that appeared in it in August last year referring to Mr Lizars?—*A.* I do.

*Q.* Did it come under your observation about the time?—*A.* It did.

*Q.* See if that is the article.—*A.* Yes, it is.

*Q.* Were you a good deal struck by it?—*A.* Yes, very much.

Lord Justice-General—At the time?—*A.* At the time.

*Q.* Do you observe the passage that is within brackets?—*A.* Yes.

*Q.* Commencing with “regarding him as long placed beyond the pale of professional respect and courtesy,” what was the impression conveyed to you at the time you read it?—*A.* The impression conveyed to my mind was that it tended very much to injure the professional character of the gentleman alluded to, Mr Lizars.

*Q.* In what respect did you think it was calculated to injure Mr Lizars?—*A.* It cast a stigma on his professional character as a man—that he was not to be trusted. That was my individual impression.

*Q.* Supposing that were true, or you saw it published of a man you did not know, would you consider that he was in good repute or the reverse in the profession?—*A.* If I had not known the individual, I would have laid it down as a most serious charge, but knowing him as I did, of course it had not the same impression.

*Q.* Supposing you did not know him, would it or would it not have conveyed to your mind that he was in good or bad repute in his profession?—*A.* Bad repute.

*Q.* That he was respected in his profession, or the reverse?—*A.* The reverse.

*Q.* That he was entitled or not entitled to respect in his profession?—*A.* That had the charge been true he was not entitled to respect.

*Q.* Would you consult with any medical man to whom that statement was applicable?—*A.* I would not.

*Q.* Would you recommend patients to go to such a man?—*A.* I would not; as I would wish to do to others as I would wish them to do to me.

Q. And you would not apply for the professional aid of such a man?—A. I would not.

Q. Would you allow any patient for whom you had regard to go to such a man if you had an opportunity of preventing it?—A. I would not allow him to go.

Lord Justice-General—By which you mean that you would dissuade him from going?—A. Yes, my Lord.

Q. You have told us that you knew Mr Lizars, but had it any effect on your mind?—A. I have had such experience of his kindness and unselfishness, and professional ability, that that was the only thing that could neutralise the effect.

Q. Do you know whether that article was talked of a good deal in the profession?—A. Very much talked of.

Q. That Journal was read a good deal?—A. A good deal.

Cross-examined by Solicitor-General.

Q. I think you said that you considered the passage which was read as casting a stigma on Mr Lizars' character—as a man not to be trusted?—A. Yes.

Q. What do you mean by that?—A. I mean that a person against whom such a charge could be made without explanation or retraction could not be a man of honour, and we wish a professional man to be a man of honour.

Q. What is the charge that you understand the passage to contain?—A. That he is not worthy of confidence—that is the charge.

Q. What are the words that appear to you to indicate that he is not worthy? Lord Justice-General—read the words.

A. "Regarding him as long placed beyond the pale of professional respect and courtesy."

Lord Justice-General—You consider that as implying that he is not worthy of confidence as a professional man?—A. That is what I consider the simple meaning when I read it by itself.

Q. What are the words in connection with what appears to be the meaning of the paragraph—"regarding him"—what does that mean?—A. That the writer regarded him.

Q. What does the previous part of the sentence state in connection with that passage in brackets?

Lord Justice-General.—Take time, Dr Renton, and read it.

A. "That I have not addressed a single word upon the subject in question to the so-called 'professor.'"

Q. What does that refer to?—A. To the dispute between them.

Q. Do you understand that the words "regarding him as so long placed," etc., are words containing the writer's reasons going before—the same as if he said *because*—is that the meaning you attach?—A. I have not considered it in that light. I simply take the meaning which I stated to you.

Q. But now that I put it to you—do you think that is the meaning?—

A. I could not state it was *because I regarded* as I read it, or that I only considered that the person had his own reasons for *regarding* it. You may twist it to anything you like, but I cannot tell you what were his reasons. I give you my simple meaning, and that is all I am able to do.

Q. Then you do not take into view the connection between those words and the passage that precedes them?—A. I take the whole thing.

Q. Do you think you understand the passage?—A. I think so.

Q. Is the latter part the reason for not addressing him?

Lord Justice-General.—He does not take that view—he does not form any opinion on that.

*Mr H. Sanderson* examined by Mr Deas.

Q. You are a medical practitioner in Musselburgh, and have been so since when?—A. Since 1817.

Q. Before that time you were in the navy?—A. Yes, surgeon in the navy.

Q. Do you see the "Monthly Journal of Medical Science"?—A. Occasionally.

Q. Do you remember seeing the No. of August 1851, containing the letter of Mr Syme?—A. Yes.

Q. Do you observe the words "You must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy.]"—What impression did these words make upon you when you read them?—A. The impression they made on my mind was that they were very damaging to Mr Lizars' professional character, and not exactly called for with regard to his character.

Q. How not exactly called for?—A. That Mr Lizars' character did not warrant the imputation.

Q. What do you understand the imputation to mean?—A. I understand that he was not a respectable practitioner, and had in some way or other dishonoured himself.

Q. And dishonoured himself to the extent of not being entitled either to professional respect or professional courtesy?—A. Yes.

Q. Have you been in the habit of meeting with Mr Lizars in consultation?—A. Yes, I have.

Q. And of sending cases to him when surgical operations were required?—A. I have not been much in the habit. I perform my own operations, if I may so speak; but I have been in the habit of meeting him in consultation.

Q. If you had believed this statement to be true, would you have continued to meet with him in consultation?—A. Certainly not.

Q. Was that letter much talked of in the medical profession at the time it was published there?—A. Yes, I should say it was.

Cross-examined by Solicitor-General.

Q. There is the word "regarding," To whom do you understand that to refer?—A. I would suppose the word means that Mr Syme, the writer of the letter, regarded him.

Q. That was the expression of Mr Syme's feeling?—A. Yes.

Re-examined by Mr Deas.

Q. You say that you understood that that was the light in which Mr Syme regarded him?—A. Yes.

Q. Did you understand that Mr Syme does or does not hold him up to other people in the same light?—A. Oh! I understand that perfectly.

*Dr Sibbald*, examined by Mr Macfarlane.

Q. You are a medical practitioner in Edinburgh?—A. Yes.

Q. You are a member of the College of Surgeons?—A. I am.

Q. How long have you been in practice in Edinburgh?—A. For fully thirty years, and I have been twenty years a member of the Royal College.

Q. Do you know Mr Lizars?—A. Yes.

Q. And also Mr Syme?—A. Yes.

Q. Have you been in the practice of consulting with Mr Lizars?—A. I have.

Q. In serious cases?—A. In important cases.

Q. Have you also sent patients to him?—A. I have.

Q. Are you a subscriber to the "Monthly Journal of Medical Science"?—

A. I am not a subscriber, but I purchase it occasionally, and peruse it frequently.

Q. You frequently see it?—A. I do—it is a matter of interest to all medical men.

Q. Do you remember an article appearing in the August No. of last year, referring to Mr Lizars?—A. Perfectly—(identified article).

Q. You had seen that article after its publication?—A. I cannot say how soon, but it was a matter of conversation.

Q. Was it a good deal talked of in the profession?—A. Yes, a good deal.

Q. You see the passage within brackets beginning “regarding him as long placed beyond the pale of professional respect and courtesy”—what impression did that convey to your mind?—A. It is very strong language indeed.

Q. If you saw that published of a medical man with whom you were not acquainted—whose position you did not know, what would you understand by it?—A. It would depend very much on my knowledge of the party who wrote it—if the person who wrote it was of mark and high respectability in the profession, it would certainly go far to extinguish my having any respect for, or confidence in, the man who was so spoken of, if I had no other means of knowing anything as to the party—I mean the professional ability and character of the party so spoken of.

Q. Do you consider Mr Syme as a person in high position and repute?—A. Assuredly I do. If the court would allow me to make an explanation, I have the highest respect for the character and status of both parties, and I come here from a sense of duty.

Q. If you believed the statement “regarding him as long placed beyond the pale of professional respect and courtesy,” would you consult with such a person, believing it?—A. Believing it, I assuredly would not.

Q. Or would you have any confidence in him?—A. Certainly not. I would not feel justified in naming him to any patient, if I believed that to be true.

Q. I need hardly ask you, whether you consider expressions of that kind, applied to a professional man, would be injurious?—A. I would consider that such expressions, applied to myself, would destroy me in the estimation of any right thinking man.

Q. Do you happen to know, Dr Sibbald, whether this periodical, the “Monthly Journal of Medical Science,” is read a good deal in the profession?—A. Yes; it is a highly respectable publication.

Q. You have got the number for August 1851 there—look at the title page and read the names of the gentlemen who are said there to be the conductors of the Journal?—A. Professor Christison, Professor Syme, Professor Simpson, Professor Bennett, Dr Douglas Maclagan, and Dr William Robertson. The two latter are not Professors, but highly respectable men in the profession.

Q. These are all men of standing in the profession?—A. Yes.

#### Cross-examined by Solicitor-General.

Q. You state, that you consider the passage there, “regarding him,” etc, as reflecting on Mr Lizars’ talents and ability?—A. I think both morally and professionally—“professional respect,” it says.

Q. Was it reflecting professionally in your opinion—does it reflect on his skill and ability as a professional man?—A. I have no hesitation in saying that I read it in that sense.

Q. Now, besides reflecting on him professionally, from what word do you consider it reflected on him morally?—A. Because I consider him a member of an honourable profession, and I hold that a man would be considered a quack, dissembler, impostor, and therefore unworthy of respect,—a dishonest man, in short, who was beyond the pale of professional respect, and unworthy of the confidence of his professional brethren.

Q. What do you derive confidence from?—A. Combination of both courtesy and respect. Courtesy is weaker by itself. Courtesy might refer to Abernethy, or some eccentric character. In important cases of life and death I would put all that aside. I should say that we have all heard of the eminence of the late Liston, some might think him deficient in courtesy. In cases of life and death I would, however, have called him in, to a person of the most refined feelings, to a lady of exalted rank.

Q. You think there is a reflection on Mr Lizars’ courtesy here?—A. Yes.

Q. But you do not attach so much consequence to that as to the other expres-



sion?—*A.* I would not like it, but I do not attach so much importance to it, although it reflects on Mr Lizars' courtesy.

*Q.* Perhaps a reflection on Mr Liston's courtesy would not have done him much harm?—*A.* No, not after his character was established.

*Q.* You have been a long time in Edinburgh?—*A.* Yes.

*Q.* Would you, from the position of some of the medical men here, be much surprised that there was a want of courtesy among them?—*A.* I would not have been here to-day if there had not been a want of courtesy.

*Q.* From what you knew, and before reading that letter, you would not have been surprised to know that there was no great feeling of courtesy between Mr Syme and Mr Lizars?—*A.* I would not have been surprised. I might have regretted it, but I would not have been surprised.

*Q.* "Respect" you think a more serious matter than courtesy?—*A.* Yes; more serious.

*Q.* In what connection is this passage, "so long placed,"—who is it that so regards Mr Lizars?—*A.* Mr Syme, the writer, and he had such an authority that it would influence others.

*Q.* In what connection do you consider that is used—has it anything to do with what goes before—is it assigned as a reason?—*A.* He states it in connection with what goes before.

*Q.* What do you understand by that?—*A.* Previous transactions between the parties—previous controversies or differences between the parties—and that the opinion the writer has formed he thought it his duty to state.

*Re-examined by Mr Macfarlane.*

*Q.* Although you might be grieved, probably you would not be surprised, at a little want of courtesy to any professional man who ———

This question objected to by the Court.

*Q.* Assuming you did not know anything about those parties, and had on that ground to scan the statement as to a professional man, that he was regarded as "long placed beyond the pale of professional respect and courtesy,"—Would you consider that that was holding out the individual alluded to as unworthy of respect?—*A.* Most assuredly. I never could consult with such a man.

*Professor Miller, examined by Mr Deas.*

*Q.* You are professor of Surgery in the University of Edinburgh?—*A.* I am.

*Q.* Look at that letter in the "Monthly Medical Journal of Science" for August 1851—Did you read that at the time of its publication?—*A.* Yes.

*Q.* Look at that part which states, "you know, or ought to know, that I have not addressed a single word upon the subject in question to the so-called 'professor,' regarding him as long placed beyond the pale of professional respect and courtesy."—What impression did these words make on your mind at that time? What did you think was the meaning intended to be conveyed as to the person spoken of?—*A.* Something decidedly disreputable.

*Q.* You thought it implied that he was a disreputable character in his profession—am I right?—*A.* Yes.

*Q.* Did you think it implied that he was respectable or not respectable?—*A.* Not respectable.

*Q.* Did you think it implied that he was entitled to respect in his profession, or that he was not entitled to respect?—*A.* Not entitled.

*Q.* Did it appear to you at this time to be deeply injurious to Mr Lizars or not?—*A.* Certainly calculated to be injurious.

*Mr Highley, examined by Mr Macfarlane.*

*Q.* What is your firm in London?—*A.* Highley & Son.

*Q.* You are medical publishers there?—*A.* Yes.

Q. Are there any other houses in London proper medical publishers but yours?  
—A. About *four*, strictly medical publishers.

Q. Are you one of them?—A. Yes.

Q. Do you know the "Monthly Journal of Medical Science"?—A. Yes.

Q. Do you remember of an article that appeared in the course of last year, referring to Mr Lizars of this town?—A. Yes.

Q. Had you occasion to see that article?—A. I am in the habit of looking to the medical journals as they appear, and I noticed it at the time of publication.

Q. Did you see the passage within brackets?—A. Yes.

Q. What impression did it convey to your mind—that passage?—A. It conveyed to my mind that his conduct in some way or other had been such, that he had failed of professional respect, and that he had been guilty of some unprofessional act.

Q. Did it convey to your feelings that he was in good or bad repute in the profession?—A. It would convey to my mind that he was in bad repute.

Q. You had known Mr Lizars, had you?—A. Yes.

Q. Now, suppose that you read that passage in that periodical as applicable to a professional man whom you did not know, or with whom you were not acquainted,—Would you be disposed to deal with him?—A. From an after-passage coupled with that, I certainly would not.

Q. What is the after passage?—A. "In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Mullar, are well-founded; but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth." It was likewise from having known of the nature of Professor Lizars' work on stricture.

Q. Were you struck a good deal with that article when you saw it first?—A. I was surprised a good deal.

Q. Was it talked of a good deal in London?—A. Several men noticed it at the time.

#### Cross-examined by Solicitor-General.

Q. You said you knew Professor Lizars, and about his book on "Stricture"?  
—A. Yes.

Q. Would you look at that, No. 27 of process, Mr Lizars' book?—A. Yes, I see it.

Q. Published by his brother here, and by you in London?—A. Yes.

Q. That is the book that you said you referred to before?—A. Yes.

Q. Did you ever read that book?—A. I have skimmed over it.

Q. Very amusing?—A. It is.

Q. That is the second edition?—A. It is.

Q. Look at the preface?—A. I think it appeared in March 1851.

Solicitor-General.—My Lord, you will see the object of reading that preface, if you look to the second article of the condescendence:—"The pursuer, early in the present year (1851), published a treatise of which he was the author, entitled 'Practical Observations on the Treatment of Stricture of the Urethra, and *Fistula in Perineo*, illustrated with cases and drawings of these affections;' and a second edition of this treatise has also been since published by and for the pursuer." This is the work to which the letter appearing in the "London Medical Gazette" of 4th July 1851, applies. It is of no consequence by whom the passages in this book are read; but I mean to give them in as bearing on this controversy,—I mean, by referring to the book, to instruct that that book was one which throws light on the meaning I attach to the words in the letter, and that it gave rise to the letter in question; and showing that it was not an unprovoked attack.

Lord Justice-General.—And therefore showing mitigation?

Solicitor-General.—So far as necessary.

Lord Justice-General.—Any objection to that, Mr Deas?

Mr Deas.—I think the Solicitor-General should in the first place explain a little more narrowly how any passages in that book are to throw light on the meaning he attaches to the words of this letter—how any passage in the book published can show in what light Mr Syme regarded the words when he said—“regarding him as long placed beyond the pale of professional respect and courtesy”—how it is to instruct that I do not understand.

Solicitor-General.—I explain in this way—I think it has been made out by the witnesses of the pursuer himself that this letter now said to be a libel, is to be taken in connection with what is contained in the whole article. The letter begins, “I have only to-day happened to see your Journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner.” Now that journal refers to this book, as the pursuer says in the third article of his condescendence, and the witnesses the pursuer has adduced connect the paragraph of the letter of 26th June referred to as in connection with the article. It goes on—“You say ‘a fierce paper war has arisen between the two Edinburgh professors, Syme and Lizars;’ but you must, or at least ought to know, that I have not addressed a single word upon the subject in question to the so-called ‘professor.’” Now, in order to understand the meaning of that passage the subject in question must be understood. The subject is that contained in this book of Mr Lizars’ and another of Mr Syme that I also propose referring to. I apprehend, in order to understand that sentence, we must know the subject it alludes to. It is the discussion of stricture in the urethra contained in these two books—particularly contained in Mr Lizars’ book. I want to show that, and may explain that these words in brackets have reference to the controversy, and are given as a reason why no direct reply was addressed to Mr Lizars on the subject. Then my learned friends have also introduced this next paragraph; and the witnesses they have adduced speak to those allegations in that book. And farther, if your Lordship will look to the article in the revised answers—

Lord Justice-General.—Before you go farther, I may complain that I have not got the papers on which the record was closed.

Solicitor-General.—That is an omission, my Lord. But you will see the 31st statement, page 20, and what is stated there. 36 and 37 are also the statements going to show the light in which I regarded this passage in the letter.

Lord Justice-General.—The question is not as to its value, but as to its admissibility.

Mr Deas.—I submit, my Lord, that it is clearly out of the question. The issue is:—“Whether the whole or any part of the said article or statement is of and concerning the pursuer, and falsely, calumniously, and injuriously represents and holds out the pursuer as a person of disreputable character in his profession, and as neither respected or entitled to respect in the medical profession,—to the loss and damage of the pursuer?” That is the issue. Now it is of no consequence to that question—What was the meaning which the writer of the letter may have intended by those words? The question—Whether the works are calculated to explain or throw light on the libel? is a different matter; one may know nothing about either of the books. Now, how can it be fairly said that the book shows the meaning in which the words are used in this letter? I submit that is out of the question. I submit that is not the purpose, and that you will be chary of allowing a party for one purpose to get in a thing under pretence of another. The more material thing is the next ground, which is this,—that the book conveys an unprovoked attack on Mr Syme, and gave rise to the attack made by him on Mr Lizars. That is the plea of previous provocation, and not very intelligible, as applied to this case. But, in the first place, it ought to have been stated on the record, so as to put us in the position of coming here with a fair notice, that that defence is to be maintained; for you will read that record from beginning to end—and I am sorry your Lordship had not an opportunity of doing

it more deliberately—you will see that not one word of provocation is in it. There is no statement whatever to the effect—that anything in the book of Mr Lizars gave rise to the attack of Mr Syme on Mr Lizars. There are plenty of statements contained in the record down to article 23. In that article you find the use, and the whole use, intended to be made of this statement. The defender submitted that the statement was perfectly justifiable. The article sums up in a few sentences everything that is previously stated in the record, under every statement, and then states that this fully justified the statement “regarding him as long placed beyond the pale of professional respect and courtesy.” Then we turn to the defender’s pleas in law at page 25. 1st. That the action is not relevant; 2d. That the expressions are not libellous. You have therefore a plea of justification,—you have a complete foundation for an issue in vindication,—you have no such issue taken, and, as I am informed, no such issue asked for. Then, if they had chosen to follow out what is in this record, they ought to have taken their issue in justification—that would have been involved. We would then have been in a position to show that it was not justifiable. But here we have been allowed to conclude our evidence, all except a single witness, on a matter of form, without a single word that implied an issue in justification; and the proposition is in mitigation of damage. My first observation is, that that is not under the record—you must infer it on the part of the defender. I need not say that this is a thing to be very jealous of, although even in mitigation. When stated in the record, it ought to be let in. On the other hand, your Lordship will be very careful, under the judicature act, to allow an issue to come in, as in justification, when there is no issue taken in justification. It comes to this, that a wrong has been done; and it is alleged, if I may say so, that one illegal thing has provoked another. Is that not a special plea or defence that ought to have been stated? The party says I wrote a libel,—it was quite justifiable,—it was provoked by a wrong done me. Is he entitled to mislead me in this way by his own record, and to say that I shall not read that record, as not justifying, but that it is perfectly justifiable? and I am prepared to prove, that the thing is justifiable in itself. How can he say that the personal injury I inflicted on him gives rise to this injury he has inflicted on me? I submit that will not do. Look to the nature of the plea. It is alleged that Mr Syme merely thought, but did not mean to represent, Mr Lizars to the world “as being placed beyond the pale of professional respect and courtesy.” Is that to be allowed? If he set forth in this record that Mr Lizars had published a book some years before—because this work in 1851 is a second edition—when the first edition was published we don’t know—but I say had he been going to make out that a book was published nineteen years ago, containing an attack on him which provoked him in June 1851 to make an attack on Mr Lizars; might he not have said so in this record, and then we could have discussed the relevancy—whether a matter of that kind might be pleaded, and in justification of a libel of this description deliberately published—not words spoken in consequence of other words spoken at the time, but a published libel—deliberately published—published to the world in cool blood; and to say that some attack on his character years before provoked that, and that he was not to be liable in damages. I submit that that is matter that ought to have been set forth; and if set forth, I do not think it would have been relevant on the record. But to propose, *per saltum*, to read passages from that edition published in 1851—but the difference between it and the first edition we don’t know—is a different thing. There might have been an attempt on their part to put in that intermediate review of that work published on 16th May 1851. They have not put that in; but they pass that over, and propose to go back, *per saltum*, to these passages of that work of Mr Lizars in order to show that those passages were the inductive cause that gave rise to that attack. I read this record as setting forth the review, and that it was this review that gave rise to this attack. The letter bears that out. Can he contradict his own letter? The letter, I say, bears it, and he there quotes the words of the review. There is the inductive cause of the libel stated by

himself deliberately on that record. It may have been under the impression that Mr Lizars had written the editor of the review on the subject. If that was ever in his (Mr Syme's) head I don't know, but it is distinctly proved that Mr Lizars had nothing to do with it and knew nothing of it. But, be that as it may, he states that the inductive cause was that review in that journal of 16th May. Admittedly, even the second edition is published months before that, and does not lead to that libel; and the first edition was published long ago, and it does not lead to this libel, on his own showing. The thing is this review, according to his own statement in the record, independently of any statement in that book, the review—as the Solicitor-General stated—gave rise to this attack; and if that is the case, will you allow—under cover of doing a thing that is not set forth in the record—that is a pretence set forth for a totally different purpose—the defender to lay the foundation for another purpose? That would not be fair play. I am not willing to make any technical objections. I do not wish to exclude anything going to the proper evidence, but I submit this attempt is contrary to good faith and the justice of the case.

The Solicitor-General.—I submit I am plainly entitled to have this book put in evidence. I think it is somewhat strange that the pursuer, who libels on books of his own, should now object to that book being put in evidence. Look at his statement in the adjusted revised condescendence No. 2.—“The pursuer, early in the present year 1851, published a treatise of which he was the author, entitled ‘Practical Observations on the treatment of Stricture of the Urethra, and *Fistula in Perineo*, illustrated with cases and drawings of these affections;’ and a second edition of this treatise has also been since published by and for the pursuer.” Has that anything to do with the present case? I submit, it plainly has. In article 3 of condescendence he goes on—“The pursuer’s treatise now referred to was favourably reviewed or noticed in the ‘London Medical Gazette or Journal of Practical Medicine,’ of 16th May 1851, a weekly periodical in extensive circulation, and of considerable repute and standing. The said review or notice of the pursuer’s treatise also contained some observations in relation to a treatise some time ago published by the defender, Professor Syme”—I am perfectly willing that he put in the second edition and the first edition of the defender’s own book if he wishes it—“On stricture of the urethra, as well as in relation to the mode of treatment or operation practised by Professor Syme in cases of stricture of the urethra, the efficacy of which treatment or operation was said to be rendered doubtful, in consequence of the statements and explanations in the pursuer’s treatise, and in a pamphlet on the same subject also recently published by Dr Mullar of Edinburgh.” Article 4 of condescendence follows—“In the Number of the said ‘London Medical Gazette or Journal of Practical Medicine’ of 4th July 1851, there appeared a portion or portions of a letter addressed to the editor thereof, by the defender Professor Syme, purporting to be of the nature of the vindication of the mode of operation or treatment practised by him in cases of stricture of the urethra, and alluding to the pursuer in very unjustifiable terms. And in an editorial note annexed to Professor Syme’s letter, inserted, as now explained, in said periodical, it was stated that ‘certain parts of this letter,’ meaning the defender Professor Syme’s letter, now alluded to, ‘which would fall under the English law of libel, have been omitted.’” Now, in what respect does it allude to the pursuer, except in reference to the subject raised by his own book? Condscendence 5 proceeds—“Notwithstanding the warning or caution thus administered regarding the libellous nature of the defender Professor Syme’s foresaid letter, or certain parts thereof, there appeared in the ‘Monthly Journal of Medical Science,’ which was published in or for Aug. 1851, an article in the following terms, viz.” That is the statement he makes. Now, my Lord, I say that within the limits of this record, it is impossible to exclude me from bringing in the matter that the pursuer refers to as the history of this matter. He states that he published a book, that it was publicly noticed, and made the subject of comment; and I am perfectly surprised that the pursuer



should close his own case without putting in his own book. Then comes the publication of this book, with the advertisement, all of which I mean to found on as a series of attacks on the defender. Then the statements contained in the 2d edition are fully stated, and also in the 1st edition. Then in February 1851, the pursuer published his treatise on the treatment of stricture, etc. Now, it is said that all this would lead to what? That there is no plea that this was provocation. Suppose it comes to this. There are two grounds. Is it possible that the defender's letter can be intelligible without reference to the dispute between the parties? But in the next place, observe what the defender says. He says I have long been entitled to consider Mr Lizars as placed beyond the pale of professional respect and courtesy. I am therefore surprised to hear it maintained that it is of no consequence what meaning the defender attaches to his words, when the issue is that they are *calumniously* published. It may or it may not be conclusive—but suppose that the *animus* attached by the defender is totally different from that attached by the pursuer—is that not relevant? Can it be said that it is solely confined to the question of the meaning that other parties attach? It is, I submit, whether the defender had *animus injuriandi*. We deny the interpretation they put upon it; but allege that the circumstances which had taken place between the parties at an earlier period, justify the words. He, the pursuer, says justification can only lead to an issue in justification. It by no means leads to an issue in justification. And in the next place, it may most materially affect the question whether any damage, or what damage, has been made out. It is not necessary to plead expressly in mitigation of the things so alleged. The things are unintelligible without going back.

Lord Justice-General.—The latter part of your argument comes to this, that the real position of the parties is—that the defender did regard the pursuer as beyond the pale of professional respect and courtesy, so far as regarded himself. That is what you are going to prove.

Solicitor-General.—That is not justification of the libel.

Lord Justice-General.—What is it then?

Solicitor-General.—An explanation of the pursuer's own conduct.

Lord Justice-General.—How, with the facts that you submit regarding him—providing that that is your meaning—can you maintain that? because, observe, if you represent him as beyond the pale of professional respect, that necessarily implies that you so regard him. Now, how will it limit the meaning of the accusation that you would answer in point of fact, that you so regarded him? It is assumed, on the broader meaning, that you so regard him.

Solicitor-General.—My position is this—his conduct might be such towards me as to make me regard him as not entitled to respect; that I am warranted in forming that opinion in so *regarding* him as *regards* myself. Is that not in reference to this case as a *probabilis causa*, legitimate in any question of this nature?

Lord Justice-General.—How far back do you propose to go? for this record goes back to 1840, being the whole history of these two gentlemen for ten or twelve years back.

Solicitor-General.—If your Lordship will have the kindness to look to the letter.

Lord Justice-General.—Suppose it had been that you had long regarded him as not an honest man, and the issue had been put, and that you offered to prove that you had good reason for so regarding him.

Solicitor-General.—That is not the matter.

Lord Justice-General.—Very near it.

Solicitor-General.—Before I go into it, allow me to refer to statement 24. What I submit to your Lordship is this, that the whole part of the *res gesta* occurring *de presenti*, which in written matter may be very different matter, is necessarily a part of the proceedings as regards understanding the position of the parties. I take the pursuer's own statement, which he published on record, about the publication of his book—that book being, as I allege, an attack on Mr Syme,



and a reason for at least not inducing him to respect Mr Lizars otherwise. That is what I undertake to prove. As part, therefore, of the *res gesta*, extending over a legitimate period of time, and embraced by the pursuer in his own statement, I submit I am entitled to introduce it; and 2d, I submit I am entitled to introduce it as quite sufficiently recent to entitle me to found on that—that the words contained in that book may be taken as explaining the feeling under which I wrote that letter.

Lord Justice-General.—Is what happened in 1840 a recent thing?

Solicitor-General.—I am not come to that; but I can show how that statement as to 1840 is made a recent thing, because the second edition of the pursuer's book is written in a strain of the most grievous character as regards me, and revives the matter of 1840; but while this was going on, and what those parties narrate, when they speak of "a fierce paper war," that letter is printed in August 1851.

Mr Deas.—That is laying the foundation of what took place in 1840. But my learned friend now says that he means to show that he, Mr Syme, did regard Mr Lizars as placed beyond the pale of professional respect and courtesy; and in the second place, that he was warranted in so regarding him. These are the two things on which he means to found. Now it is very plain that nothing in that book can show that Mr Syme did so regard him. It may show something of a reason, but it cannot show that he did so regard him; and it is to show the reason that he was so warranted in so regarding him. That is coupled with this, that the statement on the other side is that the whole meaning of the libel is that Mr Syme himself so regarded Mr Lizars. That brings us to this, that the object is to prove the *veritas* of the libel.

Lord Justice-General.—The object is to prove that that is the whole meaning of the libel—that that is reality, while the other was not—that he was warranted in arriving at the conclusion, and that that was the true meaning.

Mr Deas.—One of the statements was that he was warranted in holding the opinion he did; in the second place, to show that that opinion was justifiable.

Solicitor-General.—No; that that was the natural result.

Lord Justice-General.—It was to show that there were two meanings,—one consistent with the natural state of facts, and the other conjectural.

Mr Deas.—It is maintained what is contained in the book was so expressed as to place Mr Lizars beyond the pale of professional respect and courtesy. Now it is quite clear that that is proposing to prove the *veritas* of the whole libel as they construe it. I do not care if they do that. Mr Syme says, In my opinion Mr Lizars has been long placed beyond the pale of professional respect and courtesy, and so he holds him out to other people. Are you entitled to justify the view that Mr Syme has expressed, as regards himself, by reference to any work that Mr Lizars has published, any more than you are entitled to justify the view that he has held out to the world? If they meant to say that Mr Syme regarded him in this light, and was justified in regarding him—is that not just a plea in justification, and a proof of the *veritas* as far as it goes? He justifies the whole thing. Now, in the shape of mitigating the damage, are we going to allow that which wipes away the whole libel, and therefore that there is to be no damage at all? That cannot be done; he is not entitled to turn round and put the thing in that shape. That is a justification. I may refer your Lordship to the case of M'Neil and Robertson, where it was proposed to put in a letter in mitigation of the damages. It was held that the party was not entitled. (Counsel here read from report of case opinion of Lord Moncreiff rejecting the admissibility of the letters in that case to mitigate the damages, these letters not having been used in justification of the libel). Can your Lordship desire a better proof that the party here does consider it amounts to a justification than what the Solicitor-General states, that so far as Mr Syme was concerned, that letter was justifiable, and Mr Lizars was placed beyond the pale of professional respect? He acknowledges that he, Mr Syme, was so entitled to consider him; and if a man in Mr Syme's position was entitled to consider him, would not the

world? That is not under cover of a mitigation of damages, but is under cover of justification. Our statement is, that at the period we published our book he, Mr Syme, had not that review in May 1851. All, therefore, that is stated by us is simply, that it is proved by the witnesses that there is a review in the "London Medical Gazette" on 16th May 1851, of a certain work which had been published by Mr Lizars, and that on the back of that review Mr Syme writes this letter to the editor of that journal. Now, is that going into this book—the mere mention that there was such a book, and that there was a review? I submit that nothing is made there on our part, or brought out in our proof, to lay the foundation of anything like what the defender is contending for. He cannot go into the whole contents of that book, in order to show that he was entitled to regard Mr Lizars as here expressed. To go back twelve years in order to show what was the inductive cause of this attack on Mr Lizars in June 1851, when Mr Syme tells you that the reason was that a certain article appeared in a certain publication, is not competent.

Lord Justice-General.—You say you purpose to put in the review of 16th May 1851.

Solicitor-General.—We do.

Lord Justice-General.—Is it the first or second edition which was reviewed?

Solicitor-General.—The first edition, my Lord.

Lord Justice-General.—Does the review of 16th May review those passages to which you refer? That review cannot possibly have referred to anything in the second edition of the work, because at the time of the review the second edition was not published.

Solicitor-General.—There was a second edition published in March 1851, and the review is in May 1851.

Lord Justice-General.—But the passage, "You must, or at least ought, to know, that I have not addressed a single word"—that has reference to the observations of the reviewer—and the subsequent words, as having regarded him as long placed beyond the pale of professional respect and courtesy; that is a reason for taking no notice of his book. That is not by reason of anything in the book.

Solicitor-General.—But it is said they ought to have known how matters stood between the parties.

Lord Justice-General.—The man who reviewed "Peter's Letters" never saw the first edition.

Solicitor-General.—We maintain that he had continuous ground for so regarding him down to the period of the review. My principal object is to negative the construction of the charge *disreputable*.

Lord Justice-General.—How does that negative the construction?

Solicitor-General.—If I regard him as placed beyond the pale of professional respect and courtesy, in respect of private matters between me and him, calculated to make me cease to regard him and cease to treat him with courtesy, it exhausts the words in the letter, and leaves no room for that other construction in regard to his professional character which they maintain is held out as *disreputable* by Mr Syme. I, Mr Syme, say I am not looking to general professional character. I, Mr Syme, say that Mr Lizars was attacking me for a long time, and his attacks were such as made it impossible to treat him with that courtesy which I might have rendered to another.

Lord Justice-General.—Is that not more personal than professional?

Solicitor-General.—It is only in so far as regards his personally writing surgical books that I, Mr Syme, have to do with him—in regard to the discussion of those professional subjects.

Lord Justice-General.—That is to say, he has used *me* very ill.

Solicitor-General.—Yes, my Lord, that is it.

Lord Justice-General.—Are antecedent facts generally important towards the construction of an expression, judging of the accuracy of construction put upon words—not the repetition of the words themselves—but the construction put

upon them by substituting other words in their place. Now, are these not antecedent facts, Mr Deas? They are put on that construction.

Mr Deas.—I do not see how anything contained in this book explains the meaning of the words used when Mr Syme says that he had taken no notice of that book, because he had long regarded Mr Lizars as placed beyond the pale of professional respect and courtesy.

Lord Justice-General.—It is not this book alone—we are now dealing with those matters going back to 1840. If warranted in regard to this book, it is warranted, I think, all the way back—that such had been the relation of those parties.

Mr Deas.—The first edition of the book is reviewed, and the reason he gives for not noticing it, is not anything contained in the book, but because he had long regarded him as placed beyond the pale of professional respect and courtesy. That excludes the book. If such an inquiry is competent at all, what would have been the difference, supposing he had taken an issue in those terms—whether the pursuer had for twelve years so conducted himself as to entitle the defender to consider him so placed? Is that not the very thing that would be allowed to go to the jury? If they prove that, don't they prove the justification? Why may it not be any one thing that Mr Lizars ever did, as well as this book—why not that he was not disreputable in a medical sense as being a quack, but dishonest in every other sense? They may prove anything. It seems to me the most dangerous proceeding to go into this inquiry—without taking an issue in justification to go into the proof of all the things they say justified Mr Syme in writing this libel.

Lord Justice-General.—The circumstance of those passages being in the book, appears to me to be not important in his view, because he proposes to go back to matters that might not be in that book if they were antecedent. Therefore the book has no special relevancy, but any circumstances that placed the parties in such an antagonistic position that they could have no respect for each other. The defender wishes to show not only that such was that position, but *why* it was the position of the parties, and to show that he was justified in entertaining that opinion—and if he was justified in entertaining that opinion, it might also be the opinion of others. I think I cannot allow this. It might be perfectly competent for the defender to bring a separate action on those matters, if so advised.

Solicitor-General.—I tender separately the recent books, as in mitigation of damages.

Lord Justice-General.—I do not allow it in that sense, either looking to the terms of this letter, or the general principle. I am willing to put in the review of 16th May, and the first edition of the pursuer's work.

Solicitor-General.—I also wish a note of the names on that book (the *Monthly Journal of Medical Science*), of those who are stated to be the conductors of the Journal.

Lord Justice-General.—You have that in Dr Sibbald's evidence.

*Evidence resumed.*

*Henry Bowie*, examined by Mr Macfarlane.

Q. You are secretary of the Philosophical Institution of Edinburgh?—A. Yes.

Q. Is the "*Monthly Journal of Medical Science*" taken in there, and lies on the table?—A. Yes.

Q. Are the reading-rooms attended by a great number of persons?—A. At certain seasons more than others, but they are generally very full.

Q. But how many subscribers have you to the Institution?—A. At one time with the other, I would say about 2000.

Q. That number includes persons in various walks of life, professional and unprofessional?—A. Yes, both descriptions.

The pursuer here closed his case by putting in the documents on which he founded.

**Mr GEORGE PATTON.**—Gentlemen of the Jury, I have now to address to you a few words in explanation of the case of the defender, Professor Syme, and I think I shall best discharge my task by entreating your attention particularly to the terms of the issue which you have to try, and the terms of the documents out of which that issue has been extracted. The charge made by the pursuer against us is of this description and character, that he (Mr Syme) falsely, injuriously, and calumniously held forth the pursuer as a person of disreputable character in his profession, and as neither respected nor entitled to respect in the medical profession. The pursuer has undertaken to prove an issue which is contrary to the statement all along made on the part of Mr Syme, of his intention or purpose in the use of the expression on which that alleged libel is founded. He has undertaken to prove that it contained a false, injurious, and calumnious imputation, going, according to the various interpretations of his witnesses, to charge a want of professional ability, a want of veracity, and, at all events, of professional character. That is a grievous charge, and you will not readily adopt it—although you may find parties who say that the words bear that construction,—but, above all, you will not adopt it on the principle on which some of those witnesses have proceeded. What I state as your duty, and I have no doubt you will willingly discharge it, is to look at the whole document before you to see the true statement of the case, and the views which must be supposed to have influenced the party to make it, and the true extent and meaning of his statement. On the part of Mr Syme, I disclaim any intention either to disparage the professional ability of Mr Lizars, or to put him in the category of being a disreputable character. I do not say that he is disreputable, and I have not said so. There were publications—there were proceedings—there were dealings between the parties; and having special and exclusive reference to these, and to the position in which he stood towards Mr Lizars in consequence, he addressed the letter in question to the Editor of the “Medical Gazette.” An explanation was necessary as to the cause of his silence in reference to the effect of his operation. The explanation was made. We have seen that the Editor was not pleased to insert the letter. Mr Syme felt and believed that that party was mistaken in assuming that there was anything libellous, and in order that he might stand clear of the charge of libel, he obtained the insertion of this letter at full length in the “Monthly Journal of Medical Science.” What is the libel? Mr Syme promulgates the application of a new mode of treatment of a very afflicting and painful disease, which he believes is a discovery of the very greatest importance. I say, gentlemen, in reference to this matter, there are certain publications and certain statements made on the part of Mr Lizars—he writes a book—he makes statements on the position of the question as arising out of it, and the publication in London, in reviewing this work, makes inferential deductions as to the truth of these statements as to the result of the operation, from the fact of Mr Syme not meeting the statements, as in ordinary circumstances he might be expected to do. And what does Mr Syme write?—“Sir,—I have only to-day happened to see your journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner. You say, ‘a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars;’” that statement is contained in that paper. It says, that those two parties, who were assumed to hold professorial chairs in the same city, and they are described as engaged in a paper war—an assumption necessarily implying this,—that those parties were proceeding to attack each other in that written controversy. “But you must, or at least ought to know, (says Mr Syme) that I have not addressed a single word upon the subject in question to the so-called ‘professor,’ regarding him as long placed beyond the pale of professional respect and courtesy.” Now, gentlemen, what is to be explained to the public? It is to be explained that Mr Syme was not a party to

the controversy with Mr Lizars—it is to be explained that he was silent in that controversy, and that the reason and cause of his silence are explained by the grounds he gives for it. He says, addressing parties who must be assumed by him to have been aware of the situation in which he and Mr Lizars stood, you know, or ought to know, that I have not addressed a single word to him, because that, in the position in which he stood with regard to me, I hold him as a party I was not to enter into controversy with,—that he stood in a situation in which he had not his confidence, or was to be regarded with professional respect and courtesy. I ask you to give to the whole statement the plain tenor and meaning to be deduced from it. I say I was not a party to any such controversy as you have imputed to me, because I had to do with a party who stood in such a position to me, that I could not enter into controversy with him on a fair professional footing of equality. The statement goes on—“In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Mullar, are well-founded.” And then there is a statement in which Mr Syme says—“In fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth.” Now, then, gentlemen, we have nothing here on the point of general professional character and ability. All the charge against us is, not that we stated the facts, or declared certain statements to be untrue; but that, in the course of this statement, we had brought a charge of disreputable character against Mr Lizars. Now, gentlemen, you cannot read it in that way in reference to the position in which the parties stood; and I think you will see, from a number of witnesses we shall adduce, in the neighbourhood, that this was not the fair reading it received. There is no holding forth to the world, gentlemen, that he is disreputable in any respect,—the simple statement is, that I have not entered into controversy with him, because there stood between us those circumstances that precluded him, Mr Lizars, from standing in the situation where there could exist that courtesy in which the controversy should be entered upon. Now, when the statement is published connected with the supposed fierce paper war, and the circumstances to which the attention of the Editor of the “London Medical Gazette” is called, and which, it is said, should have been known to him, being in fact notorious among medical men, are stated, you see that Mr Syme only intends to account for, and explain, his silence. He says, that he has regarded Mr Lizars for a considerable time, as placed beyond the pale of professional respect and courtesy, so as to warrant him not entering into that controversy. So, I submit, that when you come to read it with reference to the position of the gentlemen and the circumstances of the case, you will give the statement an interpretation not only consistent with the truth of the case, but with the statement that we have all along made, and that you will come to the conclusion that the case as embodied in the issue is not proved. The question is, whether these statements do contain matter embodying or bearing this charge imputed in that particular issue. If you hold the object and meaning of Mr Syme in the use of these expressions, was simply to excuse his not entering into the controversy, having regard to the position of the parties,—you are bound under the issue to return a verdict for the defender. But the pursuer has undertaken to make out that a great deal more was intended than I apprehend Mr Syme ever did intend, and they are bound to make out that odious issue to the full extent; and if they fail, as I think they have already failed, your verdict should be for the defender. Although there may be expressions which, in a particular view of the case may be injurious to the pursuer, the question is still, whether the statement embodied a charge of disreputable character; and whether it is true, in point of fact, that those allegations go to this, and that the defender has so injuriously represented the character of the pursuer, that he is neither respected, nor entitled to respect, in the medical profession. I humbly submit that, in the circumstances, your verdict should be for the defender.



*Defender's Evidence.*

*Professor Christison examined by Solicitor-General.*

Q. You are professor of *Materia Medica* in the University of Edinburgh?—A. Yes.

Q. And I believe you are also one of the conductors of the "*Monthly Journal of Medical Science*?"—A. I am.

Q. You know the pursuer, Mr Lizars, and also the defender, Mr Syme?—A. Yes.

Q. Have the kindness to look at this letter in the "*Monthly Journal of Medical Science*," in August 1851.—You are acquainted with that letter, and remember its appearing in the *Journal*?—A. Yes.

Q. It bears to be a letter of which a part only had been inserted in the "*Medical Gazette*," and this bears to give the full letter;—there are some passages in brackets that are the omitted passages. Now, have the goodness to read the first paragraph about "a fierce paper war." This is addressed to the editor of the "*Medical Gazette*;" and the passage follows: "but you must, or at least ought to know, that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy.]" Now, explain what you understand by the meaning of those terms in brackets.—A. I understand that Mr Syme regarded Mr Lizars as placed in such circumstances towards him, that Mr Syme could not meet him in consultation, or discuss controversially any scientific subject, in consequence of the way in which he had been treated by Mr Lizars on various occasions, and particularly in reference to the subject of that letter.

Q. The subject to which this letter refers was the discovery of Mr Syme as to the operation for stricture in the urethra?—A. Yes.

Q. Mr Syme had published his discovery in a book?—A. He had.

Q. And you knew that Mr Lizars had published a book on the subject also?—A. Yes.

Q. What did you consider the object of these words—"regarding him as long placed beyond the pale of professional respect and courtesy"?—Did you consider these words were intended as an explanation of anything?—A. Certainly.

Q. And of what were they explanatory?—A. Of the reason for Mr Syme not entering into controversy with Mr Lizars.

Q. Did you consider that the words in that paragraph were intended, in any way, to represent the pursuer as a person of disreputable character in the profession?—No, I did not think so. I did not consider this as meant to imply that the pursuer was placed in this situation with regard to the profession at large,—only in regard to Mr Syme.

Q. Mr Syme and Mr Lizars were both gentlemen well known in the profession?—A. Yes.

Q. And their mutual position towards each other was well known in the profession?—A. Perfectly, I should think.

Q. Was this discovery of Mr Syme considered to be a very important discovery?—A. I think it has generally been considered so by surgeons; but on that point I decline to give an opinion, not being a surgeon.

Q. In the sense in which you read that paragraph, did it surprise you that it should be assigned as a reason for Mr Syme not having a controversy with Mr Lizars?—A. No.

Lord Justice-General.—Not a novelty to you?—A. No.

Q. As far as regarded Mr Syme's views of his position with Mr Lizars, it must have been very well known to everybody?—A. Yes, I should think to any person acquainted with the history of the profession in this country for the last fifteen or twenty years.

Q. And down to the time of the present controversy?—A. Yes.

Q. You know enough of surgery to know that stricture is a dreadful disease?



—A. Yes, particularly in the bad form, which Mr Syme's operation was intended to cure.

Q. It was the most desperate form of the disease?—A. Yes.

Q. You think it must have been a great object of Mr Syme to have his success vindicated to the public, and to satisfy the public of the utility of the operation?

—A. Surely.

Q. Had you looked into the books of Mr Syme and Mr Lizars at the time?—

A. Yes.

Cross-examined by Mr Deas.

Q. If you regarded a man placed beyond the pale of professional respect and courtesy, would you be willing to meet him in consultation?—A. I see no reason why he should not be met, merely from the circumstances in which he had placed himself with one individual.

Q. That is not my question. You have answered one question, when I put another. My question was, If you regard a man as having placed himself beyond the pale of professional respect and courtesy, would you be willing to meet him in consultation?—A. Not if he had placed himself in that position towards me.

Q. I want to know whether you confine your answer to a man who has placed himself towards you, or extend your answer to a man placed in that position towards the profession at large?—A. I do not think it probable a man could be placed in that position towards the profession at large, by these circumstances.

Q. I am not talking of Mr Lizars. I want to know whether you would be willing to meet a man in consultation who had placed himself beyond the pale of professional respect and courtesy, as regards the profession at large?—A. That being well proved, I would not; but it is an important part of my answer, the difficulty of the proof. If I am to lay down rules for the profession, I must have the grounds on which the rules are to be laid down.

Lord Justice-General.—The question assumes it as proved to your satisfaction.

Q. You are to believe that he has been so placed; and secondly, you are to believe that he has been deservedly placed—would you be willing to meet him in those circumstances?—A. If the question had been put in that fashion at first, I would have had no difficulty in answering it. I could not.

Q. If the statement in that letter had not been limited to the position of Mr Lizars and Mr Syme, would you think it a very injurious thing to say that of any party?—A. So much so, that I would not have consented to its appearing in a Journal of which I was one of the conductors.

Q. Were you, in point of fact, consulted as to whether this letter should be inserted in the Journal?—A. Yes.

Q. By whom?—A. Why, I suppose, we consulted one another. I presume the Editor would call us together.

Q. But I don't want you to presume.—A. But I must presume. I cannot accurately remember the minute details of such occurrences. The Editor of the Journal is in attendance, and he may be able to give you that information. There was a consultation of the Conductors, as there was in every other matter of importance—on that as on every other thing inserted in the Journal.

Q. Just tell me what took place about this particular article?—A. I approved of the communication given to the Editor of this Journal.

Q. But the circumstances?—A. I cannot recollect them at this particular time.

Q. Were you asked to read it?—A. I certainly read it at the time. How could I give my opinion on it if I had not read it?

Q. We only want facts, not reasons here, Dr Christison. You read it before it was published?—A. I did.

Q. Was that at a meeting of the conductors?—A. Yes; but I may have read it before.

Q. Were they all present?—A. I do not recollect.

Q. A number of them?—A. Certainly. I think it was almost certain that all

the conductors would see it, unless any of them happened to be in the country at the time; but I cannot say they were all present.

Lord Justice-General.—You think it next to absolute certainty that all would see it?—*A.* Yes.

*Q.* Are they all in the habit of reading all the articles?—*A.* Not all.

*Q.* How is that managed?—*A.* A great deal must be left to the discretion of the Editor. Many of the articles must be published entirely on his discretion.

*Q.* Did you read this article before it was published with any particular view?—*A.* Not with any particular view then. I read all the articles put before me either incidentally or intentionally, on the part of the Editor.

*Q.* Was your attention particularly drawn to those words we have been speaking of, or just to the article generally?—*A.* Do you mean my attention drawn by any one?

*Q.* Yes.—*A.* My own attention was directed to these words.

*Q.* And you construed them as applying to the position of Mr Syme and Mr Lizars?—*A.* Yes; but would you allow me to say that my own conviction was that the reason applied to Mr Syme *quoad* Mr Lizars, not Mr Lizars *quoad* the profession at large? My reason is, that Mr Lizars was not in that predicament, and he, Mr Syme, knew was notoriously not in that predicament. Mr Syme knew that I met Mr Lizars myself; and therefore it was notorious that Mr Lizars could not be in that position.

*Q.* Now, as one of the conductors of the Journal, I presume you are responsible for what appears in it?—*A.* I presume I am.

Lord Justice-General to Mr Deas.—That is a question that he should rather put to you.

#### Re-examined by Solicitor-General.

*Q.* From the reason you mention, you consider that the only fair meaning that could be put on that passage?—*A.* Yes; there is no doubt that a person ignorant of the transactions, and who did not know the parties, might put a different construction; but a professional man who knew the circumstances, I think could not in fairness put a different construction on it from that which I understand it to bear.

#### Examined by Lord Justice-General.

*Q.* You say it refers to the position of the parties towards each other, and not to the profession generally—that is of course a very important distinction. I suppose it has occurred in Edinburgh that parties holding respect in their profession have been in that position?—*A.* Unhappily, it has been so too frequently, from the circumstances in which our profession meet.

*Q.* And that does not imply a general want of respect by the profession at large?—*A.* No.

The Solicitor-General here referred to the minute, showing that the pursuer did not intend to insist against any of the conductors of the Journal, save Mr Syme; and showing therefore that the other conductors had no interest in the action.

#### Professor Simpson, examined by the Solicitor-General.

*Q.* You are professor of Midwifery in the University of Edinburgh?—*A.* I am.

*Q.* You are also one of the conductors of the "Monthly Journal of Medical Science"?—*A.* I am.

*Q.* Look at the letter in the Number for August 1851—professing to be a complete copy of a letter by Mr Syme to the Editor of the "Medical Gazette"—was your attention drawn to that letter?—*A.* Yes, before it appeared.

*Q.* Do you see the passage in brackets,—“You must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called ‘professor,’ regarding him as long placed beyond the pale of professional respect and courtesy.” What meaning did you attach to that paragraph, parti-

cularly the last part of it?—*A.* Simply this:—That Mr Syme's moral and professional character had been very unjustifiably attacked in publications of Mr Lizars; and that he wished us, as conductors, to admit this explanation, why he would not enter into any controversy with that individual.

*Q.* Did you consider it imported that Mr Lizars was a person of disreputable character in the profession?—*A.* Not at all; merely the view Mr Syme took of Mr Lizars. None of us had any doubt on the matter; none of us so recognised it; we merely regarded it as the reason why Mr Syme could not vindicate his character,—that character having been attacked by Mr Lizars; and that was explained in the Journal two months afterwards.

*Q.* Did you know that this referred to a controversy about stricture of the urethra?—*A.* I did.

*Q.* As to which Mr Syme had published a book, and as to which Mr Lizars also had published a book?—*A.* Yes.

*Q.* From what you knew of the position of Messrs Syme and Lizars for a long time, and of that controversy, down to these later days, were you surprised that Mr Syme should decline to enter into a controversy with Mr Lizars?—*A.* Not in the least degree surprised. I would have been surprised if he had done so.

*Q.* Were you surprised at the reason he assigned?—*A.* I was not surprised. Had I been in Mr Syme's situation, I must have felt the strength of that reason.

*Q.* Did you understand that was their position towards each other?—*A.* More than that. The book was so bad towards Mr Syme's character, as a surgeon and a man, that it was impossible he could enter into the controversy.

Cross-examined by Mr Deas.

*Q.* If you had considered it as applicable to the position in which Mr Lizars stood with the profession in general, would you have consented to its being published in the Journal?—*A.* Certainly not.

*Q.* Do you find anything within the four corners of that letter about Mr Syme having been attacked by Mr Lizars, or do you take that from other sources?—*A.* I will read it and see. There is the paragraph which refers to the review of Mr Lizars' work; there was an expression which Mr Syme there uses, of his desire to keep his position as a gentleman, but which he was not in a position to do, as regarded an answer to Mr Lizars.

Lord Justice-General.—You forget the question, and are going upon what you knew before.—*A.* No. I see nothing of that kind in the letter. I beg pardon. I think there is. There is the last paragraph. Mr Syme there stated that there had been no bleeding or extravasation; and he stated that, because the very reverse had been untruly stated by Mr Lizars.

*Q.* But that is not within the four corners of the letter?—*A.* It is.

*Q.* Do you not know the distinction between extravasation and bleeding, and the statement in the letter, that Mr Lizars had attacked the character of Mr Syme?—*A.* It is only inferential.

*Q.* The words are, "regarding him as long placed beyond the pale of professional respect and courtesy,"—is there anything in these words to limit that to the relative position of Mr Syme and Mr Lizars?—*A.* Yes, inasmuch as there is an "I" before it.

*Q.* That shows it is Mr Syme?—*A.* Yes, and the relation Mr Lizars stood to Mr Syme. You may express it more clearly, but that is the way that Mr Syme expressed the effect to us.

*Q.* You say that you find it is limited to the relative position of these two parties because there is an "I" before it?—*A.* I don't take that as the only reason; that is only one reason.

*Q.* Suppose you were to say to me, I regard you as long placed beyond the pale of professional respect, how could I find out from these words that it only applied to you and me?—*A.* For this reason, that people would know you had been quarrelling with me for a long time.

*Q.* But when you said to me that I had long been so placed, how would that

enable me to show that it meant with you only, or with all the world beside?  
—A. You might torture it the other way.

Q. I regard you as long placed beyond the pale of professional respect and courtesy—what torture does it require to make that apply generally?—A. There may be two different ways:—you may do it in one way or the other. The torture is in the way you wish to do it.

Q. The torture is in applying it the general way?—A. Yes.

Q. Are not the words general?—A. Partly general.

Q. Are they not entirely general?—A. No; I do not think they are entirely. I think it might have been worded “beyond the pale of the respect and courtesy of the profession and the public.”

Q. You think, although the words are all general, it requires torture to give them a general meaning?—A. Take your own view of it, Mr Deas.

Q. No; but that is your view. But if you thought the words had only a general meaning, and that they were relatively applied to an individual, and he had so acted, would you think that a man to meet with in consultation?—A. No.

Q. You think that these general words would imply that he was not reputable in his profession?—A. Yes; that he had done something wrong.

Q. And therefore that he was a disreputable character?—A. Yes.

Re-examined by Solicitor-General.

Q. Do you think any man would read that paragraph as implying disreputable character generally?—A. I think, according to the general impression, probably not deduced so logically as Mr Deas takes it, it would not be so read; and I may add, that I think that was the impression.

Q. There is nothing reflecting on Mr Lizars' ability; would not courtesy imply that it referred to some personal intercourse?—A. Yes; courtesy and civility are synonymous terms.

Lord Justice-General.—You think the construction would be affected more or less by knowledge of the previous circumstances?—A. Yes.

Lord Justice-General.—And in Edinburgh there were enough circumstances known to give a construction?—A. Yes.

*Dr William Scott*, examined by Solicitor-General.

Q. You are both a physician and surgeon?—A. Yes.

Q. And surgeon to the Infirmary of Dumfries?—A. Yes.

Q. You have been there how long?—A. For the last twelve years.

Q. Do you know the pursuer, Mr Lizars?—A. By his writings only.

Q. And the defender, Mr Syme?—A. Personally.

Q. You saw a letter in the “Monthly Journal of Medical Science” for August, from Mr Syme?—A. Yes.

Q. Did you see it at the time?—A. Yes.

Q. Did you take in the “Journal”?—A. Regularly.

Q. You noticed that letter by Mr Syme, as referring to Mr Lizars?—A. I did.

Q. You see this first paragraph,—“I have only to-day happened to see your journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner. You say, ‘a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,’ but you must, or as least ought to, know.” This was addressed to the “Medical Gazette”?—A. Yes.

Q. Did you know what that referred to?—A. Yes; the operation of Mr Syme.

Q. Have you performed it?—A. No; but I have had patients who have been operated upon.

Q. And you consider it a valuable discovery?—A. I do.

Q. It is more particularly applicable to very bad cases?—A. Yes; severe cases.

Q. Read the next part,—“That I have not addressed a single word upon the subject in question to the so-called ‘professor’ [regarding him as long placed beyond the pale of professional respect and courtesy.]” What did you understand that to mean?—A. I understood it to refer to Mr Syme’s opinion of Mr Lizars.

Q. Did you know that there had been disputes and discussions between them of long standing?—A. Yes.

Q. Was that generally known?—A. Among the profession, I should say it was.

Q. From what you know of that position, was it any surprise to you that Mr Syme should assign the want of respect and courtesy as a reason for not entering into controversy with Mr Lizars?—A. No, sir.

Q. Did you consider the letter as in any passage reflecting on the general professional character of Mr Lizars?—A. Not at all.

Q. As inferring that he was a person of disreputable character?—A. Not by any means.

Q. I understand this as the impression you formed of the meaning and import of the letter as you first read it in your own place?—A. Yes; quite so.

Q. It was a very bad case in which that operation was had recourse to?—A. It was.

*Dr Johnston, examined by Mr Patton.*

Q. You are a physician and surgeon at Berwick?—A. Yes.

Q. And have been so for a considerable period?—A. For thirty-two years.

Q. Had you occasion shortly after the publication to read a letter inserted in the “Monthly Journal of Medical Science”—the letter of Mr Syme touching the conduct of the “Medical Gazette”?—A. I had, shortly after publication.

Q. Did you observe the expression in the beginning of the letter,—“You say, ‘a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,’ but you must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called ‘professor’ [regarding him as long placed beyond the pale of professional respect and courtesy.]” What meaning did you attach to the expression at the time?—A. I attached this meaning,—that Mr Lizars, by the manner in which he had conducted himself towards Mr Syme, had placed himself beyond the courtesy which Mr Syme owed to the other members of the profession.

Q. Did you understand it specially and exclusively to refer to the position between Mr Syme and Mr Lizars?—A. Clearly so.

Q. Were you surprised, Dr Johnston, at the expression of Mr Syme towards Mr Lizars?—A. I was not.

Q. How did you come to that conclusion?—A. Because it was known that Mr Lizars’ conduct previously to Mr Syme had been discreditable to himself.

Q. You did not consider it as inferring an imputation against the character of Mr Lizars generally, or his abilities professionally?—A. I considered it merely as referring to him in his position to Mr Syme. It never entered into my mind to consider it otherwise.

Lord Justice-General.—And, putting that construction on it, you were not surprised at the thing? it was no novelty to you? it was consistent with what you knew of their position?—A. It was, my Lord.

*Dr James Combe, examined by Solicitor-General.*

Q. I believe you are President of the College of Surgeons?—A. I am.

Q. You know Mr Lizars, and you know Mr Syme?—A. Yes, I do.

Q. Have the kindness to look at this letter of Mr Syme’s, in the “Monthly Journal.” Did you notice that letter at the time, or shortly after it was published?—A. Yes.

Q. Look at the second paragraph, about a fierce paper war. Now, in reference to that part, that Mr Syme had not addressed a single word to the “so-

called professor," what do you consider the last words to import?—*A.* I consider it refers to a knowledge of the previous terms of the parties.

*Q.* Were these terms of the parties such as precluded any feeling of respect or courtesy from Mr Syme towards Mr Lizars?—*A.* I think so.

*Q.* You knew of questions between them of long standing, and you were also acquainted with this controversy?—*A.* I was.

*Q.* In which Mr Lizars and Mr Syme had fallen out on the surgical question?—*A.* Yes.

*Q.* Mr Syme first recommended to the public a new mode of operation in bad cases of stricture;—did you consider that an important discovery?—*A.* In a limited number of bad cases important.

*Q.* And as to which Mr Syme naturally felt great anxiety?—*A.* Yes.

*Q.* To explain his position?—*A.* Yes.

*Q.* Did you consider this was at all reflecting on the general professional character of Mr Lizars?—*A.* No. I considered it in reference to the personal standing of the parties. Mr Syme was entitled to explain how his position was such that there could be no controversy.

*Cross-examined by Mr Deas.*

*Q.* You read this letter simply in reference to the state of matters between Mr Syme and Mr Lizars?—*A.* I could not help knowing how matters stood; and I read the statements by that light.

*Q.* The words are, "regarding him as long placed beyond the pale of professional respect and courtesy;"—do you find anything in the words to limit them to the position between Mr Syme and Mr Lizars?—*A.* It was impossible for me to separate the one consideration from the other.

*Re-examined by Solicitor-General.*

*Q.* These circumstances were very generally known in the medical profession?—*A.* Yes.

*Dr Carpenter, examined by Mr Patton.*

*Q.* You are professor of Medical Jurisprudence in the University College, London?—*A.* Yes.

*Q.* And also examiner of Physiology in the University of London?—*A.* Yes.

*Q.* You have had occasion to study the question in reference to the important mode of operating in cases of stricture, as discovered by Professor Syme?—*A.* Yes, I have had occasion to look into it.

*Q.* Have you considered it an important discovery by Mr Syme, in reference to the cases in which it is applicable?—*A.* I would so consider it; but not being in the practice of surgery, I would not be justified in giving a decided opinion.

*Q.* You have seen patients operated upon?—*A.* I have seen patients on which the operation appeared to be extremely satisfactory in its results.

*Q.* Do you know that the discovery was communicated to the professional public by Mr Syme in a treatise?—*A.* Yes, I have read that treatise.

*Q.* You have occasion to know that a book was published by Mr Lizars some time afterwards?—*A.* Yes; and I have also read that book.

*Q.* You have read a letter in the "Monthly Journal of Medical Science," published by Professor Syme, on the subject?—*A.* I have.

*Q.* You read it at the time?—*A.* I did at the time.

*Q.* That is the letter?—*A.* Yes.

*Q.* Do you see the expressions,—“You say, ‘a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars;’ but you must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called ‘professor’ [regarding him as long placed beyond the pale of professional respect and courtesy.]” In what way did you interpret the meaning of these expressions, as applied to Mr Lizars?—*A.* Being acquainted with the previous proceedings that had taken place between Mr Lizars and Mr



Syme, but, particularly, having in view the tone and character of Mr Lizars' work on "Stricture," I thought Mr Syme intended to say that the conduct of Mr Lizars to him had been such as would prevent him taking notice of any attack on any of his works on stricture, regarding him as having excluded himself from that professional respect, courtesy, and attention, which any professional man would exercise towards another, whether a stranger to him or otherwise.

Q. Then you read and understood it as applicable to the circumstances in which Mr Syme and Mr Lizars stood in reference to this controversy, and not applicable to the general position of Mr Lizars?—A. Yes, that it refers to Mr Syme's own position to Mr Lizars.

Q. You did not read it with reference to the general position of Mr Lizars with regard to other medical men?—A. Certainly not.

Cross-examined by Mr Deas.

Q. Did I misunderstand you, when you referred to the attention that was due, whether at the hands of a stranger or otherwise?—A. I understood it prevented Mr Syme from extending towards Mr Lizars that respect which he would towards another. Suppose I or any other individual had thought that Mr Syme's operation had not that value which he himself attached to it,—suppose I were to criticise it, I would expect that Mr Syme would extend towards me the professional courtesy of taking notice of statements made on the other side. But I understood Mr Syme to mean that Mr Lizars had excluded himself from the privilege—so to speak, had put himself out of court.

Q. But do you see anything there to explain that?—A. It must be taken with the previous portion of the sentence. I understand in my apprehension that it at once carries back the mind to previous occurrences.

Q. But is there anything there to limit it to the state of matters between Mr Syme and Mr Lizars? Does it not say, "Regarding him as long placed beyond the pale of professional respect and courtesy"? Is not that quite general?—A. He does not say "*of the profession*;" but the interpretation I put upon it simply had reference to himself, connected with antecedent occurrences which I knew very well.

Q. You knew that there was a misunderstanding between them?—A. I knew that, and had Mr Lizars' own statement of this misunderstanding in his own book.

Q. Was there cause of offence on both sides in your judgment?—A. Mr Syme gave no cause of offence in this case.

The Solicitor-General.—What is this? Are we to go into this? I will soon show you cause of offence, if you will let me go into it.

Lord Justice-General.—I understand you to read it in this way—As long placed beyond the pale of professional respect and courtesy for me. That you think the fair meaning?—A. That is the sense in which I read it. It might not appear to be the meaning to any one who did not know the previous circumstances.

The Solicitor-General.—Do you think any one who had read Mr Lizars' own book could hesitate to put that construction on it?—A. I think no one would hesitate who had read that book, and there saw the position of the parties.

*Dr Robertson*, examined by Solicitor-General.

Q. I think you are Editor of the "Monthly Journal"?—A. Yes.

Q. Do you see that letter of Mr Syme to the Editor of the "Monthly Journal"?—A. Yes.

Q. You knew about the controversy, and had read the publications of Mr Syme and of Mr Lizars?—A. Mr Syme's work I had not read at that time; Mr Lizars' I had read.

Q. Had your attention been directed to what the Editor of the "Medical Gazette" had said about it?—A. It had.

Q. And did you direct your attention and that of some of your conductors to what was said as to those passages?—A. I did.

Q. What did you understand to be the meaning of the second paragraph, including the words in brackets?—A. I regarded it as Mr Syme's reason for silence—for not answering Mr Lizars' attack,—the reason of Mr Syme's silence, which he was bound to express.

Q. As the Editor of the Monthly Journal, would you expect an explanation from one of the parties in such a controversy?—A. Yes. Either that he would answer the statement, or express a reason for his silence.

Q. What did you understand to be the meaning he assigned?—A. That Mr Syme, from his previous experience of Mr Lizars, considered he had so grossly injured him, that he could not hold any professional communication with him.

Q. Or engage in controversy?—A. Yes.

Q. Did you understand him to reflect on the general professional character or conduct of Mr Lizars?—A. No.

Lord Justice-General.—But did you understand the words as bearing any such interpretation?—A. No; but specially applicable to Mr Syme's individual opinion of Mr Lizars.

Q. If you had supposed that these words were meant to impute a disreputable character to Mr Lizars, would you have allowed them to be inserted?—A. I would not have sanctioned their insertion.

Cross-examined by Mr Deas.

Q. You said you considered this statement as Mr Syme's individual opinion of Mr Lizars?—A. Yes, as founded on his previous misconduct towards Mr Syme.

Q. That is what you mean when you say you take the words in a special, not a general, sense.—A. Yes.

Q. You say you were aware of previous quarrels between Mr Syme and Mr Lizars?—A. Yes, I had some general notion of those quarrels.

Lord Justice-General.—He said he had read the pursuer's book, but not at that time the defender's book.

The Solicitor-General.—Yes, my Lord.

The defender put in the interlocutor of May 28, 1852, assailing all the Conductors of the Monthly Journal, except Mr Syme, from this action, and closed his evidence.

Mr DEAS.—Gentlemen of the Jury, I must now trouble you with a few observations on the part of the pursuer, Mr Lizars, who complains of this libel which you have heard read, more particularly that part of it in which Mr Syme states that he regards Mr Lizars as long placed beyond the pale of professional respect and courtesy—that is to say, professional respect and professional courtesy. It is said that this was merely an expression of Mr Syme's individual opinion. In so far as it is said that it was merely an expression of Mr Syme's individual opinion, I don't think the remark can carry with it much weight, or at all affect the substance of this case, because, assuming it that he had given his individual opinion, still it is the opinion he publishes to the world, an opinion therefore, as one of the witnesses observed (Dr Sibbald), given as that of himself and of all those whom his high authority could influence. Looking at it in this point of view, it is an opinion expressed in regard to a member of the medical profession by another member of the medical profession, a man of great ability, of great success, and acknowledged throughout this country as one of the most eminent members of that same profession. So his opinion necessarily carries with it the highest possible authority, and falls on any other members of that profession with the most crushing effect. That which Mr Syme expressed in public to the world, as his own opinion, may fairly, I think, be assumed as the opinion entertained by others, so far as they had the same grounds of judging. When he gives it forth, therefore, as his own opinion, you see in a moment it is just the same thing as if he was asserting it as a matter of fact, given with all the authority he can command. He is just

stating in plain English, That is my opinion, and I think it ought also to be yours. It is no answer whatever, it is no mitigation of the offence, that a man may assert that you are a person of disreputable or dishonest character, in so far as he simply is happening to give his own opinion. In so far as this is said by the other side to be Mr Syme's opinion alone, I say that the defence comes to be no alleviation of the injury done. But the next thing that is said is, that the words were only intended to represent the state of matters and state of feeling as between Mr Lizars on the one hand, and Mr Syme on the other. Let us take it in the first place that it were so, and let us see whether it would be a defence against the libel if it were. Suppose the words had been added—"regarding him as beyond the pale of professional respect and courtesy, *as regards me*,"—the question would at once arise—Had Mr Syme a right to say, and publish to the world, any such thing? An action, I take it, would have lain, although the words had been the words I have now stated; and it might have been an answer to that action if Mr Syme had said—Very true, I have so stated my opinion, but I am ready to justify it—I will prove to the satisfaction of the jury that Mr Lizars has so conducted himself as to place himself beyond the pale of professional respect and courtesy, so far as I am concerned. He might have said that, and he might have taken an issue to prove it; and if he had proved it, it might have been a defence. But with all the ingenuity of my friend the Solicitor-General, can he make out, that if I say a professional brother is put beyond the pale of professional respect and courtesy—that I justify myself for having so said by maintaining that, through his fault or through his misconduct, he has put himself beyond the pale of professional respect and courtesy, so far as regards me? Unless I prove that, I desire to know on what ground of law or common sense or justice that man is entitled to place me in any such position. If I were to say and circulate throughout the world that my learned friend the Solicitor-General had so conducted himself as to place himself beyond the pale of professional respect and courtesy, so far as I was concerned—would that not be a libel, and a libel of a gross character, unless I stated that I had grounds for proving that to the satisfaction of the jury? Therefore this miserable defence, which is not to be found within the four corners of that record, would not avail my learned friend, even if he could maintain it.

Lord Justice-General.—He would not have been entitled to an issue, unless he had undertaken to justify the whole proceeding.

Mr Deas.—I dissent from that. He might have said on the record that was the meaning that he attached, and that he would justify that meaning. I think, whether it was taken as justifying the opinion according to my sense, of the libel, or whether it was taken as justifying it only in the limited sense, that he might have justified in part, or he might have justified in whole. You have your answer in the first instance; and most assuredly, if they had chosen to make a special case on the record, they could not have been excluded in some shape or other in going to a proof on that matter, whatever that mode might have been. Now then, there is no such case here; and if I were here speaking to this as a matter on which your Lordship is to direct the jury, I would be entitled to refer to the record in a different way. I refer to page 23 of the closed record, and reading it in connection with the whole preceding articles, I do not find this limited meaning attached. I do not find it stated that that limited meaning would be justifiable. But be that as it may, I say that no man is entitled to assert that I have so conducted myself as to place myself beyond the pale of professional respect and courtesy, even as regards that individual. He is not entitled to say any such thing, otherwise he is responsible for the communication of that statement, which is presumed to be false. Therefore, he could not get out of the case by any means, although he were to satisfy you that he was merely telling his own opinion to the world. But it is really of little moment to trouble ourselves as to what may be the result, when you have a libel before you that could bear no such construction. It is the law, and

it is common sense, I think his Lordship will tell you, that no man is entitled to use words, still less to print and publish a statement to the world, containing words which may be read in a sense injurious to an individual. No man is entitled to justify himself in doing that, by saying that he meant the words to be understood in some other and more limited sense. The supposition is absurd. If a man deliberately concocts a libel, and he prints and publishes it to be read by all the world, what signifies it to the man whose character is injured and destroyed by the paragraph, that he, the writer, meant the thing in a different and more limited sense, and that a great portion of the world have in reality attached that limited sense to it? I can quite understand that a statement of that kind might go some length, although a very small length, in alleviating the injuries. It may be with that view that a person may use the words, but I cannot understand how any such statement could be possibly maintained here. It would be like adding insult to injury, to say that Mr Syme is presumed to know the meaning attached to the words, and to know the meaning the world would put on them, when he writes and publishes this, that he has long regarded Mr Lizars as beyond the pale of professional respect and courtesy. What right has he to suppose or require that people who read those words shall understand them in some different or more limited meaning, when all that he said he meant to say, he might have said easily, if he had that intention? He might have said that he had long regarded Mr Lizars as placed beyond the pale of professional respect and courtesy, so far as he himself was concerned. There is no such limitation in the words. Just look at them, and consider what would have occurred, and did occur, on reading that paragraph, and without knowing anything about the quarrels or misunderstandings that had taken place. Could you limit the meaning, or would you not read them in the plain and actual sense, that Mr Lizars had so conducted himself as to be long placed beyond the pale of professional respect? Can anything be more distinct than these words without any limitation? They do not bear the sense which it is now said was intended. It might have been broadly said in the clearest manner, that he had long placed himself in that position—that he was not entitled to expect from him (Mr Syme) that professional respect. Was there any difficulty in saying that that was what he meant—that he (Mr Lizars) had so acted that he could not expect Mr Syme to treat him with the courtesy and respect which he (Mr Syme) would extend to another individual?

Now, gentlemen, his Lordship will tell you that even if Mr Syme had written and published those words under the misapprehension that this was their meaning—that that would amount to no defence against libel. A man must be cautious and careful as well as honest in his proceedings, and it will not do to injure the character of other men by publishing statements of which the writer may have misunderstood the import. No man is entitled to publish words as to another which may be misunderstood; and therefore that is no defence at all on the part of Mr Syme. But I am sorry to say that, poor and paltry as that defence is, there is no room for it. If ever a man came into Court and was not entitled to place that limited meaning on his words, it was Mr Syme. But what takes place? These Edinburgh gentlemen tell you that they knew the state of feeling between those two doctors, and that, knowing that state of feeling, they thought that Mr Syme merely meant to express the mode in which he would think himself called upon to treat Mr Lizars. But you will be pleased to remember that the thing which was done by Mr Syme was not to express this opinion in Edinburgh society, or even to print this in an Edinburgh paper. The thing he proposed to do was to send it to that London Medical journal—a periodical published, and proved to have a large circulation, in Great Britain and Ireland and over the civilised world. He sends it out of this place—he sends it to be circulated among thousands and thousands of individuals, many of whom, with all their love of controversy and all their knowledge of medical science, would read it, and who had never heard of either the name of Mr Syme or of Mr Lizars before. He shows that he is not using it for a limited purpose or a limited circulation, but putting it into the hands of everybody, professional or non-professional—for

non-professional persons do sometimes read those books, and any individual might take it up, who might naturally know nothing whatever of the circumstances. Therefore I think it a very weak defence, and a very weak observation at the outset, that this was his intention in publishing this libel. The conclusive answer to such a plea is this, that the intelligent Editor of that journal, Dr Taylor, tells you that when he read that paragraph, he refused to publish the words in brackets, because they would bring him under the law of libel. In the month of July 1851, Dr Taylor, after striking out those objectionable passages, published Mr Syme's letter in his journal, and adds the note which you have proved to you, to the effect that he left out certain passages because he considered them to come within the English law of libel. Mr Syme reads it—he sees that that paragraph was objected to by an editor of great knowledge and experience, who found it to be libellous. And is he entitled after that to come into a court of law and say that all that he meant was only to express his individual opinion of Mr Lizars, and that it was not libellous at all in the sense in which the pursuer takes it? Now was not this rejection of the article by Dr Taylor enough, at all events, to make Mr Syme pause? If he was a man who did not wish to injure his neighbour if he could help it, who did not wish to do anything destructive to his character—was it not a thing to make him carefully read over that paragraph which Dr Taylor refused to insert? And a single sentence would have sufficed to have made it known that he did not mean that Mr Lizars was placed beyond the pale of professional respect and courtesy; but simply that he had so conducted himself towards Mr Syme, that he (Mr Syme) was not called upon to answer him. What would have been easier than to have added that explanation, after the caution he received and the very ground on which it was rejected? In place of that, what does he do? Could anything be more reckless? I do not think anything could be more wicked and malicious than that, after the interval of more than a month, in August 1851, when no other journal would take it in, he should publish it in his own Journal, in which he has fully explained his views, and tells you plainly why he did it, and that it was refused by the "London Medical Gazette," because that matter excluded might have been subject to the English law of libel. These are his own words—"One of the conductors of this Journal lately felt it necessary to address a letter of remonstrance to the Editor of the 'London Medical Gazette,' who published it in an imperfect form, under the pretext that the matter excluded would have been subject to the English law of libel." Gentlemen, what kind of statement is that? There is no qualification about it, it does not call in question the good taste of Dr Taylor in rejecting it. He rejects it because he believes it falls under the English law of libel; and Mr Syme chooses to be so adventurous as to take the risk and lay it before the public, although rejected on the law of libel. He says, I will publish it in my own book—libel or no libel, it shall go forth to the world—not one syllable of modification—not one word of explanation. He says that Dr Taylor mistook his meaning, and that he had no motive for libel. He does not choose to say in a single sentence that his sole motive was to explain his reason for not entering into controversy. If it was so, where was the call for saying Mr Lizars was placed beyond the pale of professional respect and courtesy, if the thing was simply to explain that after what has taken place between Mr Lizars and me, I do not feel called on to have any controversy? Would the English language not have enabled him to have said so, and easily have given that explanation? There is, therefore, not the pretext or shadow of excuse for not doing it. I do not believe it was his meaning. I do not believe that Mr Syme was so dull of comprehension as to fall into such a mistake—I do not believe that after the letter being rejected by Dr Taylor, and he resorting to the mode of publication in his own book—I say, I do not imagine that he could suppose that the words were used in the sense which they could not at all bear, or at least which the words could not be said to express. It appears to me that the only natural explanation was, that his prejudice was so strong that it blinded his better judgment. In the state of prejudice and excitement he was in, he could form no deliberate and intelligible belief that the words would be so understood in that



sense. But if he did persuade himself into that belief—persuading himself into a belief in the most reckless manner that man could do—then he is equally responsible as if he had acted from no error of intention. Now, no doubt the conductors of this Journal, who are men of the highest eminence and respectability, are led to suppose that, from previous circumstances relating to the two parties, the words bear a light which is not to be found within the four corners of the letter, and that the letter must therefore be understood in a more limited sense. I do not in the least question, that on such a perusal as any of those gentlemen may have taken in a general way—for, as Dr Christison said, they had all read the letter with a view to the relations of the parties—that they may have been led to suppose that such was the meaning, as it struck them. We cannot exactly tell what was the degree of attention which those gentlemen really gave to this matter. I think it impossible to say that there was not some degree of carelessness. When they saw that the letter had been rejected by Dr Taylor, I think they ought to have been more careful. And I think it is somewhat unusual in a court of law, that an array of witnesses, who are all art and part in this injury, should be brought to put their interpretation of the libel which they allowed to go into their Journal, when they fairly admit, that if they had viewed it in the light of a more general statement they would not have permitted it to appear. I daresay they fell into a mistake; I daresay they regret it, and that they are willing to persuade themselves that this was the only meaning. But it does not follow in the least that that was the meaning in which the words were originally used, or the meaning which the words were naturally calculated to convey. Now, you could not fail to observe that there was not a single witness brought forward on the side of the defender to prove that they interpreted the words in that way, except witnesses who knew of the previous quarrels between the parties. And they tell you that they read the words in that light alone—not a man among them alleged that without that knowledge they would have put the same meaning on the words. Not one of them attempted to say that he would put the same meaning on the words, had he been entirely ignorant of the previous relations of those two parties. Now, that goes no way at all in regard to the construction of a document published to be read by all the world. Well, gentlemen, while, therefore, the evidence of those witnesses, three or four of themselves, for it is just that—those who might be in law responsible for this blunder—I say, while their testimony, and the testimony of the other witnesses for the defender, that this was the state of feeling in which they read the letter—is weakened by that consideration on the one hand—their evidence is still of the greatest possible importance, on the other hand, in this way—for remember this, that every one of them tell you—Dr Christison, Dr Simpson, and every one of them—that if it had occurred to them that the words might have been understood in a broader sense, they would never have admitted the same into their Journal, because they would have been most unjustifiable and most injurious to Mr Lizars. But if the words could be read in the sense I am now putting on them, Dr Christison, and all of them, tell you that, read in that sense, they would have amounted to a most injurious and crushing libel. And how does Dr Christison not read it in that sense? The reason was this, not what he knew about the previous quarrels, but he said that Mr Lizars was not in that predicament in the profession—that Mr Syme must have notoriously known that Mr Lizars was not in that predicament—that Mr Syme must have known that Mr Lizars had a very high position in the profession—therefore, that it was impossible that Mr Syme, a man of high standing—it was impossible for him to imagine that Mr Syme could mean to say that Mr Lizars was placed beyond the pale of professional respect and courtesy as regarded his professional brethren at large. No doubt, however, says Dr Christison, in his re-examination, a person ignorant of the transactions between Mr Syme and Mr Lizars might put a different construction upon it, although not those who knew the parties. Why, there is no libel whatever, or slander however gross, that may apply to a man of a totally different character, that might not be thus put forth. No slander would injure



any man in that sense. But it is impossible to say, and Dr Christison and Dr Simpson did not, that those who did not know the parties, were to form this opinion. Their testimony, and that of Dr Carpenter, a witness for the defender, is most irresistible and strong in regard to the atrocious nature of the libel, if the words are capable of being read on the construction I put on them. But although they put that meaning on those words, you can judge of those words as well as they can do. I think you would observe, that although men of the greatest ability and eminence took this view, they still found very great difficulty to give you any reason. Dr Simpson admitted that the words were perfectly general,—he could give no intelligible reason why the words should be applied in a more limited way. If I say of any man, that he has been long placed beyond the pale of professional respect and courtesy, I just say, that the world ought to believe this as well as me. Now, the statement here is this—that Mr Syme has long regarded Mr Lizars in this light, and not one syllable is added to make it anything else than this, that, as a professional man, he has been long placed beyond the pale of professional respect and courtesy. There is not one syllable to add anything else to that. If there is nothing here to indicate anything else than that, and if Mr Syme publishes that to the world, and in a deliberate way, weeks after he had been warned that it was libellous, I submit to you it is totally absurd to ask you to look at the statement in that limited way. Is that the light in which it appeared to any of the intelligent witnesses who were examined for the pursuer,—is that the light in which it appeared to Dr Taylor, a man accustomed to conduct a review, and, therefore, constantly obliged to read articles with a view to ascertain whether they tended to be injurious to a party or not—a man who did, and must have known, the previous controversy between those parties, for it was impossible he could be ignorant of the same. There is Dr Taylor with all that experience and intelligence, and he takes the words in their natural sense;—he thinks when a man says of another, that he has long been placed beyond the pale of professional respect and courtesy, that he really means so; he saw no limit,—he saw the general statement that he (Mr Lizars) had so conducted himself as to place himself beyond the pale of professional respect and courtesy. He understood the words in that sense, and consequently he rejected them as injurious and libellous. Mr Kesteven, another man of equal intelligence, a practitioner in London, and accustomed to write for that highly respectable review,—why, the words struck him in the same sense, although he had access to know of that controversy. Then there is Dr Renton, one of the most respectable medical practitioners in this neighbourhood, a man of great intelligence, and who had the light not only of all the controversies, but the knowledge of the individual parties,—who read the article, and never doubted that the meaning was, that Mr Lizars had generally conducted himself so as to place himself in that position. Then you have Dr Sanderson, another man of intelligence and experience; and you have Dr Sibbald of Edinburgh, who tells you the same thing,—men who knew the parties, and were in the habit of consulting Mr Lizars, and recommending patients to him. And knowing all that, they never doubted that the meaning of Mr Syme was, so far as his high authority could influence the profession, that Mr Lizars had placed himself beyond the pale of professional respect and courtesy. Then, you have Professor Miller—he had just as good cause to know of the controversies between the parties and the position in which they stood to each other, and it never occurred to him to form any other opinion. And it never occurred to Mr Highley, the intelligent and respectable medical publisher in London. Every one of those intelligent witnesses had this privilege, and all formed their opinion, that that was the meaning, and that nothing else was the meaning. Has any man a right, especially after such a warning, to publish to the world what half a dozen of men, or some few people, may attach a particular sense to? Is he entitled to come forward and to tell me on that ground that I am not entitled to damages? What right

has he to subject me to that risk, by publishing words which may be liable to his construction,—still less words which can be held to mean nothing else according to their plain import? Now, you will observe, that the issue you have got to try is—“Whether the whole or any part of the said article or statement is of and concerning the pursuer, and falsely, calumniously, and injuriously represents and holds out the pursuer as a person of disreputable character in his profession, and as neither respected nor entitled to respect in the medical profession,—to the loss and damage of the pursuer?” I think every one of those witnesses whom I have named to you for the pursuer, tell you that they read this in the sense that is implied, a disreputable character in the profession, and that Mr Lizars was not respected in the profession; and I think it is quite impossible that you or any one else can read these words and attach a different meaning to the same. If there is any one thing more severe than another to a professional brother, more insupportable and hurtful to his feelings, it is the allegation that he so conducted himself as to place himself beyond the pale of professional respect and courtesy. The legal and medical professions are both of a learned description, and in which there ought to exist a high sense of honour; but I put it to you that I do not know anything under which it is so difficult to live, as a statement to the effect that any one connected with either of these professions had placed himself beyond the pale of professional respect. Next to that, I know no stigma more difficult to submit to, than even the more limited statement made by a man at the head of a profession in reference to a brother member,—even to say that he had so conducted himself towards him, as to place himself beyond the pale of professional respect and courtesy. Therefore, gentlemen, in considering this issue, you will be told by his Lordship, that where no issue is taken in justification, it is not incumbent on the pursuer to prove the falsehood. The law presumes the falsehood. The pursuer is not called on to prove that he is entitled to that respect. All that is taken for granted by the law itself, unless the contrary is held to be proved. Therefore, it is not necessary for Mr Lizars to bring any such testimony to his character—the testimony he looks to is your verdict. It is not incumbent on him to bring witnesses to prove that he has a high character and standing and respect in his profession. But, if there had been any necessity for that, it was amply supplied by the witnesses of the defender, out of whose mouths it has been proved that Mr Lizars is notoriously a man of eminence and ability and respect in his profession; so that Dr Christison could not even see the possibility of Mr Lizars being placed beyond the pale of professional respect. Therefore, you have the highest possible testimony to the character of Mr Lizars; and although the great object is not pecuniary reparation, it is impossible to lay out of view, that it is the amount of pecuniary damage which imparts your sense of the injury he has received. It is taken as part of your evidence. This is an action by one man of high standing against another; and the pecuniary damage is the mark of your sense of the injustice and of the injury that has been done to the character of Mr Lizars, and of the compensation, therefore, which it is necessary to make to him. It is not the least necessary to suppose that Mr Syme did this with the intent to injure. If I have said anything that seems to imply that, I did not wish that to be understood; but I do say that he acted in a rash and reckless manner, for which he must be equally responsible, as if he had intended to injure to the utmost degree.

The SOLICITOR-GENERAL.—Gentlemen of the Jury, at the conclusion of the evidence it is now my duty to make some observations which may seem necessary on the part of the defender. I shall endeavour to occupy your time as shortly as possible, and to bring the case to as narrow a compass as I can. Gentlemen, the form of our proceedings, for the sake of rightly understanding what parties are about when they send a case to be tried by a jury, brings it to a particular question that you are to consider. You are not to consider anything that might have been in their case or in their issue, but you are to consider

the question as to the manner in which the pursuer stood towards the defender, and you have to decide whether that did not warrant the defender in acting as he did. Now, have the kindness to read the issue on which your answer is now asked. After setting forth the article before it, it bears—"Whether the whole or any part of the said article or statement is of and concerning the pursuer, and falsely, calumniously, and injuriously represents and holds out the pursuer"—As what? "as a person of disreputable character in his profession, and as neither respected nor entitled to respect in the medical profession." That is the question for you, gentlemen—whether that article falsely, calumniously, as well as injuriously, sets forth that the pursuer is not entitled to respect or to courtesy? And when we come to the justification, his Lordship will tell you that there could be no justification in the sense that the defender attaches to the words. When he wants to justify, or take an issue in justification, he must say that the charge is true. Now, I have no intention to say that the pursuer is a person of disreputable character, and not entitled to respect and courtesy in his profession. That is not my meaning. Therefore there could be no issue in justification, as his Lordship will tell you, unless I had offered to prove what I expressly repudiate, because I do not assert it. That is not the question. Now, you will observe that the pursuer endeavours to extract this meaning out of a passage in the letter which is quoted; and he does so, and I beg you to observe more particularly, by putting in words into the issue which is not in the letter published. He must affirm that that was calumniously said of him which was not said. It is not the words of the issue that you are to consider—whether any calumnious meaning is to be attached to the words of the letter is a different question—but no words are contained in that letter equal either in character or strength to the words that are brought into this issue. The issue bears—"Because he is of disreputable character in his profession"—that is the leading part of that issue, and you must say whether he was accused of that or not; and if you do not believe he was, then you cannot bring in a verdict for the pursuer. While every man is entitled to reparation for injury done to his character, he is not entitled to reparation for injury which is not done. There are two ways of considering this—one, that he is not proved to have suffered injury. That is one of the ways; and second, that if not true that he was called disreputable in his profession, he cannot get damages for an injury that he did not suffer. I will come back on this again when I come to speak more particularly as to the means used by the pursuer's advisers in torturing out of those words a sense that they do not bear, and insisting that the pursuer was violently abused when there is no such abuse. The circumstances of the case, as they are presented to you, and on which the defender relies, are these—that Mr Syme, an eminent surgeon, had announced what he considered, and others may consider, a most important discovery referring to a disease which, in its worst form, is a source of misery to those who suffer under it, and as to which it has been the anxious wish of medical men to discover some remedy. Mr Syme announced a most important discovery, and in so doing, I trust gave no offence to Mr Lizars—none is alleged to have been given—and one of the witnesses, Dr Carpenter, told you that in this controversy, at least, no cause of offence had been given by Mr Syme to Mr Lizars. Well, then, Mr Syme having announced this discovery—a discovery of the greatest importance, not only to himself but to the profession, and still more to patients under that infliction, Mr Lizars publishes a book on it. He would not, gentlemen, let us see the book, but it is still for you to say how far you are to be influenced by the evidence you have heard of the position of the parties when the pursuer will not let you judge for yourselves on that subject. Well then, this book of Mr Lizars is taken up by a medical journal, and Mr Syme then addresses to the "London Medical Gazette" the letter which is here printed. I may observe in passing, that it is not the sending of that letter to the "London Medical Gazette" which is the charge with which you have to do. What my learned friend said about the circulation of the "London Medical Gazette" is what you have nothing to do with. It is, that he printed

that letter in its full import in the "Monthly Journal of Medical Science," published in Edinburgh. This letter is partly inserted in the "London Medical Gazette." You have read the letter as it was originally sent:—"Sir,—I have only to-day happened to see your journal of May 16, which contains some statements that certainly should not have remained so long unnoticed if they had been known to me sooner. You say, 'a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,' but you must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy. In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Mullar, are well-founded: but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth.]" Then he goes on to say that he has performed this operation nine times in the Edinburgh Infirmary, in presence of the largest clinical surgery class in the kingdom. "These gentlemen can testify that in no instance has there been bleeding, extravasation of urine, or any other unpleasant consequence, and that all the patients speedily and completely obtained the relief which they desired. As you say that 'something more than the guarantee of Mr Syme's reputation is wanting to assure the surgeon that he would be justified in having recourse to the proposed operation,' I beg to inquire if you think the evidence thus afforded sufficient, and if not, what further proof you deem requisite to establish the safety and efficiency of my operation?" Now, the first observation I make to you is this, that that is obviously a letter written by a party interested in that discovery, and for the sole purpose of its being fairly considered by the journal. "I beg to inquire if you think the evidence thus afforded sufficient, and if not, what farther proof you deem requisite to establish the safety and efficiency of my operation?" Then that letter is not written with the object or intention of abusing anybody else. The object was a fair and it was a laudable and legitimate object. It was to state the success that had attended that operation, and inviting the Editor of that journal to say whether the evidence was not sufficient. And I think the editorial note recognises the letter in that view. Therefore, the general object and purpose of the letter was a fair and proper object—to show that the allegations made as to its being attended with bad effects, were not to be taken for granted when it had been performed in the sight of the world and with perfect success. And it is very important in you, when you consider any publication, to inquire what is the spirit and *animus*—what is the legitimate object—whether the object was to abuse Mr Lizars, or whether the object was to recommend and make known the true state of the case with regard to that important surgical discovery. Very well, the "London Medical Gazette," from a feeling that I do not think it necessary more particularly to analyse, from caution, or perhaps from a desire to avoid giving offence, do not insert two of those passages, and they say they may be subject to the English law of libel. One of those passages is not intended to be charged as libellous—that second passage within brackets, the most important that weighed in their mind—the allegations made with regard to himself, in so far as they were declared to be utterly devoid of truth. He was entitled to state that; because if any man says that a surgical operation has been attended with bad effects, then a party is entitled to set the public right. It is necessary for the sake of truth it should be so—a stranger would be entitled to say so—much more the person performing the operation. He said, Do not here depend on the allegations of Mr Lizars and Dr Mullar; on the contrary, in so far as they attribute bad effects to my operations, they are devoid of truth—so he says, Do not assume that. Now the English people did not like inserting that passage, and just because the words "devoid of truth" were used. Why, be-

cause his own operations were spoken of, he was entitled to say so; and, accordingly, his Lordship will see that in the first part of the summons something was intended to be made of that. It is now out of the case, and so much for the implicit views of the English people in excluding what the pursuer, with all his desire to make a libel, could not make a libel of—for it was within the legitimate province of a man wishing to establish his discovery. But what further proof can you have of the fairness of Mr Syme, than that the letter published in this mutilated form does not satisfy him, and he therefore lays the entire letter before the Editor and Conductors of this other respectable Journal, and they, having their attention called to it, are satisfied that there is no libel intended. And I think the strongest feature in this case is the perfect honesty of Mr Syme, and I do not think anything shows the thing to be more free of libel than his inserting it as he did. And see what is said, "One of the Conductors of this Journal lately felt it necessary to address a letter of remonstrance to the Editor of the 'London Medical Gazette,' who published it in an imperfect form, under the pretext that the matter excluded would have been subject to the English law of libel. Two results have followed. In the first place, the letter is rendered meaningless." So it was. "And secondly, the author is made to appear having used libellous language. In order that our readers may judge how far this conduct was warranted, we now place before them the letter in its original form, the omitted portion being enclosed within brackets." To enable you to judge for yourselves, I publish it because I am satisfied that there is no libellous language in it. I may be wrong—I will come to that presently—but I am now speaking to the alleged hostility, and to show the *bona fides*, and I merely mean to account for the position in which I stood, so that I was not able to answer Mr Lizars. You have heard what Dr Robertson said—he said, in such a controversy, where Mr Lizars had written a book impugning Mr Syme's views, Mr Syme must either answer, or he would require to explain why he did not answer it. Why, for the cause of truth—that the truth should not suffer—that this operation might do good to thousands of men—he was bound to take notice of Mr Lizars' statements, to the extent of stating that he could not answer the same, because he owed him neither respect nor courtesy. Now, in reading a letter of this kind, it is your duty to read the words that are fixed upon as objectionable, and, in the first place, to give them no meaning beyond what they appear to bear; and, in the next place, to have regard to the context. My learned friend wants to begin with the words, "Regarding him as long placed beyond the pale of professional respect and courtesy." But you must not do that. It is assigned not as a reason why the Editor of the "Medical Gazette" should not review Mr Lizars' book, but that you, the Editor, must be made aware that I have not addressed a single word to Mr Lizars on the subject. I do not know if any damage has been suffered. I know this, that there are none amongst us to whom the relations of those two parties can be said not to be known. I know that the position which Mr Lizars and Mr Syme have occupied since I came into active practice, was such, that so far from being surprised, I should certainly have been anything but surprised, on reading this letter. We have not been allowed to go back into what previously happened, and I do not care. But here is the position which Mr Syme occupies. Mr Lizars writes a book, which he will not let you and me see, but others know about it, and are not surprised at this letter. They would have been surprised if Mr Syme had answered Mr Lizars' book; but still it was necessary to show that it was not for fear of encountering Mr Lizars in argument—not from having the worst argument—that it was not answered, but because that position prevented Mr Syme from entering into controversy with Mr Lizars. That is the statement that Mr Syme gives you of his views, and is it not borne out by the feeling of those gentlemen who did insert the letter, and who appear to be about the best evidences you can get about this matter, for their attention was called to this by what the English Editor had done? and they were, as far as Mr Deas tried to persuade them, responsible both legally and morally for what was inserted. In the next



place, they are now free from every risk, because they have been absolved from this action. But they had everything to fear then, and they tell you that, apart from pecuniary responsibility, "that if it had been the intention to call Mr Lizars a disreputable man in his profession, and that he was not entitled to respect and not entitled to courtesy from the profession at large"—they tell you that they could never have received it. But they tell you that they read it with the light of the facts in the controversy. How ought it to be read?—are you to take it to a boarding-school young lady who knows nothing of those matters? No, you must make it be read, in the first place, by a man who understands the language; and is it likely that any man would read it who did not know something about the parties, or as to what it relates? Mr Lizars may carry it about in a bottle-like manner, and go to people that know neither of the parties, and who say that they read it in a particular sense. That may be the case; but that will not influence your minds, who are to look first to the *animus* of Mr Syme. Did not those gentlemen tell you, on their oaths, that they never meant to make a calumnious representation against Mr Lizars, a gentleman living in their own town? On the contrary, does it not come to this, that it is the reason for Mr Syme not doing a particular thing—engaging in a controversy with Mr Lizars? Why? Because Mr Syme regarded him as not entitled to respect or courtesy from him—not from others. He does not say that others are not to enter into controversy, but I may tell you why I do not enter. And it was a very innocent mistake in Mr Syme to suppose that the people who read those controversies should have known something about their previous history, which all the medical men know about. But Dr Carpenter tells you that if you had read that book of Mr Lizars, that tells you all about the previous quarrel, you would know everything that tended to make this personal estrangement a bar of separation between those two gentlemen. It is not that Mr Lizars' general character, or his moral character, has been assailed, though one of the pursuer's witnesses, Dr Sibbald, thought that the moral character was involved, and that most perfect destruction would follow if the words had been applied to him, Dr Sibbald. The word "professional" is put in by the defender. He did not say that the moral character of Mr Lizars was disreputable. Mr Syme is not to be responsible for all the possible meanings these words may receive from people who are speaking of other things. Mr Syme is speaking of this thing alone—why he did not enter into controversy. And the adding of the word "courtesy," is very important, because as Dr Kesteven told you—though he did not particularly well arrange his ideas on that point—courtesy has nothing to do with ability. A man may have the greatest ability, and yet be not entitled to courtesy—a man, on the other hand, may have no ability, and yet be entitled to the greatest courtesy. Courtesy is not a thing which a man is bound to give to every man. It depends on the conduct of one man towards another. Now, the question is not whether Mr Syme was bound to regard him with courtesy. Might not any man say it lies with a party's own conduct to render himself respected? But as put in the issue, the question is, whether Mr Lizars was held out as a person of disreputable character. Now where is that got? Is not Dr Simpson right that that is a torturing of the meaning? Then the issue also bears, "as neither respected, nor entitled to receive respect,"—where is that to be found? There are some qualities which, when they are imputed to a man, or denied to a man, are purely personal to that man. If I say a man is dishonest, that applies to himself. If I say that a woman is unchaste, that is her own want of virtue; it is personal to her, and any imputation as to those qualities must be general. They depend on the inherent character of the man or woman spoken of. But when I speak of courtesy and respect, these are feelings that may be denied, as they are denied here by Mr Syme to Mr Lizars. But the passage merely means this, that Mr Lizars stands in such a position to Mr Syme, that the feelings of respect and courtesy are not the feelings in his mind—not the qualities which Mr Lizars commands from me—and my position was notorious to every medical man. I do not say that there might not be some who, taking it in one view or another, may not give it a torture. I cannot be



altogether answerable for that. But that is not the question. There may be persons who cannot follow the connection. But I look to men of intelligence, who can follow what is written, and who, in reading it, will not look to one-half of a sentence without the context; it must be men who will endeavour to find out the fair meaning of the parties. I have at least as good men as the pursuer—I say better men in the circumstances—who tell you what was their meaning. On the other side, you have men who refer only to one or two passages, and then give their construction;—some saying that the statement implies disrepute in the profession; others, like Dr Sibbald, that it reflects on the moral character of Mr Lizars. Then you come to those who were not in a situation to judge fairly; and I am not responsible for their opinions. I write for those who understand, or ought to understand; and I have very little regard to any man who would read this letter, and not see what it alludes to. Could not every medical man have told him what it alludes to—that Mr Lizars had been attacking Mr Syme with great cause of offence on one side, and that he was entitled to assign that as a reason for not entering into the controversy? That being the case, I submit it will not bear that construction for which the pursuer contends. Now the evidence I have adduced goes to support my construction; and I think it is complete proof, that the meaning in which it was received by the Conductors of the Journal was the definite meaning; and if that is the meaning, then that cannot be the meaning which is expressed in the issue. That is not the issue you are to try to-day. If the issue had said, that I did not regard Mr Lizars with respect and courtesy, I would say, No, I do not; and I have good reason; and his Lordship would have allowed me to prove it. But unless you are satisfied that it was my meaning that he was generally disreputable, you cannot find me guilty, or subject me to damages. None but the most ignorant man in the world would tell you that that was meant. It is well known that Mr Lizars was in that position with Mr Syme; but that he is not in that position with everybody. And did Mr Syme mean to assert a downright falsehood, which could have been proved by all his own professional friends? Therefore you must take the qualified meaning, which is the natural meaning in the circumstances, and not that meaning which, looking to the whole tenor of the letter, is obviously extraneous. Something has been said about damage; but is there anybody who has said that Mr Lizars has been actually injured, or that he has lost a single patient? It is said that the Journal is taken in by the Philosophical Institution; but has any of the blue-stockings who attend the lectures of that institution, been brought forward to say that they would not employ Mr Lizars in consequence? I maintain that Mr Lizars' character is not in issue in the sense in which he contends. That I disclaim from the first, and would not have published the letter with that view. Mr Syme was not to express what he did not feel; but he was bound to explain why he did not enter the arena, put on the gloves, and fight with Mr Lizars. But he did not say that anybody else might not have a fight with Mr Lizars. Gentlemen, that is my case. I submit that, as a vindication of Mr Lizars' character, nothing is needed, because it was never attacked. No doubt he wants to have it attacked; but the Journal disclaimed that intention. That was brought out by Dr Simpson. You have Mr Syme earnestly bound up in his own duty, which was that of recommending fairly his operation, and leaving to the public to decide on the merits of the discovery he had made. He explains why he had not answered Mr Lizars' book; and in a case of libel, I want you to take the natural and ordinary meaning, otherwise it would be most unjust. A man who did not mean to be offensive should not have words put into his mouth which he never intended to use. On the whole, I submit that the case is not made out by the pursuer, and that the passages founded on do not amount to a libel against Mr Lizars.

The LORD JUSTICE-GENERAL.—Gentlemen of the Jury,—I will now make some observations with the view of aiding you in arriving at a conclusion. The question put to you is in the third page, “Whether the whole or any part of the said article or statement is of and concerning the pursuer, and falsely, calumni-

ously, and injuriously represents and holds out the pursuer as a person of disreputable character in his profession?" The thing is printed in the Journal, and it is not disputed that the defender, Mr Syme, is the author; and the pursuer says that those words in the passage so often alluded to, hold him out as a person of disreputable character in his profession, and as neither respected, nor entitled to respect, in the medical profession. If it does so hold him up, no doubt that is a serious imputation, and such as entitles the pursuer to reparation. You must hold that the pursuer is not of disreputable character, and is not a person either not respected, or not entitled to respect. He is entitled to have that assumed in his favour,—1st, because the defender has not undertaken to prove the reverse; and 2dly, because there is positive evidence that he is not a person of that character, but quite the reverse. Therefore, if this statement does so represent him, he is entitled to reparation. The question is, Does it so represent him? If the defender had admitted that that was the meaning of his passage, and had intended to justify it, he would have set forth that the pursuer was of that disreputable character, and he would have stated why, and he would not have declined an opportunity of proving it; but as he has not so represented the pursuer, it must be held that there is no ground for the imputation. But what the defender says is, I did not so represent him at all; and the question you have to try is, whether the defender did so represent him. What the defender says is, I stated a totally different thing; and he recurs to the passage on page two, so often read, and he tells you, that what he meant by that passage is, that in the position in which he and the defender stood towards each other, by reason of past discussions, disagreements, and controversies, that he, the defender, could not treat the pursuer with that professional respect and courtesy due to a person with whom he was not so situated; and that was the reason why he did not enter into controversy. That is what the defender says. Now the words that are used in page two are not the same words that are used in page three; they differ. On page two the words are, "You must, or at least ought to know, that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy.]" But on page three the words are, that he held him out as a person of disreputable character in his profession, and as neither respected nor entitled to respect in the medical profession. Now these are not the words in the issue; but the pursuer says that is their true meaning, and he is quite entitled to say so. He is quite entitled to say that the words at page two have a meaning of this offensive and injurious nature, and to satisfy you that that is the meaning. He says, I shall satisfy you that the words on page two are used in the sense I attach at page three. Had he undertaken merely to show, or had he complained merely of this, that the defender had said of him that he, the pursuer, was in such a position in regard to him, the defender, by reason of former quarrels and disputes, that he had not treated him with that respect and courtesy that he would have treated another, by noticing his publication,—if that had been the ground, then the defender would say, I did say so; I adhere to it; I had sufficient reason, and I shall prove it. But the pursuer did not say that. He says the defender meant something totally different from that; and therefore it is out of the question for the defender to prove that he meant so. Therefore you cannot hold that the pursuer is now entitled to fall back on that other and different construction of the words, and to say, Although I have not proved what I have undertaken, I have proved something equally bad. If that was the case, the defender might have taken an issue in justification; but he was precluded by the pursuer saying, You did not mean that. What you meant was, that I was a person of disreputable character, and neither respected nor entitled to respect. Now this second meaning implies something directly the reverse of the first meaning; but it is the meaning that the pursuer puts at page three. Now you have it as facts in the case, that there were controversies between these parties; that they were in such a position towards each other, and that was previously well known to the profession in Edinburgh at least; that there was not much

professional feeling or courtesy by the one towards the other,—that was spoken to by various witnesses connected with Edinburgh, examined by the defender; and, further, that their position was such, that nobody could suppose that the defender was to regard the pursuer with those feelings of professional respect and courtesy with which he would regard other members of the profession. It also appears that there had been a publication by the defender on the subject of this operation. It appears also that there was a publication of the pursuer on that subject. And those publications led to comment in these medical periodicals; and it was in consequence of one of those articles in one of the periodicals that the defender wrote this letter of 26th June 1851. It was addressed to the Editor of the London "Medical Gazette," and says, "You say in your Journal that a fierce paper war has arisen between the two Edinburgh professors, Syme and Lizars," and so on. Now it is stated by evidence, uncontradicted also, that with regard to the observations made on the defender's operation, some attached a great deal of importance to the same—that the observations made by the pursuer were such that required notice—that the defender was entitled to vindicate his own reputation in regard to his own discovery—that he was entitled to expose the erroneous nature of the attacks made on it, and to place it in the most favourable light; and that the matter had got into that position, that it was expected he should make some reply. And in that position, he comes forward with this letter—"You (the Editor) say, 'a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,' but you must, or at least ought to know, that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy.]" Then he goes on to say—"In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Mullar, are well-founded; but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth." In that passage there is nothing founded on—it is within the fair scope of discussion; but in regard to the first passage, he was entitled to declare why he did not enter into controversy, but he was bound to do so in proper language; he was not entitled to impute to the pursuer that he was a person of disreputable character and not respected, nor entitled to respect. And while he was entitled to explain why he did not notice that, it is alleged by the pursuer that he has gone beyond the proper limits, and accused the pursuer of being disreputable in the profession; and if you are satisfied that he has done that—and you must be satisfied that it is false—then the pursuer has made out his case. But the question always comes back to that, Has the defender really said so? Some have been brought to say that that is the construction they were disposed to put on the passage; others, that they put a different construction on it. These latter witnesses say they do so from their knowledge of the previous discussions, and of the bad blood, if I may so speak, between those parties; and as such, it did not surprise them on seeing that the defender could not treat the pursuer with proper professional respect. Now it was proposed, on the part of the defender, to give you an opportunity of seeing that it was well known to the medical profession that there had been a running fire kept up between these parties for the last ten years. I did not allow that to go on—we had enough to do without it; but it is a fact that is proved, that they were in that antagonistic position, and that they were in that position, that if the defender had confined himself merely to the expression of that which he says that he intended to say, there would have been nothing wrong about it. But did he only intend to say that? It is said that the language is ambiguous; and that, although he intended so to confine himself, he had not made it so clear but that other people would construe it as the pursuer has done, and thus render him liable to damages. It is of consequence that you

should get to what was the real meaning ; that is the key to the construction. No doubt, a party is not entitled to use two-edged language, to state his meaning in one way and find escape for himself in another way—nor even if he did not mean it in that sense, but was careless of his language, which had led to damage, and thereby injure the party. But you will consider what is the meaning that you will impute to it, looking to the whole circumstances of the case. Do they, or do they not, mean that the pursuer was a person of disreputable character in his profession, not respected, nor entitled to respect ? It was remarked by the counsel of the defender, that respect is a thing exclusively in the mind of others, not in the character of the man himself. That is quite true according to the defender's construction, but the pursuer's construction is totally different. He, the pursuer, construes that it imputes disreputable character, and that the defender is not entitled to regard these as qualities in him. Now the words, taken by themselves, may apply to the feelings of others, and do not imply qualities inward in the pursuer. But the pursuer's meaning necessarily implies qualities in himself—and you are to judge whether they were used in the one sense or the other. If you think there was a reason for using the expressions in the one sense, and none for using them in the other, there may be a probability in the defender's construction. Then you will inquire further, whether there is room for a double meaning, and that the words are so recklessly used that they do carry a double meaning. Now, the witnesses who have spoken to the defender's interpretation are witnesses from Edinburgh, from Berwick, and Dumfries. These gentlemen are all aware of the position of the parties, and it seems it was generally known in the profession in Scotland, and as far as Berwick-on-Tweed. Dr Carpenter, who read of this matter, had read it in the same light ; and Dr Carpenter told you that any person who had read the pursuer's book must have been aware of the position in which the parties stood. On the other hand, witnesses are examined on the part of the pursuer—not so far away, but you had a gentleman from Dalkeith, Dr Renton, a highly respectable man ; and Dr Sanderson, from Musselburgh ; and Dr Sibbald, and Professor Miller, who are not out of the atmosphere of medical contention here, and they put the construction on it which the pursuer does. Dr Simpson considered that strained, and said it was quite another thing that that construction might be extracted out of it. You will consider whether it is a fair and reasonable construction ; and, if so, then the party must be responsible. But if, on the other hand, this is only a strained construction, then, looking to what was the meaning, it is important to look to the views of Dr Christison and Dr Robertson, and to consider the grounds on which the letter had been rejected in England, and admitted into their Journal, because they held that they could attach no such meaning to it as had been assumed in England—that meaning Dr Christison thought quite extravagant. If you go into that view, then you will be disposed to take the interpretation put on it by the defender. It is very much to be regretted that these controversies should arise, and that those two eminent individuals should occupy so much of their valuable time in this way ; but still it is so, and we must do our best in the battle that has arisen between them. But it is for you to say whether the meaning put on it by the pursuer is the fair meaning, or whether you think that is not the fair way of dealing with it ; but that, on the contrary, and on the evidence of the whole matter, you think all that was meant was, that the pursuer was not a person whom the defender, in the circumstances in which they were placed, could regard with professional respect and courtesy. Some of the witnesses say, that it might have been expressed, as placed beyond the pale and respect of the profession so far as regarded him (Mr Syme). But it is not so said. It is for you, then, to say, whether the pursuer's is a fair or strained meaning—if a strained meaning, you will find for the defender, if there appear to be no such reckless use of language so as to give rise to a double meaning, whatever was the intention of the writer. Then as to the damage, neither party has said anything about it, and you will

deal with it as you think proper, if you find the pursuer has made out his construction of the words. But if, on the other hand, you don't think the pursuer has made out his construction of the words, then you have nothing to do with the consideration of damage.

The jury then withdrew, and returned after an absence of about twenty minutes, when the chancellor announced that they were unanimously of opinion that no dishonourable motive was intended by the defender in the publication of the letter. The following is the signed verdict:—

“At Edinburgh, the 26th day of July 1852, before the Right Honourable the Lord Justice-General, appeared the said pursuer and the defender, the said James Syme, Esq., by their respective counsel and agents; and a jury having been balloted, and sworn to try the Issue, marked No. 9 of Process, and authenticated by Lord Anderson on the 28th of May 1852, between the said parties, say upon their oath that, in respect of the matters proven before them, they find for the defender.”

*Counsel for the Pursuer*—George Deas, Esq., and Robert Macfarlane, Esq.  
*Agents*—Messrs Inglis & Leslie, W.S.

*Counsel for the Defender*—Charles Neaves, Esq. (Solicitor-General), and George Patton, Esq. *Agents*—Messrs Smith & Kinnear, W.S.

[Taken in short-hand by Mr D. Buchanan, Editor of the “Caledonian Mercury.”]

#### CAVENDISH SOCIETY.

The following extract from the “Report of the Council to the Fifth Anniversary Meeting of the Cavendish Society,” may interest some of our readers, and induce them to enrol themselves as members of the Society.

“Two books have issued for 1851—namely, the first volume of Lehmann’s ‘Physiological Chemistry,’ and the sixth volume of Gmelin’s ‘Hand-book.’ The former of these works will be completed in three volumes, the second of which is now in progress, and will constitute one of the books to be supplied to the members this year. The sixth volume of the translation of Gmelin’s ‘Hand-book’ concludes the Inorganic part of this work, in the production of which the Society has enriched the scientific literature of the country with a complete and systematic exposition of the existing state of knowledge upon the subject to which it relates. The desire to make this work generally available to British chemists was one of the motives which originally contributed to the establishment of the Cavendish Society; and the almost unanimous approbation, which has been expressed by the members, of the selection which the Council made of this as their first great publication, has induced them to persist in applying nearly all the means at their command towards the completion of the Inorganic part, now finished, before undertaking other works which have been in contemplation.

“In order to meet the wishes of those who may be anxious to join this Society, with the view of possessing Gmelin’s work, the Council have arranged that the sixth volume may be substituted, when desired, for the volume of ‘Chemical Reports and Memoirs,’ which is out of print, as one of the books for the subscription of 1848, by which means the six volumes of the Inorganic part of the ‘Hand-book,’ together with the ‘Life of Cavendish,’ may be obtained for three years’ subscription—namely, 1848, 1849, and 1850. It has been arranged also that gentlemen commencing to subscribe for 1851, may have the option of taking the ‘Life of Cavendish,’ instead of the sixth volume of Gmelin’s ‘Chemistry,’ as the book which is given in addition to the first volume of Lehmann’s ‘Animal Chemistry’ for that year.”



## OBITUARY NOTICE.

We have to record the death of a distinguished member of the Madras medical service—Physician-General John Wylie, C.B., who died at his residence in Clackmannanshire, on the 16th of June. Dr Wylie entered the E.I.C. service in 1813, and rose through the various grades, until he became senior member of the Medical Board. He finally retired from the service in 1851, receiving, at the time of his departure from Madras, the merited compliment of a general order of the Governor in council, expressive of the high sense entertained of his “highly meritorious services during a lengthened period of thirty-seven years.”

Dr Wylie was one of the medical officers of the Queen's and E.I.C.'s services, who were, in 1850, selected for admission into the Military Order of the Bath. The honour thus conferred on medical officers of the public services has been universally recognised as a merited, though somewhat tardy, compliment to the profession generally, and to the individuals who were thus distinguished. To none of them was the admission into a military order more appropriate than Dr Wylie, who, at an early period of his career, was called upon to render to his country a service more purely military, than it commonly falls to the lot of a medical officer to perform. The exploit is notorious to every servant of the E.I.C.; but many of our readers may not have heard of the affair of “Corygaum,” where one professional brother fell, and another bore a conspicuous part in achieving a victory under desperate circumstances.

The following account of this remarkable combat is taken from a divisional order of Brigadier-General Smith, of 7th January 1818.

“The commanding officer having received the official accounts of an attack made by the Peishwah's army on a small detachment, commanded by Captain Staunton, of the 2d battalion 1st regiment Bombay N.I., at the village of Corygaum, has great satisfaction in publishing the particulars for general information, and in holding it up to the force, as one of the most brilliant examples of gallantry and perseverance recorded in our Indian annals.

“This detachment, consisting of a detail of Madras artillery and two 6-pounders, 2d battalion 1st regiment N.I.—about 600 strong, and about 300 auxiliary horse, the whole under Captain Staunton, marched from Seroor to Poonah, at eight p.m., on the 31st December, and reached the heights overlooking Corygaum about ten o'clock in the forenoon, 1st January, from whence the whole of the Peishwah's army, estimated at 20,000 horse, and several thousand infantry, were discovered in the plain south of the Beemah River.

“Captain Staunton immediately moved upon the village of Corygaum, with the intention of occupying it, and had scarcely succeeded in reaching it with his detachment, when he was attacked in the most determined manner by three divisions of the Peishwah's choicest infantry, supported by immense bodies of horse, and the fire of two pieces of artillery.

“The enemy's troops were stimulated to their utmost exertions, by the presence of the Peishwah on a distant height, attended by the principal Mahratta chiefs, who flattered his Highness with the prospect of witnessing the destruction of this gallant handful of British troops.

“The enemy obtained immediate possession of the strongest parts of the village, from which it was found impossible to dislodge them, and the possession of the remaining part was most obstinately contested from noon till nine p.m., during which time almost every pagoda and house had been repeatedly taken and retaken, and one of the guns at one time was in possession of the enemy.

“Towards the close of the evening, the detachment was placed in the most trying situation. At this period nearly the whole of the artillerymen were killed or wounded, and about one-third of the infantry and auxiliary horse. The exertions



which the European officers had been called upon to make in leading their men to frequent charges with the bayonet had diminished their numbers. Lieutenant Chisholm of the Artillery, and Mr Assistant-Surgeon Wingate, 2d battalion 1st Bombay N.I. were killed; and Lieutenants Swanston, Pattinson, and Connellan were wounded, leaving only Captain Staunton, Lieutenant Jones, and Mr Assistant-Surgeon Wylie nearly exhausted, to direct the efforts of the remaining part of the detachment, who were nearly frantic from the want of water, and the almost unparalleled exertions they had made throughout the day, without any sort of refreshment, after a fatiguing march of twenty-eight miles.

"Under cover of the night, they were enabled to procure a supply of water; and at nine P.M. the enemy were forced to abandon the village, after sustaining an immense loss in killed and wounded.

"The British character was nobly supported throughout the whole of this arduous contest by the European officers, and a small detail of Madras artillery.

"The medical officers also led on the sepoys to charges with the bayonet, the nature of the contest not admitting of their attendance to their professional duties; and in such a struggle, the presence of a single European was of the utmost consequence, and seemed to inspire the native soldiers with their usual confidence of success.

"At daylight on the 2d, the enemy were still in sight, but did not renew the attack, although it prevented the troops, whose ammunition was nearly expended, from procuring any supply of provisions.

"Captain Staunton, however, made preparations for moving according to circumstances; and the manner in which that officer availed himself of the few resources which remained to him after such a conflict, to prosecute his march and bring away the numerous wounded of his detachment, is highly praiseworthy.

"The detachment moved, during the night of the 2d, upon Seroor, which they reached at nine o'clock on the forenoon of the 3d, having had no refreshment from the 31st December.

"Captain Staunton brought in nearly the whole of the wounded, and both guns and colours of the regiment, which the enemy had vainly hoped to present as trophies to the Peishwah."

Dr Wylie's own account of his share in this transaction is highly characteristic of the modesty which distinguished him. Writing to a friend a few days afterwards, he says:—"Swanston, who was wounded twice—severely, very early in the day—I took to a pagoda, dressed him, and also Lieutenant Connellan, and some others; but I did not remain long, finding it absolutely necessary to render the two remaining officers all the assistance in my power, in another way."

Some of the circumstances connected with the action at Corygaum are illustrative of the neglect with which it was formerly too much the custom to treat the services of medical officers. In a general order by the Governor in council, promulgated in the succeeding month, embodying the very divisional order from which we have quoted above, and also in a general order by the Commander-in-Chief, we have the thanks of Government conveyed to the captain and lieutenants, to the native commissioned and non-commissioned officers and privates, but the name of Dr Wylie is omitted from both. It is, however, only just to the memory of the Governor-General, the Marquis of Hastings, to state, that, in his general order of March 18th, Dr Wylie is mentioned by name along with the other officers.

We hope that the late elevation of some half-dozen medical officers to the rank of C.B. may be taken as evidence of a kindlier feeling towards our brethren in the public services, on the part of those in authority; and that the recent examples in Africa and Burmah, of medical officers being not only exposed to fire, but also wounded, will induce those who have the bestowing of honours, to dispense them more liberally to our brethren, though they be professedly non-combatants.

MR COULSON.

*To the Editor of the Edinburgh Monthly Journal of Medical Science.*

SIR,—I must beg permission to reply, briefly, to some remarks in your last Number, relative to the history of the perineal section. I cannot plead guilty to your charge of “having desired to conceal the object of the operation” quoted by me from Desault. Although the passage which you have marked in italics was omitted by me (and I cannot now explain to myself how I omitted to copy it), the omission was not intentional; nor would it have the effect, as you seem to imagine, of concealing Desault’s true meaning, because, in a line or two afterwards may be found—“Mais aussi elle n’offre aucun avantage dans le traitement des retentions d’urine”—words which clearly prove that the author, in this passage, was speaking of an operation applicable to retention of urine.

At the conclusion of your remarks, it is affirmed that “Desault nowhere mentions the operation of boutonnière, except as a mode of relieving retention of urine.” Now, I assert, in the most positive manner, that Desault does speak of external incision on a grooved sound (*une sonde cannelée*), as one of the modes employed for the treatment of stricture independently of retention of urine. The passage in which Desault alludes to this method, may be found at page 245, vol. III. of his work—“L’opération connue sous le nom de boutonnière, quoiqu’ en apparence mieux adaptée à la nature *de la maladie* est presque toujours ou inutile ou dangereuse. Elle est inutile, si, pour la pratiquer, on peut passer un catheter ou une sonde cannelée.” The words which I have marked in italics, allude to the stricture, and not to retention. I have no desire whatever to detract from Mr Syme’s merits; but, in lecturing on any subject, I am bound to lay what seems to me the truth before my hearers.—I am, Sir, your obedient servant,

WILLIAM COULSON.

London, July 22, 1852.

If Mr Coulson will again refer to the article in the Monthly Journal, he will find that we did not charge him with “*having desired to conceal the object of the operation.*” The two words printed in Italics are his own, not ours. We have, however, great pleasure in giving insertion to his explanation; and are perfectly satisfied with his assurance that the omission which we pointed out was unintentional, and that he had no sinister design in his remarks on Mr Syme’s operation.

But we still maintain our expressed opinion, “that Desault nowhere mentions the operation of boutonnière except as a mode of relieving retention of urine.” If Mr Coulson thinks otherwise, we can only say, that he ought to have adduced some better reason for his belief than the passage above-quoted from Desault; which indeed proves to demonstration the very fact which we are seeking to establish. The boutonnière is here alluded to—according to our apprehension of the context—not as a cure for stricture, but as a substitute for puncture of the bladder, and for other procedures applicable to *la maladie*,—i. e., to retention of urine from stricture. Besides, even for this purpose, Desault far from recommending *la boutonnière*, explicitly condemns it as *almost always either useless or dangerous*. He characterises it as *useless if a grooved sound can be passed through the contracted part of the canal*—a preliminary condition which Mr Syme declares to be essential to the performance of his operation.

Any unprejudiced person, who understands French, and will take the trouble to consult the valuable work of the great French surgeon, will confirm the literal accuracy of our statements.

## COD-LIVER OIL A REMEDY FOR LUPUS.

The great efficacy of cod-liver oil in cases of lupus, has been pointed out by various authorities. Dr Teirlinck, of Ghent, has lately communicated to the "Annales de Gand" his experience of the remedy, given in large dose, in the case of a man twenty-three years of age, with extensive ulceration on the sternum, neck, and entire left cheek, which had commenced four years before. Cod-liver oil was ordered for the patient, commencing with *half a pound* per diem, in two doses: five days afterwards, *one pound*; in eight days, a *pound and a half*; and after twelve days, *two pounds* per diem were prescribed. In six weeks some symptoms of indigestion manifested themselves and continued for fourteen days, during which period the use of the oil was suspended. It was then resumed in gradually increasing doses, till after the lapse of two months the daily quantity of *three pounds* was reached; a dose which was persisted in for six weeks. The cure went on steadily, and the patient, rallying from his exhausted condition, assumed the look of health. During his treatment, between the 6th December 1850 and 13th July 1851, he had used 265 pounds of cod-liver oil, and although at first there were occasional slight irregularities of appetite, he soon began to tolerate a nutritious diet. Hebra, of Vienna, has also recommended the administration of large doses of cod-liver oil in cases of lupus, and lately exhibited to the society of Viennese physicians, a patient in whom a lupus hypertrophicus had been almost thoroughly eradicated, between the 27th March and 17th November, by the use of 708 ounces of the oil taken during that period.—*Nederlandsch Weekblad*: Maart 1852, p. 108.

## SUCCESS OF A CHINESE STUDENT IN EDINBURGH.

We have much pleasure in recording an interesting fact connected with our Medical School. A native of China—Wong Fùn, who commenced his medical studies here last winter, has gained the highest prize in the botanical class, for excellence in the monthly competition, by means of written questions and answers, as well as by the examination of fresh specimens of plants, without any aid from books or notes.

In this very searching examination there were in all 114 questions given, and the gainer of the prize had to encounter 66 competitors.

## THE MEDICAL DIRECTORIES.

The Publishers of the three Medical Directories for England, Scotland, and Ireland, are now collecting materials for their volumes for 1853, and it is again our duty to urge upon our readers the expediency of carefully filling up the blanks in the printed circulars now in their hands. It is only by the co-operation of the medical profession that these works can be made generally useful, and we trust that this co-operation will be cheerfully extended to the Publishers. We are content that the Editors should, as heretofore, exercise their own discretion in stating the titles and pretensions of all who return the schedules issued from 4, Adam Street, Adelphi. We anticipate no harm, but, on the contrary, much good, from homœopaths *et hoc genus omne* being permitted the utmost latitude in exposing their peculiar claims to public confidence.

## TO OUR READERS.

In consequence of our desire to furnish our readers with a full report of the Jury Trial which occupies so large a space in our present Number, we have ventured to defer our publication for a few days, and to postpone our usual reviews of new publications.

## PUBLICATIONS RECEIVED.

Ten Numbers of the *Nederlandsch Lancet*. Edited by Professors Donkers and Jansen. (This exchange, which we highly value, will now, we trust, proceed regularly.)

*Illustrirte Medicinische Zeitung*. München. Nos. 2, 3, and 4. (The Editor begs to thank the Publisher for his communication. The exchange will be conducted with due regularity.)

*The Physical Diagnosis of Diseases of the Abdomen*. By Edward Ballard, M.D. London: Taylor, Walton, & Maberly. 1852.

*Clairvoyance and Practical Mesmerism*. By James Esdaile, M.D. London: Bailliere. 1852.

*The Prescriber's Complete Handbook*. By M. Trousseau and M. Reveil. Edited,

with Notes, by J. Birkbeck Nevins, M.D. London: Bailliere. 1852.

*Atmosphere: a Philosophical Work*. By George Woodhead, Esq. London: Bailliere. 1852.

*Twenty-Second Annual Report of the Belfast District Asylum for the Insane*. By the Resident Physicians. Belfast. 1852.

*Die Lehre vom Hornhaut-Staphylom* von W. Roser, Prof. der Chirurgie in Marburg. 1851.

*Dictionary of Domestic Medicine and Household Surgery*. By Spencer Thomson, M.D. London: Groombridge and Sons. Part VII.

*On Diseases of the Kidney*. By George Johnson, M.D., London. London: Parker and Son. 1852.

## Part First.

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### ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Hydrophobia.* By J. A. LAWRIE, M.D.,  
Professor of Surgery in the University of Glasgow.

(*Read before the Glasgow Medico-Chirurgical Society, 13th July 1852.*)

ON Wednesday, 9th June, I got a note from my friend, Mr Hislop, of Renfrew; requesting my immediate attendance, as he feared he had met with a case of hydrophobia. I instantly obeyed the summons; but, before detailing the symptoms which I observed, I would call your attention to the following statement, with which Mr Hislop has most kindly furnished me, and which I give nearly in his own words:—

On Tuesday, 8th June, Mr Hislop was hurriedly called, about mid-day, to see Miss M. M., a robust healthy-looking girl, of twenty. He was informed that on Monday, 7th June, she had consulted Dr Robertson for symptoms which appeared to him to be hysterical. Dr R. prescribed a saline laxative, which did not relieve her. Next morning, the character of the symptoms appearing unchanged, the aromatic spirit of ammonia was ordered in repeated doses. The girl complained to Dr R. of depression of spirits, and general uneasiness, but especially of globus, and a feeling of suffocation. The only peculiarity, hardly noticed at the time, was that she said,—“I cannot swallow the medicines.” In fact, difficulty of swallowing did immediately occur during her attempts to take each dose of the aromatic spirit, and, with accompanying spasms in the throat, had so much increased, that when she was offered her mid-day dose, she threw the glass from her, rushed down stairs, and casting herself into her aunt’s arms, exclaimed she was dying. It was then that, in consequence of Dr Robertson’s absence, Mr Hislop was hurriedly summoned, and saw her for the first time. He found her in considerable distress, imploring relief from a ball of wind in her throat, which she every moment expected would suffocate her. She made frequent efforts “to break the ball,” and occasionally succeeded

after much retching, in getting rid of a little flatus. Along with this she had distinct spasmodic paroxysms, occurring perhaps every five minutes, during which she clutched her throat with both her hands. She seemed much excited. Pulse, 120; bowels very constipated, no stool for eight days; catamenia quite regular. The opinion Mr H. formed coincided with that given by Dr Robertson, that the case was one of acute hysteria. He immediately put gr. i. ss. of tartar emetic into a teaspoonful of water, and with considerable difficulty persuaded the young woman to swallow it, which she did with a peculiar snap and some effort. In ten minutes he repeated the dose, and remained with her three quarters of an hour. She did not vomit, but her pulse fell, and the spasms were quieted. He ordered a purgative enema, and left her. During this lengthened visit a circumstance occurred, which is worth mentioning. The girl was engaged to be married; her intended called, and remained with her about twenty minutes. Immediately before he entered the room, she was complaining very much of the spasms in her throat; but no sooner was she aware of his presence than she seemed to forget her ailments, and talked, laughed, and jested, as if she were quite well. No sooner, however, did he leave her than her symptoms returned. Mr H. called at eight in the evening, and found her much better; the enema had acted freely, and during its operation she had vomited a quantity of bilious matter. She was in good spirits, and attributed her illness to constipation and "cold."

June 9th.—At one o'clock in the morning, Mr H. was roused by a loud and urgent summons to visit his patient. He found that, although she had been comparatively quiet in the early part of the night, she had not slept. The spasms had returned, and been much aggravated by an attempt to drink some ginger beer. They were now almost without intermission, and so violent that when they came on she would spring suddenly out of bed, and cling to those beside her in an agony of despair. Her pulse was 150, and very full. She was still making constant efforts "to break the ball in her throat," and complaining of pain, which he attributed to these efforts, in the regions of her heart and stomach. He resolved to bleed her, and with some difficulty, on account of her constant change of posture and unsteadiness, took fully  $\text{xxxiv}$ . of blood. The spasms subsided to a slight occasional sigh, and she seemed much relieved. She soon complained of thirst; the offer of water seemed to produce a spasm, which was renewed by her attempting to swallow it, but she appeared to get over a mouthful with some difficulty. She next said she was hot. Mr H. lifted his hat, and began to fan her face gently; but each impression of the air on her face brought back the spasm. The suspicion of hydrophobia now for the first time crossed his mind. At this juncture he had some conversation in another room with his patient's aunt, with whom she resided. No other person being present, he took the opportunity of asking if her niece had ever been bitten by a dog. She seemed surprised at



the question, and, after enjoining him to secrecy, said, that her favourite dog, Neptune, "had set upon her, the day before he died, and scratched her back." (The history of the dog, and of this attack, will be given in the sequel.) On returning to his patient he found her much easier, and left her at four o'clock, with directions that she should have a purgative enema at six o'clock, if awake.

*At half-past eight A.M.*, Mr H. was again hurriedly summoned. The enema had come off, and brought with it *only a little blood*,—since which the paroxysms had gone on increasing in frequency and violence, and the mere idea of swallowing seemed to induce them. She had occasional vomiting of bilious matter. Turpentine was applied to the back and chest, mustard to the feet, and a blister to the back of the neck,—all without benefit; indeed the vapour of the turpentine proved a source of great annoyance. By *eleven A.M.* the vomiting had become almost constant, and soon assumed the appearance of "coffee grounds."

I saw her about a quarter to one P.M., and was much struck with what I witnessed. On entering the bed-room, I saw a robust, fine-looking young woman in bed, seemingly under great suffering. She turned her head towards me as I entered, with a sudden movement, and I shall not soon forget her wild expression as she gazed at me for an instant, and threw herself back on her pillow. I especially remarked the following particulars:—

(a) The flash of her eyes—wild, excited, half-hopeful, half-defiant—such as I had never remarked in a sane patient before.

(b) Her great suffering. Her constant cry was,—“Oh! what I'm suffering! Oh! what suffering! What have I done that I should suffer thus?”

(c) *Her restlessness and strength.* She was never for one moment at rest. She moved with ease, and any attempts to restrain her movements were difficult, and required considerable effort on the part of the attendants.

(d) *Salivation.* A frothy saliva was constantly collecting in her mouth and between her lips. With her left hand she wiped this saliva from her lips, and rubbed it on the edge of the bedding. I particularly remarked that, when her left hand was unoccupied, she did not spit, but cleared away the saliva with her thumb and forefinger, rubbing it on the bedding, or spattering it about, regardless of where it fell. When her left hand was held, and she was not vomiting, she was unceasingly spitting, and such was the quantity discharged, that before I was many minutes beside her my hands and dress were spattered over with the white froth, which issued in profusion from her mouth.

(e) *Vomiting.* This was, perhaps, her most distressing symptom. It was almost constant, difficult, painful, and straining. The quantity of matter discharged was not great, but was slimy, and of a "coffee grounds" appearance.

(*f*) *Thirst—desire for liquids—and power of swallowing.* She complained of urgent thirst, with burning in her mouth, throat, and stomach. She did not appear to have any dread of liquids, nor did she exhibit any horror when it was proposed that she should drink to quench her burning thirst; on the contrary, she freely discussed what would be best for her, asked if she might have ginger beer, and showed no annoyance when her aunt tried to jest with her on the love she had shown for so vulgar a beverage as gin and water. When liquid was actually brought to her, the following circumstances were well marked. She would not allow any one to put it into her mouth, but sat up in bed, and most reluctantly, and at last with a quick movement, seized the glass. She held it for an instant with a convulsive grasp, her hand quivering and spilling a portion of its contents, and then threw (pitched) the remainder into her mouth. Having emptied it, she cast the glass from her, regardless where it fell, and convulsively clutched her throat with her hand. A rapid, violent, convulsive movement of the throat and neck followed, and the fluid was jerked out by a motion not unlike that by which it was pitched in. The attempt to swallow increased the irritation of the stomach, and she immediately threw herself across the edge of the bed, and renewed her painful straining vomiting. It was very doubtful if she *swallowed* any liquid,—a convulsive closure of the pharynx seemed to arrest it, and eject it with an irresistible and powerful effort. Her existence at this time may be described as consisting of an unceasing round of tossing, complaining, salivating, vomiting, with convulsive movements in her throat, and a sense of suffocation from a never-absent indestructible ball, to relieve which she clutched her throat with her hands, as if she would tear out what she could not otherwise rid herself of.

(*g*) Her pulse was at least 150, regular, but not strong.

(*h*) The intellectual faculties seemed unimpaired. She moved her tongue and jaw with perfect facility, and articulated quite distinctly and with great volubility. There was no appearance of mania or furor—not the slightest disposition to hurt herself or any one near her; on the contrary, her manner to her attendants was kind. There was no peculiar sound of her voice, nothing which could at all be likened to the barking of a dog.

(*i*) She did not appear in the slightest degree to connect her illness with the bite of her favourite dog, or to have the least suspicion of the nature of her disease. She was early impressed with the idea that she was to die, and told her aunt that her “doctors did not know what was the matter with her, and she wished that her body might be opened after her death.” This was said on Tuesday forenoon, before her medical attendants were at all alarmed as to the issue of her illness. She showed no fear of dogs. On the forenoon of the day on which she died, a terrier having come into her bedroom, her aunt ordered it to be put out. She seemed sorry when it left her, and, looking after it, said, “Poor Jack!”

Such, so far as I can recollect, are the principal circumstances which I remarked in this poor girl.

The vomiting of coffee-ground matter, with burning in her throat and extreme suffering, being very prominent symptoms, I confess that the suspicion immediately crossed my mind that the case was one of poisoning. I therefore gave directions that the matter ejected should be carefully preserved. I took a part of it with me to Glasgow, and gave it to Dr R. D. Thomson, who, with great inconvenience to himself, but with that desire to oblige, and to make his high chemical attainments of use to his less qualified brethren, for which he is so much distinguished, immediately ascertained that it contained no acrid inorganic matter. I thought it necessary to communicate my suspicions to the girl's aunt, as well as to her medical attendants, and some circumstances being mentioned which rather seemed to corroborate my views and to implicate our patient, I, with their sanction, indeed at their express desire, stated them to the poor girl herself. She gave them a distinct, emphatic, and circumstantial denial, but showed no anger or irritation that I should entertain them. Her answers and her manner satisfied me on two points already adverted to—that her intellect was quite entire, and that there was a total absence of furor or mania. In a word, that the peculiar symptoms which I witnessed arose from overwhelming bodily suffering, and not from mental aberration.

I remained with her for fully two hours, during which she had two large opiate enemata, and hydrocyanic acid by the mouth. As I have already said, she did not refuse to attempt to swallow, but I doubt very much if any liquid reached her stomach. The medicine was pitched into her mouth, and the glass thrown from her hand, in the manner I have already described.

I had chloroform with me, and after the first opiate enema I proceeded to try it in the usual way. Great difficulty was experienced in getting her to inhale it. Not one instant was she at rest. Her hands being held, and her head steadied, I held the napkin before her face; but her gurgling breathing and livid countenance soon warned me that I must desist. Had I persisted, I am satisfied she would speedily have been suffocated.

I sat beside her, watching the symptoms, for an hour; and seeing that ʒij. laudanum by enema had no effect, and that the exhibition of medicine by the mouth was hopeless, I determined to try the chloroform again. Having laid her upon her back, with her head raised and steadied, and her hands forcibly held, I kept the napkin well saturated with chloroform opposite to, but two or three inches from, her face. She was soon gently under its influence, but never to complete insensibility. The effect on the symptoms was that the vomiting completely ceased, and the restless jactitation disappeared, or was easily controlled; but she did not cease hardly for one instant to speak in the most energetic voluble strain, and the froth continued to issue in considerable quantities from her lips. Her pulse varied,

but frequently fell to little above, sometimes even below, 100. I continued to hold the napkin and watch the effect of the chloroform for about an hour, and having satisfied myself that, whatever the ultimate result might be, the inhalation was alleviating her sufferings, I left her, suggesting to Dr R. and Mr H. the propriety of continuing the chloroform as long as they thought they could do so with safety. I returned in the evening, and found that shortly before my arrival she had been relieved by death from her terrible sufferings. Mr Hislop informed me that the chloroform had been continued with great relief till about half-past four o'clock, when the vomiting and spasms returned, and the pulse became so feeble that it was deemed prudent to omit it. The difficulty of swallowing continued to the last. "About twenty minutes before her death," says Mr Hislop, "she complained of thirst, and I offered her a glass of gin and water. On taking it in her hand she became violently agitated, and in attempting to carry it to her mouth spilt it on herself and her attendants. She said it was the gin which prevented her swallowing, and I gave her pure water. The result was the same, and I do not think she swallowed a drop of the fluid. She sank rapidly from five o'clock, and died rather suddenly at seven in a violent spasm, which seemed to suffocate her in an instant."

Her aunt gave her willing consent that the body should be carefully examined; and on Thursday evening, about twenty-four hours after death, Dr Aitken and Mr Macdowal gave me their valuable assistance in effecting a careful and minute examination. The following is Dr Aitken's report:—

*"Morbid Appearances in a Case of Hydrophobia."*

"June 11, 1852.—Along with Dr Lawrie, I examined this evening, at Renfrew, the body of a female, who had been dead about twenty-four hours, and who had every appearance of having been a well-formed healthy person, about nineteen years of age. The weather was moderately warm, and the body appeared to be more than usually livid, especially towards its posterior aspect; the eyeballs were somewhat sunk within the orbits, and no post-mortem rigidity existed in the limbs.

"The bites of the dog appeared as the cicatrices resulting from the indentation of three teeth; but they were so completely healed up and obscured by the general lividity of the parts, that their position required to be pointed out to us before we could observe them. When thus pointed out, however, the cicatrices were perfectly distinct, consisting of a glistening pellicle of skin, and not differing from the cicatrices of any incised wound. On dissecting the cicatrix cuticle, it was found very loosely attached to the areolar tissue below; but no change could be observed in the parts, nor any accumulation of fluid different from what could be pressed out of any other part of the areolar tissue of the body. The bites seemed to have been in-

flicted on the back, about two inches to the left side of the spines of the dorsal vertebræ, a little below and to the vertebral side of the inferior angle of the scapula.

“The contents of the cerebro-spinal cavity were next examined, but no unusual appearance was observed, if we except a slightly increased vascularity of the membranes surrounding the upper part of the spinal cord, chiefly in the dorsal region and vicinity of the brachial plexus of nerves. This vascularity was accompanied in some places with a slight extravasation of blood on the outer surface of the membrane, and this extravasation was more particularly evident about the middle of the cervical region of the cord.

“The encephalon having been removed, its nerve substance, more especially in the vicinity of the origin and course of the roots of the eighth pair of nerves, was carefully examined, both microscopically and otherwise, but no structural lesion could be observed in any part; on the contrary, the texture of the brain, spinal cord, and nerve, was firm, and the fluid contained in the cavities, both as regards quantity and quality, appeared to be natural.

“Turning to the thoracic and abdominal cavities, we found the lungs generally more vascular and more red than usual, but otherwise healthy. Heart also was healthy; and the blood, where found in quantity, was fluid. The papillæ on the back part of the tongue were much enlarged, and the whole of the pharynx, along with the epiglottis and larynx, as far down as the vocal cords, were much congested, and covered with a tenacious frothy mucus, tinged with blood. On removing this tenacious mucus from the cavity of the larynx, its surface was here and there observed to be dotted over with little swellings, about the size of pin heads, projecting from the surface. Examined microscopically, they seemed to consist of accumulations of little cells, varying in size from that of a human blood corpuscle to a pus corpuscle; all of them were more or less granular, and some of them nucleated.

“The stomach and intestinal canal were removed with their contents for chemical analysis and careful observation. The contents of the stomach consisted chiefly of a fluid, resembling in appearance coffee grounds; the granular and more solid particles appeared under the microscope to be made up of coagulated blood, more or less changed. Similar contents existed throughout the rest of the tube. The mucous membrane of the stomach presented a very much congested appearance, and more especially large patches near the cardia, where in many places streaks of blood could be seen extravasated on the mucous surface. Similar highly congested patches occurred in many places throughout the small intestine, with similar extravasated streaks of blood. These patches varied in size from three to four or even five inches in extent, and in the midst of them, both in the stomach and intestine, air was so abundantly extravasated as to inflate the submucous areolar tissue, and raise the membrane into the form of little air-vesicles, resembling in

appearance the texture of the lungs in emphysema. All other organs were healthy.

"(The cicatrices of the bite, as well as the tongue, pharynx, and larynx, are preserved in the Pathological Museum.)

" WILLIAM AITKEN, M.D.,

" Demonstrator of Anatomy, College, and

" Pathologist to the Royal Infirmary of Glasgow.

" Dr James A. Lawrie,

" Professor of Surgery."

To complete the narrative of this interesting case, I must turn as briefly as possible to its history previous to the 7th June. Mrs W., the aunt of the patient, had a large dog of the St Bernard's breed. By all accounts it was a remarkably fine animal, on which she set so high a value that she had refused L.20 for it. It was particularly attached to the unfortunate subject of this case, and seemed to take pride in being her attendant and protector. About the 1st of March it became suddenly ill, and a gamekeeper in the neighbourhood pronounced it to be affected with severe inflammation, the consequence of poison maliciously administered to it. He bled it, and directed it to be put into the coach-house, loose, and to be fed upon warm gruel. Immediately before going to bed on the night of the 1st March, Mrs W. placed a basin of gruel before the dog, and set another at the side of the fire to be ready for it in the morning. About six o'clock she called her niece, and asked her to see how "Neptune" was, and to take him his gruel. The poor girl got out of bed, threw a light shawl loosely over her shoulders, and did as her aunt had desired her. When she was leaving the coach-house the dog endeavoured to rush past her and escape, whereupon she closed the door, shutting herself in with her back towards the dog. He instantly sprang upon her, seized her by the left shoulder blade, sank his tusks into her back, and bit or scratched her on her face. Her aunt, when speaking of this attack, endeavoured to make light of it, but one of the servants assured me that the infuriated dog was "worrying" the poor girl, and that she might have been killed had some one not promptly come to her assistance. When the door was opened the dog made his escape, remained away the greater part of the day, and returned in the afternoon of his own accord. He was again shut up in the coach-house, and became quite furious,—tearing the door, and every piece of wood he could get hold of. He was so bad, that a veterinary surgeon who was asked to see him, was with difficulty permitted to open the door; and what food and medicine he got subsequently was pushed under it. He died the following morning. No suspicion was entertained of his being rabid. On the contrary, so firmly was Mrs W. persuaded that he had been poisoned, that she urged the public prosecutor to take up the case, and had the body sent to Dr Mackinlay in Paisley for examination.



This latter step was not taken till the 8th March, by which time the carcass was putrid; and having been previously cut up by a game-keeper, admitted of a very imperfect examination. The stomach was examined by Dr Mackinlay, jun., who kindly informs me that he could discover no poison. Mrs W. publicly offered a reward of L.5 to any one who would assist in discovering the person who administered the supposed poison. No informant appeared.

The above statement can hardly leave a doubt as to this animal having died furiously rabid.

*Remarks.*—The first question which suggests itself in this, and all similar cases, is, “Was this really hydrophobia?” Now, it does appear to me, that if we are to admit the existence of hydrophobia as a specific disease, we cannot refuse our assent to this being an example of it. The history of the dog, the history of the girl, the peculiarity of the symptoms, and their rapidly fatal termination, appear to me to be as clearly traced, and as well marked, as we could possibly desire. The diagnosis of hydrophobia ought not to be difficult. I have heard experienced surgeons assert, that it so much resembles tetanus that they are in reality the same disease. It has been my misfortune to see too many cases of tetanus, and only this one case of hydrophobia; but symptoms more dissimilar than those of traumatic tetanus, and the sufferings endured by this unfortunate girl, I have never witnessed. It much more resembles acute hysteria. The globus and incessant tossing were well marked; but although the desire to move was irresistible, the motions had no appearance of being involuntary, or associated with insensibility. The attempts at swallowing brought out symptoms such as I have never before seen, and which appeared to me quite diagnostic between this and any other form of disease. The constant discharge of froth from the mouth was also a very peculiar symptom. My first impression was, that the girl was poisoned. The same suspicion had been strongly entertained regarding the dog. I have only seen one case of pertinaciously concealed suicide from arsenic. In it the symptoms closely resembled those of cholera, and were mistaken for that disease. There was no difficulty of swallowing; the cramps were most overpowering; the vomiting and purging profuse; and the “natural characters” of the case very different from those of Miss M. Still I think hydrophobia deserves the careful study of the medical jurists.

*The prophylaxis* in this case was very imperfect. Mrs W. sucked the wounds on her niece's back with her mouth, carefully washed them and dressed them with simple domestic remedies. Nothing else was done, and they healed readily.

*Period of incubation* was about ninety-eight days, certainly beyond the average.

*Predisposition and Predisposing Causes.*—The girl's temperament was ardent and excitable, with a full share of self-will. In January she was engaged to be married to a youth, who took small-pox

and died. She persisted, in spite of all remonstrance, in attending upon him, and there is some reason to believe that she took the disease in a very modified form. In a few months after her first lover's death, she was engaged to be married to a second. Her aunt disapproved of the marriage, and for some time keenly opposed it, but was constrained to give her consent a short time before the symptoms of hydrophobia appeared. Powerful mental emotions very frequently precede the first threatenings of hydrophobia, and this case adds one to the number which make it probable that they may act as predisposing causes. Fear had no share in it. The girl had no suspicion of the nature of her illness, and no dread of dogs.

The stage of *recrudescence*, as it is somewhat barbarously called, was altogether absent in this case. She was not heard to complain of her shoulder or face, and Dr Aitken's accurate report shows that the cicatrices were perfectly normal.

*Morbid Anatomy and Pathology.*—The appearances discovered by Dr Aitken's careful examination confirm the opinion, that congestion and extravasation of blood connected with the spinal cord and mucous and sub-mucous tissues, with prominence of the mucous papillæ, constitute the principal morbid appearances hitherto discovered in hydrophobia. It seems highly probable that these are effects, not causes, of the disease, or if causes, are but fragments of the efficient cause. What that cause is, or how it acts, is still a mystery. That it is a morbid poison I have myself no doubt. That it may remain in the system for an indefinite period, and then be thrown off, or be brought into play by secondary causes, seems highly probable. I am not disposed to assign any limit to the period of incubation. The poison of syphilis may lurk in the system undeveloped for years; and why not that of hydrophobia?

*Treatment.*—The nature of the disease not being suspected until the symptoms were fully developed, the treatment was more directed to meet symptoms than to arrest so fatal a malady as hydrophobia. Still it was sufficiently active, and conducted on unobjectionable general principles. I was anxious to give sedatives and chloroform, especially the latter, a fair trial. Great caution is necessary in using anæsthetics in this disease. If carried so far as to prevent expectoration, the copious secretion of frothy saliva is almost certain to suffocate. As exhibited in this case, chloroform gave great relief; but as a curative it was powerless. I have tried it freely in tetanus with the same effect. It diminishes pain, but cannot cure; indeed, I fear its tendency is rather to shorten than prolong life. After my experience in this case, I should consider all attempts to treat hydrophobia by medicines given by the mouth as worse than useless. If we look upon the disease as a morbid poison, acting in an unknown manner on the nervous system, our indications will be to allay the consequent irritation, and fight against death with the hope that the poison may exhaust itself before life is extinguished. In this view strong soup, wine and brandy, given by the rectum, with free

doses of sedatives exhibited in the same manner, would constitute the most rational treatment. We must not despair of discovering some medicine (possibly some well known medicine applied in a novel manner, as ether by inhalation), which will prevent death until the disease exhaust itself. Perhaps anæsthetics, *early* begun, before the stage of acute spasm and exhaustion have set in, may be of use. All treatment, whose effect is to diminish strength, should be discarded.

*Frequency of the Disease.*—Until within the last three years, hydrophobia was certainly a rare disease in Scotland. In 1849 a case occurred at Dalkeith, and no fewer than three perfectly authenticated cases are recorded in the Monthly Journal as having occurred in the east of Scotland in 1850. When lecturing on this disease during last session, and adverting to these cases, I stated that, so far as I knew, no unequivocal case had been seen in Glasgow or its neighbourhood for at least a quarter of a century, but that, in all probability, twelve months would not pass and leave us in the same boasted immunity. It is remarkable, that about the very time that I made that statement the seeds of the disease were being implanted in our unfortunate patient; and it is not much less so, that I should have been called upon to assist in its treatment.

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ARTICLE II.—*Illustrations of Erysipelas.* By JAMES BEGBIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, etc.

(*Read to the Medico-Chirurgical Society of Edinburgh, July 7, 1852.*)

I. IN a remarkably well-aired and healthy situation in the New Town of Edinburgh, which it is unnecessary more fully to describe, there stands a double row of shops,—the upper being on a level with the foot-pavement in front, the under occupying the area or sunk floor of the building. These shops were chiefly tenanted, at the time to which this notice refers, by several thriving tradesmen, whose occupation consisted in the sale of a great variety of animal and vegetable substances as articles of food. The debris of the stock in trade of these different shopmen was too often left to accumulate in, or around, their several offices; and the atmosphere of the vicinity was, generally, so deeply tainted with the noxious effluvia, arising from putrid animal and vegetable substances, as to call for the exercise of some sanitary regulation, or, more properly, of some stringent police application to remove the offensive *materiel*, which, especially during the summer months, constituted a public nuisance.

In the winter of 1850–51, the floor immediately above this double tier of shops was occupied as a dwelling-house by a family from the north of Scotland, consisting of the head of the house—a landed proprietor, his wife, and two children of the ages of twelve and three

years, together with one male and three female servants. During the early part of the winter, and, indeed, throughout their occupation of the premises, the family suffered much from ill-health: the children became pallid and cachectic; the father dyspeptic and nervous to a high degree, while the mother suffered from severe headache, and almost constant diarrhoea. Accustomed, as they had been, to health promoted by a residence in one of the most salubrious counties in Scotland, the family had no difficulty in tracing to the impure and vitiated air, with which they were now surrounded, the origin of their different ailments; and had frequent occasion to complain of the direct effects of the noxious exhalations entering the windows, or penetrating the flooring of their dwelling. Towards the close of winter the father and mother, with the eldest boy, left home for a time, and escaped the contamination to which they had been exposed. Early in spring the youngest child had become more than usually ailing; and in the beginning of March the man-servant, generally remarkably healthy and robust, began to complain of more serious indisposition. After some days of excessive languor and sickness, he was seized with violent and oppressive headache, succeeded by the characteristic features of erysipelas, which, appearing first on the right ear, rapidly spread over the cheek and neck of the same side, and accompanied by much febrile disturbance, quickly diffused itself over the whole face and head. It was at this stage that I was requested to see him; and finding the pulse firm and frequent, the headache unabated, and considerable delirium and occasional somnolency prevailing, I ordered the abstraction of blood by leeches to the temples, small and frequently-repeated doses of saline diaphoretics, with antimonials, a mercurial purgative, and the regulation of the bowels afterwards, by rhubarb and the carbonate of potash. The tongue continued loaded with a creamy fur for many days; and the urine, scanty in quantity, deposited largely the lithates and lithic acid for a much longer time than I had anticipated. The convalescence was slow and unsatisfactory; and four or five weeks elapsed before the patient was able to resume his duties.

The dwelling-house in which I had witnessed so much sickness was now abandoned, and in the purer air, and more elevated situation of another street, the family by-and-by completely recovered from the injurious consequences of the foul air they had too long been permitted to breathe. It was not, however, before another of their number had passed through an attack similar to the last, that they were allowed to enjoy this immunity. The nurse, a fine healthy woman, approaching the age of fifty, was, within a few days after the removal to the new residence, seized with symptoms of acute illness, commencing with violent headache, flushed face, severe pain in the lumbar region, great febrile excitement, and high delirium. With these symptoms the erysipelatous rash appeared on the right ear, and quickly spread over that side of the face

in the course of the night. The aspect of the case, at this early stage, indicated a severe and lengthened illness; indeed, I do not remember to have seen, during a long practice, one which, from the constitutional disturbance and local appearances, threatened a more unfavourable issue. Having, at this period, listened to the remarks on the treatment of erysipelas by the muriated tincture of iron, communicated to this Society by Mr George Hamilton Bell, and his brother Dr Charles Bell, I felt disposed to give a trial to the remedy; but, seeing that the case was of a severe character, I hesitated placing reliance on iron alone, and directed the abstraction of  $\text{xxij}$ . of blood from the nape of the neck by cupping, and the administration of a full dose of castor-oil. These means being premised, and having observed that the urine passed in the course of the day—the second of the illness—was of a deep red colour, and scanty in quantity, that it was loaded with biliary matter, and presented under the microscope numerous blood corpuscles, and many crystals of the triple phosphate, I ordered the muriated tincture in the manner recommended by Mr Bell, in doses of twenty drops every two hours, continued through the night and day. At the end of twenty-four hours there was a marked remission in all the more prominent symptoms,—the erysipelas was arrested—the headache subdued—the delirium overcome—the pulse reduced in frequency and force—the skin covered with a gentle moisture, and bereft of its burning heat—the pain in the back removed, and the urine rendered more copious, and freed from much of the blood and bile of the previous day. The remedy was continued for twenty-four hours more; and without experiencing anything but continued improvement, at the end of the fourth day the patient was convalescent, presenting a striking contrast to the case of her fellow-servant, who, with symptoms of a less severe character, had his illness lengthened out to as many weeks. In both cases the throat was much affected from the commencement of the attack; but neither the pharynx nor larynx suffered in the manner which is sometimes observed in severe cases of erysipelas, from the infiltration of serum into the sub-mucous tissues.

It adds greatly to the interest and importance of the history now related to learn, that the wife of one of the shopmen referred to, who was in the daily habit of attending her husband's place of business, and assisting him in conducting it, was, during the progress of the first of these cases of erysipelas, carried home in the pains of childbirth, and that she died on the fourth day after delivery, with obscure indications of puerperal peritonitis, and rapid sinking.

II. The ancient village of the Water of Leith, well known to many of us as the seat of cholera, fever, and other epidemic diseases, was, at the commencement of last winter, visited by erysipelas. In the second week of November, and during the prevalence of cold and wet weather, four cases presented themselves nearly simultaneously, in



the narrow street called the Dam-side, lying between the river on the south and the steep bank on the north, at the foot of which runs the water of the mill-lead. Two of the cases lay in the same house, the sufferers being sisters; but, though affected at the same time, and now living under the same roof, they had, apparently, caught the disease from different sources, having resided apart from each other for some time—the one occupying their present dwelling, the other residing in the family of a gentleman in a distant part of the town, and having had no communication with her sister or her sister's household. The third case was that of a man,<sup>1</sup> living at the opposite end of the lane, and with whom no communication existed, either on the part of the sisters referred to, or the subject of the fourth case, to which I have now more particularly to refer:—

Mr B., a cabinetmaker, aged 31, of stout build, and full habit of body, long in the enjoyment of good health, and active and industrious as a workman, though not strictly of temperate habits, had been five days under the care of Dr Sibbald when I was called to see him early on the ninth day of the eruption of erysipelas. His face and head, at this time, were fearfully swollen, and his features distorted. He lay in a drowsy state, with muttering delirium, varied by fits of excitement, which required the assistance of his male friends to control. His pulse was rapid, firm, and full; his tongue dry and brown; his skin hot, and he passed his evacuations unconsciously in bed; his eyeballs were red and injected; and his face and forehead covered with vesications. The eruption first manifested itself on the left ear; and from the commencement the patient had complained much of sore throat. The stools were dark and offensive, and the urine scanty and high-coloured, containing lithic acid and a small quantity of blood. The case had resisted the treatment judiciously pursued by Dr Sibbald, consisting of purgatives, nauseating doses of antimonials, and saline diaphoretics.

Though it was late in the disease, yet the pulse was firm and full, and the symptoms generally not contra-indicating it, I recommended  $\text{ʒviiij.}$  of blood to be taken from the nape of the neck, after which the muriated tincture of iron was commenced, and pursued as in the former case. The following day found him decidedly improved: he spoke, and answered questions intelligently; his pulse had fallen in number and force; his skin was soft and moist; his tongue improved at the tip and edge; and there was a manifest diminution of the swelling of the face. The iron was continued till the close of the fourth day from the commencement of its use, in the doses in which it was begun, and afterwards administered at longer intervals. It produced no unpleasant effect, but, on the contrary, all the symptoms gradually abated under its use. The urine, particularly, became copious and healthy, and the skin soft and

<sup>1</sup> This patient was attended by Mr J. T. Alexander, who informs me that three other cases occurred, in succession, in the same household.



moist. Convalescence set in, and was only interrupted by the formation of matter, and a copious discharge from below the scalp, which continued for many days to weaken and annoy him.

It is worthy of remark, that a brother of this patient, residing in a distant part of the country, and with whom he had maintained no intercourse for years, was labouring under an attack of a similar kind, and at the same period, as that now related. It also deserves notice that, during this time, a female in his immediate neighbourhood, residing, indeed, in the same lane, died in childbed,—the cause of death being attributed to phlegmasia dolens.

III. The month of December 1851 was not particularly noted for the prevalence of erysipelas; and the southern districts of the city, so far as I could collect evidence, were not visited by any epidemic fever beyond what is usual at that season of the year: puerperal fever was certainly not observed. On the 31st day of the month I was requested, by a practitioner of high character and great experience, to visit with him a lady, aged 28, who had been confined, under his care, on the evening of the 24th, of her second child. The labour had been easy, and no manual interference had been required. Her recovery appeared, in all respects, to be going on well, till the 27th, when rigors took place; and from that time, until within a few hours of her death, at mid-day of the 31st, the chief symptoms were alternate chills and hot fits, with excessive perspiration, great mental anxiety, and continued watchfulness. The pulse ranged from 96 to 110: there was an abundant lacteal secretion; rather sparing lochia, of unhealthy character; but no abdominal swelling or pain. Until four hours of her death—the period when I was called to see her—no apprehension of danger was entertained. She had then, however, become rapidly worse; the countenance assumed a leaden colour, and the lips a purple hue; the pulse became feeble, rapid, and intermitting; she was bathed in profuse and sickly perspiration, and vomited much. She became delirious, had frequent twitchings of the muscles of the face, followed by convulsions of an epileptic character, and died comatose. The whole aspect and character of the case conveyed to my mind the impression, that our patient was labouring under oppressed brain, the consequence of a morbid poison having entered the circulation, such as is seen in obscure cases of puerperal fever or uterine phlebitis. My first inquiry was in regard to the prevalence of erysipelas in the neighbourhood, or the circumstance of the practitioner, in attendance on the delivery, having visited any case of the kind, or of puerperal fever, scarlatina, or other contagious disease. I received a satisfactory reply in the negative, and relinquished any investigation into the probable cause of the illness, when the evening of the day of the decease revealed a circumstance which threw light on its hidden nature. An urgent message, requesting the attendance of my professional friend on the father of the deceased, an elderly gentleman, living on the floor

below his daughter's house, was delivered at that time; and on visiting him he found that this patient, who had been under his care for upwards of a year with hemiplegia, was now suffering from erysipelas; and that the paralysed limb, from the ankle, on which was a large ulcer, to the groin, was covered with a dark-coloured rash. No distinct account could be obtained of the precise period when the erysipelas appeared; but redness was observed at least four days previously, and then, too, a shivering had taken place. This was, it will be observed, three days after his daughter's confinement, and on the same day on which she was seized with rigors. She had dressed the ulcer on the day she was taken in labour, as she had done daily for some time before. The father recovered from this attack. No farther light was thrown on the daughter's death by examination of the body. This is perhaps less to be regretted, seeing that numerous dissections, conducted by the most diligent and impartial inquirers, have done so little to elucidate the pathology of such diseases. We have, perhaps, been misled too much by our firm faith in the doctrines of solidism, and have held too lightly those of the humoral pathology. When patient microscopical examination, and chemical investigation, of the fluids shall have had a full trial, we may then be prepared with a rational explanation of the serous, and purulent, and sero-purulent effusions, found not only in the cavity of the abdomen, but in that of the thorax, in the ventricles of the brain, in the blood-vessels, and in the joints; and find that these, and such like appearances, are not the products of inflammation, commonly so called, but the results of chemical or vital changes in the blood itself.

The narrative now closed appears to me to possess some points of interest as regards the etiology of erysipelas, on which we may venture the following remarks:—

The first series of cases, those, namely, in the New Town, illustrate the connection of the disease with foul or vitiated air; and show how powerful must be the influence of this cause, even under circumstances calculated to deprive it somewhat of its virulence; for we cannot doubt that, had the materials which operated so effectively in a well-aired and healthy part of the New Town, been called into exercise in some of the more densely populated and confined districts of the older part of the city, a much more extensive and serious malaria must have resulted. This is a question of grave importance to all who are interested in sanitary regulations, and especially to those in our own profession. "Of idiopathic erysipelas," says the learned author of the "Inquiries on Constitutional Irritation," "the causes are referred to sudden changes of temperature, a humid soil and atmosphere,—of which particular conditions, at certain periods of the year, render the disease more frequent. Especially also, an unchanged, and, therefore, as well as from other causes, a polluted atmosphere; such as the ill-ventilated apartments of the poor, and even of many old parochial and eleemosynary insti-

tutions, manufactories in crowded cities, narrow alleys, and underground chambers, where running sores, and other uncleannesses, additionally contaminate the stagnant air." In the history before us we had not an unchanged and stagnant, and, therefore, polluted atmosphere; but we had a changing, and still polluted atmosphere: for no one who is acquainted with the locality in question can, for a moment, doubt that it possesses advantages in point of ventilation so enviable as only to demonstrate, to a proof, how great must have been the contamination they were called to overcome. It is a curious feature in the history of this polluted atmosphere, that it should so early, and so generally, have affected those who came under its baneful influence, fresh from the salubrious air of the country, and spared, apparently, the many who had long been accustomed to brave its dangers, till one striking example manifested its power in the trying and perilous hour of childbirth.

The second series of cases,—those in the Water of Leith,—illustrate the influence of atmospheric and terrestrial causes in the production of the disease; for it is, I apprehend, not through any contagious principle, that it affects so many at one time, among whom no intercourse can be traced. Among the five cases alluded to, there was one instance of two sisters suffering at the same time; and another of two brothers simultaneously labouring under the disease, while, in the former, communication by contagion was most improbable, and in the latter, impossible. We are, therefore, in such cases shut up to the conclusion, that an epidemic constitution is in operation, aided by predispositions derived both from personal and family taints.

I had lately an opportunity of seeing exemplified this marked family diathesis, in a case which I visited with Dr Burn. The patient was the mother of the family, at that time suffering from a severe attack of erysipelas of the head and face: it proved fatal by coma on the eleventh day. A daughter was at the same time recovering from the same disease, and some months afterwards had another attack, from which she recovered; and subsequently a sister was similarly affected. Still, it cannot be denied that erysipelas may be communicated by contagion; the experience of Drs Wells, Stevenson, Gibson, Mr Arnott, and others, appears to be conclusive on this point; and the examples furnished by Drs Watson and Elliotson admit of no other explanation.

The third history, taken in conjunction with the lying-in cases of the two former, appears to illustrate the association or connection of erysipelas with those obscure and rapidly fatal examples of uterine phlebitis, or puerperal fever, with which the records of midwifery abound. The first instance, that, namely, of the shopman's wife in the New Town, seems to illustrate the connection of such cases with one of the ordinary causes of erysipelas—the agency of foul or vitiated air, arising from animal and vegetable matter in a state of

decay or decomposition. The second, that of ~~the woman~~ in the Water of Leith, their occasional dependence on, or connection with, an epidemic constitution, arising from atmospheric causes. And the third, or last history given, may, perhaps, serve to illustrate the doctrine of contagion as the link connecting the two diseases. Many will at once account for the fatal illness in that case, by the contagious principle being carried directly from father to daughter; but others will doubt this ready explanation, and ascribe it to a cause acting simultaneously on both,—the puerperal condition of the one, and the ulcerated leg of the other, affording a predisposition for its operation. It is worthy of remark, that the rigors of both took place on the same day, and that the period which brought the erysipelas of the father to its full height, witnessed the fatal termination of the puerperal malady of the daughter. It would not be unphilosophical, I apprehend, to conclude, that a common poison deteriorated the blood of both; and that it exerted its blighting influence on each, irrespective of any direct transmission from one to the other. This view, of course, leads to the conclusion, that the poison was absorbed during the last weeks of pregnancy, and that its operation was not confined to the period of delivery. Denman has remarked, that there are not wanting instances in which puerperal fever has been evidently forming before delivery, or during labour. Joseph Clark informs us, that he saw reason to date the commencement of several cases from before delivery, and refers to two cases in which the speed of termination of life after labour, and the appearances on dissection, evidently led to this conclusion; and Hey refers to two cases in which evident symptoms appeared previous to delivery, one fatal.<sup>1</sup> Such a view, if found to be correct, would tend to modify the notion so generally entertained, that the cases of puerperal fever occurring in succession to the same practitioner, are instances of direct inoculation,—the morbid matter being brought into contact by the hand of the accoucheur, with the uterine fluids of the mother. In the case before us, had the attending practitioner dressed the ulcerated leg of the father before being called to the bedside of the daughter, no question would ever have arisen in regard to the communication of the fatal illness. That the two patients imbibed the morbid poison simultaneously, or nearly at the same time, and from the same source, is an impression which it would be difficult to remove from my mind.

Before concluding, I beg to offer a few remarks in regard to treatment.

The remedies for erysipelas are as various as the forms and features of the disease; and the judicious and skilful physician will select those which occur to him as best suited to the prevailing epidemic character, and the habit and constitution of his patient. The disease, at one time, partakes of a sthenic, at another, of an

<sup>1</sup> See Craigie's Practice of Physic, vol. ii. p. 231.

asthenic character: it presents itself in one form among the luxurious and over-fed of the upper classes, and in another, among the half-starved inhabitants of the lanes and closes of large towns. Bleeding may be extolled by one class of practitioners; and wine and bark by another, and both on perfectly legitimate grounds; but no one remedy, or set of remedies, will be found equally available at all times, and in all places. The disease, in many instances, will run its course; and the office of the physician should be, not so much to cut it short, as to guide and regulate nature in conducting it to a favourable termination.

Erysipelas is a blood disease: and, though generally considered a specific inflammation, is not to be cured by antiphlogistic means. Bloodletting is, no doubt, frequently admissible, and often beneficial. It is not, however, I apprehend, so much by diminishing the force and frequency of the circulation, as by relieving the system, and particularly the brain and nervous system, from the full weight of the poison which has contaminated the blood, and thus restoring the powers of life, and enabling the depurating organs and emunctories to act and eliminate the offending matter, that it produces its beneficial effects. It is in this manner, I believe, that bloodletting is found useful in other cases of morbid poison,—as, for instance, in small-pox, in typhus, and in acute rheumatism,—in all of which we must have noticed many examples of the remarkable effects of a well-timed and judicious loss of blood, in facilitating and promoting the action of other remedies, and so conducting the disease to a happy issue. The case of Mr B., just related, shows that, even at an advanced stage of the disease, bloodletting may be employed with advantage.

Emetics are often useful at the onset of the disease; purgatives throughout its course. In the early stage, mercurial and saline laxatives are required by the state of the stomach and liver; and in the more advanced, the oppressed circulation in the head demands their employment, especially when the tendency to coma cannot be obviated by bloodletting. Both remedies, however, require to be used with caution and judgment. “The truth is,” says Mr Travers, “that as hypercatharsis of a very short duration, or the loss of a few ounces of blood, has often proved fatal in an erysipelas; and the disease is not unfrequently terminated on a sudden, or the patient narrowly saved from sinking by such volatiles and cordials as could be got down.” Such an experience as this is well calculated to recommend and enforce the practice of treating the disease by wine, and brandy, ammonia, and bark, and other stimulants, a plan which has its advocates among the best observers and most skilful physicians of the day. Dr Williams gives wine in *all* cases, from the very beginning. Many trust entirely to quinine, and uphold the reputation which bark acquired under the prescription of the Fordyces, and Heberdens, and Wells’ of a former day. What can be more satisfactory, for instance, than the following case,



given by the last-named writer, who is justly entitled to the credit of being one of the most original observers of disease in this country?—"Mrs Hunter, a large woman, of a florid complexion, with a constitution vigorous from nature, and unbroken by intemperance or disease. Though fifty-one years of age, she menstruated as regularly, with respect both to periods and quantity, as at any former time of her life. In short, she was altogether such a person as would have been deemed fit to bear large evacuations by those who attempt to cure erysipelas through means of them. I saw her a few hours after her face began to swell, and immediately prescribed for her the Peruvian bark. No other means of relief in the way of medicine were employed throughout the disease, except that from the second day linen rags, moistened with brandy, were kept constantly applied to the parts affected. Under this treatment, the disease gradually subsided, and on the fourth day it entirely disappeared."<sup>1</sup>

It is not my intention, however, to discuss the merits of opposite modes of cure, or to enlarge on the circumstances which ought to give a preference to one above another. I shall limit these remarks to a notice of the remedy which has recently been recommended to the profession by Mr Bell. Besides the two cases related in the foregoing histories, I have employed the muriated tincture of iron on several other occasions; and, in all, with marked benefit and success. In the case of a lady, whom I saw lately with Dr Taylor, it was administered on the fourth or fifth day, when the swelling of the face was great, the fever high, the tongue loaded, the urine scanty and high-coloured, together with much restless anxiety, severe headache, and tendency to somnolence. Its exhibition was quickly accompanied by a remission of all the symptoms, particularly by diminution of headache, by gentle perspiration, increased flow of urine, and reduction of fever. On the occasion of a relapse, the patient was so satisfied with the benefit derived from the remedy, that she begged that the administration of it might be resumed. So, in the case of another lady, seen within the last few weeks, were its curative powers so manifest to herself, that, though in considerable danger from two previous attacks of the disease, she hesitated not to have recourse to the remedy before summoning her medical attendant. So, also, in the case of an elderly lady, recently seen with Mr Syme, on the occasion of the thirty-first attack of erysipelas in the face, was the exhibition of the iron, for the first time, so marked, that she intimated her fixed resolution to resume its use on the occurrence of any future attack of her persevering enemy, in preference to any plan from which a long and painful experience had taught her to expect benefit. The last case in which I have witnessed its efficacy was that of a female servant, under the care of my son.

<sup>1</sup> Transactions of a Society for the improvement of Medical and Chirurgical Knowledge, vol. ii. p. 226.



She was a woman of stout habit of body, and healthy appearance, of the age of forty-five, who, after exposure to cold and much fatigue, had her second attack of erysipelas in the head and face, accompanied by severe headache, and great swelling and redness, preceded by rigors, and followed by high fever, hot dry skin, foul tongue, and quick pulse, and altogether presented the appearance of the commencement of a severe attack. She was seen on the second day of her illness, and, after the exhibition of an active purgative, was placed under the muriated tincture of iron, in doses of twenty drops every third hour. On the following day she was considerably relieved, the fever diminished, and the swelling reduced. She continued the iron, without any other remedy, for two more days, when the swelling subsided and convalescence commenced. As in other cases, the patient expressed herself sensible of the benefit derived from the remedy, and contrasted favourably the plan of treatment with that adopted on a former occasion. It appeared to act, as in other examples, particularly on the skin and on the kidney.

The employment of the muriated tincture of iron is not contraindicated by the presence of fever, or by symptoms of derangement of the digestive functions. I have seen it useful in scarlatina, when the skin was hot and dry, the pulse rapid, and the urine scanty and bloody. The effect appeared to be to lower the pulse, to relax the skin, and to increase the urinary discharge, while it diminished the blood from the kidney. In chronic hematuria, I have noticed the same effects from the medicine; and in one instance particularly, where well-marked anæmia had resulted from the long continuance of the hemorrhage, the persistent use of the tincture arrested gradually the discharge of blood, restored colour to the exsanguined countenance, and, for the first time for many months, brought about a soft and perspirable state of skin.

The state of the urinary secretion, during the febrile stage of erysipelas, still requires patient investigation. Many have remarked that this secretion, as well as that of the bile, are highly morbid; and that their improvement in this respect is sometimes critical of the disease; but there are still wanting carefully instituted experiments in regard to the daily characters of the urine in this, as in many other diseases. Becquerel has made an examination of the secretion passed in the morning, in five cases of erysipelas, from which it appears, that it invariably presented the characters of febrile urine, very well marked, being diminished in quantity, of a very rich colour, and high density. In four of the five cases, there were almost constantly deposits of lithic acid, and these frequently of a red colour. In two of the five a small quantity of albumen was detected. I have lately examined, from time to time, the urine of six patients labouring under severe erysipelas of the face and head, and have found a general correspondence in the results with those quoted; particularly as regards quantity, density, and colour. In five of the

six, the lithates and lithic acid were largely deposited; in one, and that the most severe, the triple phosphate was discovered in large numbers. In this last, also, numerous blood corpuscles were found; and in two others they presented themselves in a less copious form. In one other case albumen was present; in two, neither blood nor albumen was found. These six cases were all treated with the muriated tincture of iron, and the quality of the urine was tested, in each, before the remedy was administered. The blood and albumen sensibly diminished, from day to day, as the remedy was employed; and the quantity of urine, as well as of perspiration, were invariably increased under its use. In eleven cases, then, of erysipelas, in which the urine has been tested at the height of the disease, we find three in which blood corpuscles were seen, and three in which albumen was present; and, taking these in connection with results obtained during the desquamative stage,<sup>1</sup> we may conclude that a very considerable amount of renal disorder exists during the progress of this affection. My son, however, informs me that, in the course of numerous examinations performed by him in the Infirmary and elsewhere, it was not common to find either blood or albumen in the urine during the febrile stage of the disease.

It may be premature to hazard a conjecture in regard to the operation of iron in erysipelas. Many may be disposed to consider its efficacy as dependent on its general tonic qualities; and its mode of operation as identical with that of quinine or wine. Others may regard its remedial powers as connected with its diuretic, or diaphoretic properties, and particularly its influence in promoting elimination. Some, having respect to the congested state of the kidney, or the hemorrhagic condition of the vascular system, may attribute its beneficial effects to its action directly on the renal apparatus, or more indirectly through the general circulation. Many will call to mind its long-maintained specific properties in almost all urinary affections; and none should forget its wonderful efficacy in restoring the red particles of impoverished blood. Whatever may be its *modus operandi*, it may be well to cherish a hope that, since it has proved useful in erysipelas itself, it may not be found unavailing as a resource in those awful disorders, hitherto irremediable, with which that disease is, in some mysterious manner, so intimately associated.

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ARTICLE III.—*Remarks on the Yellow Fever which appeared of late years on the Coast of Brazil.* By WILLIAM M'KINLAY, M.D., late Surgeon of H.M.S. "Cormorant," employed on that Coast.

THE science of statistics has made very little progress hitherto in Brazil. All the statements of the amount of population in different

<sup>1</sup> See Monthly Journal of Medical Science for July 1852, p. 92.





places are mere estimates, made by different people, which often vary much; for instance, one estimate gives the capital of the empire a population of 300,000—another, of 140,000 only. And I am not aware that there are any regular series of meteorological observations kept at any part of the whole coast.

Brazil comprehends the north-east portion of South America, extending from  $5^{\circ}$  north to  $32^{\circ} 30'$  south latitude, and from  $35^{\circ}$  to  $70^{\circ}$  west longitude, with a coast-line of probably 4000 miles, and an area of between two and a half and three millions of square miles. It is open to the southern trade-wind, which, however, is rarely felt near the coast, where the winds are very variable. The land and sea breezes are generally felt in the harbours, and near the shore; but they are of variable degrees of intensity. Violent gales are seldom experienced, except in the most southern provinces; and the rainy seasons are equally irregular. North from Bahia,  $13^{\circ}$  south, there may be said to be only two seasons,—a wet and a dry one; the former commencing in January and ending in June. The amount of rain which falls in the course of the year, however, is not considered very large; and the thermometer does not vary much either in the course of the day or year, ranging generally from  $76^{\circ}$  to  $86^{\circ}$ . Thunder-storms, with much lightning, are frequent.

From Bahia to St Catherine's,  $28^{\circ}$  south, they can only be said to have two seasons also,—a hot and a cool one; the two hottest months being December and January, and the coldest July and August; the intermediate periods verging gradually into one or other of these,—the average temperature of the two former being  $82^{\circ}$  and of the latter  $70^{\circ}$ , with a diurnal difference of about  $4^{\circ}$ , which varies little throughout the year. The winds are very irregular, with the exception of the land and sea breezes. There cannot be said to be regular rainy seasons; but rainy days are more frequent in January, December, and June, in perhaps the order mentioned, than at other times; it is rare, however, to have four or five wet days in succession, and the amount in the course of the year is inconsiderable, much less than on the northern part of the coast. Thunder-storms, with much rain and lightning, were said to be more frequent until the year 1846 than they have been since. I may state from my own observation, that in the hot months of 1839, 1840, and 1841, "sheet" lightning, as it is called, to distinguish it from "forked," was much more frequently observed playing about the clouds, which appeared in the shape of light cumuli, over the land in the evening and early part of the night. These appearances were not accompanied by thunder, nor followed by rain.

From St Catherine's to the southern limit of Brazil, the climate is more fluctuating and occasionally tempestuous. Snow has never been known to fall in these provinces. Wheat and potatoes grow well in Rio Grande, the southernmost province of Brazil; and excellent horses are reared there. The distinction between summer

and winter is clearly marked, the medium heat of the latter being about  $65^{\circ}$  at sea, sea-water being of the same temperature. In summer and early autumn the heat in the middle of the day is often great and oppressive, average  $75^{\circ}$ , with a dry parching atmosphere, causing the lips to chap, and the face to become frequently much sunburnt. The daily difference of temperature is generally considerable, often  $15^{\circ}$ . The climate of spring and late autumn is very variable, and liable to sudden changes and storms.

The coast between Rio de Janeiro,  $22^{\circ} 53'$  S., and Santos,  $24^{\circ}$  S., is bold, high, and rocky, displaying in many places most beautiful scenery; and, as you proceed to the southward, it becomes gradually lower, until, in the most southern province, no mountain intervenes between the sea and the immense range of the Cordilleras. Advancing to the northwards, also, the coast becomes gradually lower, as far as Bahia; and thence, to the northern limit, it is low and flat, having few harbours except such as are formed by the mouths of rivers, where there are very generally, along the whole coast, marshy or wet alluvial plains of more or less extent, and thickly covered with the vegetation usual in such places in the tropics. The extent of this marshy ground is generally in proportion to the proximity of the mountains; the more distant the mountains, the more extensive it is. The north coast, from Cape St Roque westwards, is low and marshy.

The distance of the mountains from the sea varies from 0 to 180 miles. The highest of the coast range, about 2000 feet, and those which approach nearest to the sea, are between Rio and Santos. They are of granitic formation chiefly, and partly of gneiss.

Wood does not abound in the southern provinces, where there are immense pasture-fields, with large herds of cattle almost wild, but, advancing northwards, the country becomes thickly wooded. Forests, almost impenetrable, of very great extent, are everywhere to be found, rarely intersected by roads, of a poor description, to the more important of the inland districts, such as that from Rio to the mines, in the province of Minas Geraes. The immense plain on both sides of the Amazon, consisting of the richest alluvial soil, is often inundated to a great extent, and agues are rather common there; and even at Bahia, a general description, referring to the district at the head of the bay, says, "The perpetual moisture, caused by the dense vegetation on a wet soil, occasions aguish diseases."

The coast of Brazil, until lately, has been considered extremely healthy, and possessing a remarkable immunity from tropical and other fevers, and indeed from all epidemic and most endemic diseases. Ague has certainly been endemic in particular localities—the marshy mouths of rivers for instance; but it never prevailed extensively, nor proved very fatal. Epidemic cholera has never reached this part of the world. In her Majesty's ships, employed



on this coast during the last twenty years, ulcers, visceral derangements, and catarrhal complaints have been the most frequent diseases, and the standard of health in them has been high. Taking for our guide the statistical reports on the health of the navy, for fourteen years, from 1830 to 1843, both inclusive, the following comparison may be made, with the Mediterranean command:—

	Number of Sick per 1000 of Mean Strength. Annual Ratio.	Number sent to Hospital. Ratio as above.	Invalided. Ratio as above.	Died. Ratio as above.
South America,....	1321·	13·	27·3	9·4
Mediterranean,.....	1350·1	52·4	26·1	12·7

The great difference in the number of hospital cases here shown is easily accounted for, by the want of hospital accommodation for the navy in Brazil.

Brazil has often been referred to as a country where all the supposed local causes of yellow fever existed, without producing the disease. It is still doubtful whether yellow fever ever prevailed in Brazil before the end of 1849. Many statements, both verbal and written, have been made on this subject, but all the references are unsatisfactory. The years 1686, 1723, 1808, and 1809, have been referred to by De la Rosa, Labat, Santos, and others. Southey, in his "History of Brazil," 2d Part, quoting the first of the above authors, speaks of a pestilence which, in 1686, began at the Recife, soon extended to Olinda (the one the new town, the other the old town of Pernambuco), and arrived at Bahia as soon as the news of the disease. It proved very fatal; not a house escaped; in some houses not an individual. It was neither so general nor so fatal in the country. It affected the white part of the population exclusively, more especially the sailors. Europeans, if not acclimated, suffered much, after it ceased among the residents. Strangers were attacked, and died; and the disease was most fatal to the robust.

I think it may be allowed, considering that the symptoms are not described, and the time and place referred to, that this resembles very much a description of yellow fever. Santos, a physician of Rio, says that, in 1808-9, a fever prevailed in that city, attended with yellow skin and vomiting. It was not, however, by any means a fatal disease; and it may be remarked that yellow skins are always prevalent in Brazil, whether accompanied by fever or not. Dr Lallemand says that "he has no doubt in his own mind that yellow fever prevailed at Rio forty years ago."

Dr Fairbanks, a physician in practice at Bahia since 1823, in an excellent manuscript report on the late epidemic, says, "Although

fevers, attended with black vomit, have very rarely been witnessed in Bahia, yet that they have occurred is undeniable." "In some notes of the diseases of Bahia, furnished me years ago by a distinguished physician of Bahia, the late Dr Jose L. Coutinho, he expressly says, 'This disease, the *Malina*, sometimes takes on the gangrenous form, accompanied by black vomit and other symptoms observed in the worst fevers of the Antilles.'" "Dr Sauto, in his 'Memoir on the Yellow Fever of Bahia,' lately published, states that he attended a patient, in 1848, who died, after a few days' illness, with black vomit. In the month of June last (1849), I received into my hospital a woman from the vicinity of the city, and who died on the 7th, after having vomited a large quantity of dark matters like those of the epidemic. A case occurred also to Dr Magalhaes in the same month, and another to Dr Monteiro, both intelligent physicians of this city. They both terminated fatally in a few days." "On my first arrival in this city in 1823, I found the master of an American vessel, who had left Pernambuco before me in perfect health, dying from fever; and distinctly recollect the surgeon of the Tartar frigate, who attended him with the then surgeon of the British Hospital, Mr Dundas, saying, that had the case occurred in the West Indies, he should have considered it decidedly one of yellow fever. I do not now recollect whether he had or not black vomit."

A consular despatch from Pernambuco says that yellow fever epidemics had prevailed there in 1640, 1710, and 1780; another period of seventy years would bring us to 1850.

During the four years preceding the outbreak of this epidemic of yellow fever, in the end of 1849, a fever was prevalent on the coast of Brazil, in the hot months, which was generally called the "Polka Fever," from having made its appearance there about the same time as the dance of that name. This disease resembled precisely a very slight attack of yellow fever; and during these years, according to a statement of Dr Ellis, who has been surgeon of H.M.S. "Crescent" (a dismantled frigate constantly at anchor in Rio harbour) since April 1846, who is well acquainted with the yellow fever of the West Indies, his opportunities of observation being ample, and his talents unquestionable, this "Polka Fever" had been yearly becoming more severe, until, in 1849, a fatal yellow fever was developed.

We are told by Dr Lallemand, a German in practice at Rio for the last fourteen years, and whose report on this epidemic is very valuable, that he has been reminded of a letter of his, written in 1847, in which he predicted that this Polka, or "Insolation Fever," as he calls it, "might easily develop itself into a perfect yellow fever epidemic, if the circumstances to which the insolation fever are ascribed were to assume a more important character."

Dr Fairbanks, referring to the Polka in 1848-9, the similarity of its symptoms to those of yellow fever, and to its being a forerunner

of that disease, says, "The French Corvette, 'Expeditive,' was attacked in March 1849; but, by removing the sick promptly on shore, the disease was checked in a few days. Fourteen men were sent to my hospital on the 3d, and six more, including the commander, between that and the 10th of the month."

For several years antecedent to 1849, a remarkable absence of thunder-storms had been observed by many, both medical men and others, on the Brazilian coast. There had been also a less abundant fall of rain in that year; more than double the usual number of cattle died on their way to Bahia from the interior for want of water. In Rio Grande also there was a great want of rain.

These thunder-storms, and the heavy rains which followed them, were generally looked upon as salutary, as they purified the atmosphere, and cleaned the streets. It was often remarked, also, that, in the summer of 1849, the land and sea breezes were very irregular, often absent, and, altogether, that the weather was unusually hot.

It would be idle at this time to prove that the late epidemic was one of yellow fever. Many remained for a long time incredulous on the subject in 1850, so unexpected was the disease; but it returned again early in the year 1851, and by the mortality it caused, left no doubt in the mind of any one as to its power or existence.

There is as little doubt that Bahia was the first place attacked on the coast. It is situated in 13° south, and is said to have a population of about 140,000. Nearly the half of this number are negroes, the finest and stoutest I have ever seen; and the rest of the population may be divided into one-half white, of whom many are Europeans, and the other half are more or less coloured. This city was formerly the capital of the empire, and is often called by the Brazilians "St Salvador," and so marked on many of our maps. It is situated on the inner side of the point of land which forms the north-east side of the northern entrance into the Bay of All Saints (Bahia des Todos os Santos). This bay is about thirty miles by twenty-six, and the entrance is divided into two by the island Itaparica, twenty-three miles long by eight broad. The lower town, where all the shipping trade is carried on, consists chiefly of old buildings. The streets are narrow and dirty, with open gutters in a bad state of repair. The ascent to the upper town is very steep. Sedan chairs, or caderas, are generally used by Europeans for ascending this hill; the upper town is about 200 feet above the sea level, clean, and airy. Almost all the Europeans reside here, their places of business being in the lower town.

The ground on which the upper town is built is of a remarkable character. The surface is very uneven, consisting of many little elevations near each other, something of the shape of low mole hills, but of course much larger: these elevations, even to the top, as well as the little intervening valleys, being very fertile, and also dry, unless immediately after rain. It is only justice to say, that the present President of Bahia has made great improvements in the

paving, sewerage, and general cleanliness of the place; and what is more to his credit, his exertions did not cease on the disappearance of the epidemic which originally prompted them. The immediate neighbourhood of Bahia is quite free from marsh; but at the head of the bay, where several rivers fall into it, there are several wet alluvial plains covered with dense vegetation.

With reference to the climate and sanitary state of Bahia, Dr Fairbanks has the following remarks:—"The peculiar state of the atmosphere at the time of the appearance of the fever; frequent rains, not copious but drizzling, accompanied by a close and oppressive state of the atmosphere; heavy thunderstorms, one particularly on the afternoon of the 15th November; copious rains in the interior of the country, causing the numerous rivers which flow into the bay to overflow, one of them having a course of more than 100 leagues, bringing down from a great expanse of country, and after a drought of three years, an immense quantity of stagnant water, vegetable and animal remains, etc., which tinged the waters of the bay of a dark colour, almost like ink, and causing them to emit a most disagreeable odour, so much so as to render bathing in them unpleasant, and obliging many to forego this salutary custom. To appreciate fully this cause, it is necessary to remark, that in this part of Brazil there are two rainy seasons, one limited to an extent of perhaps thirty leagues from the coast, and lasting from April to July; and the other, which only affects the interior, commences in October, and lasts till December or January." Dr Fairbanks then remarks on the want of cleanliness, good sewerage, etc., and on the pernicious custom of burying the dead in churches. "At the same time, an old burying-ground was re-opened for the purpose of removing the bones, and forming a public promenade; and, to add to all this, a large quantity of putrid salt beef, more than thirty barrels, was, in the last days of October, opened and thrown into the sea from the Custom-house wharf. This beef, carried by the current to the shore, floated about until finally decomposed and washed away."

The weather at Bahia is generally not so hot as at Rio. Let it be remembered that Rio is very near the tropic, and I think all who have been much in such places will acknowledge that they are hotter, taking the average annual temperature, than places near the equator. This is easily explained. Looking at the globe, and using common language, the sun travels much more slowly as it approaches to the tropics than it does nearer the equator, and it will be evident that at all places between  $20^{\circ}$  and  $23\frac{1}{2}^{\circ}$  of latitude the solar rays must fall during two whole months either perpendicularly or in a line which deviates from it by  $3\frac{1}{2}^{\circ}$  at most at noon.

At Bahia also the sea breezes set in earlier and stronger; they are also more regular, and the upper town at all events is better exposed to these breezes than Rio. The trees bear blossoms and fruits all the year round, and the vine is said to bear three times in the year.

Bahia was remarkably healthy for some months prior to the appearance there of yellow fever. The first case of which we had any notice for some time was seen by Dr J. Paterson on the 3d of November 1849. A Brazilian boy, who had been ill for some days, on the afternoon of the 3d of November had black vomit, and died within a few hours. It is now generally acknowledged that several cases occurred before this one. Among others, one seen by Dr Fairbanks on the 2d November; but this case of Dr Paterson's is noticed here, because an effort was made to connect it, the supposed first case in Bahia, with a vessel called the "Brazil," which was supposed to have imported the disease.

To the history of this vessel we shall have occasion to return again.

Dr Fairbanks says:—"The first case of the fever to which I was called, was a student in the upper city, and not likely to have had any communication with persons belonging to the 'Brazil.' This was on the 2d November." The second was on the 12th, "in a child also in the upper city." "The vessels in port were at the time unusually healthy, as I learned from repeated inquiry, being surprised that no cases had occurred among them, while so many of the inhabitants of the city were laid up with the disease." "The first vessel attacked in the port was the Swedish brig 'Scandia,' which had arrived from Lisbon on the 5th October." Dr Fairbanks was called to her on the 28th November, and was informed "That a boy belonging to the vessel had died on the preceding Saturday (24th) of black vomit, having been removed to the Misericordia Hospital." The master and nearly all the crew died. "Of the whole crew only one man escaped."

We may state that medical men and the people generally were unprepared for the disease, and that the former naturally hesitated before they announced the presence of a fatal epidemic, until the disease had decidedly declared itself. After the first week in November the disease extended rapidly over the town and suburbs, and by the end of February 1850, it is said that 96 per cent. of the population of Bahia had the disease. The first fatal case afloat died on the 18th of November. The first case in the British shipping occurred on the 3d of December 1849, and the last case on the 23d August 1850; and between these dates 535 attacks and 156 deaths occurred in British ships alone. The British subjects residing on shore in the *province* of Bahia numbered from 180 to 200, including children; of these five died. Swedes, Portuguese, and Sardinians suffered very severely; 112 Swedes died, and the Sardinians, out of 487 persons at Bahia, lost 93 by death.

The disease had declined considerably by the first week of July,—that is, there were fewer attacks, but the proportional mortality had not diminished. Out of fourteen cases at that time six died; there were three or four cases in September, and the last fatal case died on the 4th of that month.

Suppression of urine was the most fatal symptom, and adolescence the most fatal age. The greatest distance to which the disease extended inland was to the district of Chapada, which is rather elevated, and 180 miles from the sea, and only a few cases occurred there.

Ships with coals on board for a cargo were said to have suffered more than others, while vessels with a cargo of salt escaped the disease as long as the salt remained on board; afterwards they suffered like the others.

Victoria, the most airy and the cleanest part of Bahia, where nearly all the English merchants, and most of the other wealthy inhabitants of the place reside, where almost every house is detached, and surrounded by its garden or shrubbery, did not escape the disease, although the lower town undoubtedly suffered most severely.

It is supposed that 7000 persons died at Bahia of yellow fever during this epidemic. The authorities there granted clean bills of health until 29th of January 1850.

Many of these statements are from documents which I had access to at the British Consulate at Bahia, through the kind intervention of Captain Schomberg, of H.M.S. "Cormorant."

The following communication appeared in the "Times" of February 14th, 1850:—

"Bahia, December 18th, 1849.

"The Committee of the British Hospital have just received the enclosed communication from Drs Alexander and John Paterson, respecting the very serious nature of the fever now raging; and they have strongly to recommend to consignees of vessels, the necessity of following out the suggestions therein contained. In the meantime, the Committee concur in the propriety of not sending any more sailors from the shipping to the hospital.

"PETER SIMPSON.  
"JOHN ELLIOTT."

"British Hospital, Bahia, Dec. 18th, 1849.

"Gentlemen,—The yellow fever at present prevailing in the shipping has, during the last few days, developed itself in this hospital in a form so pestilential, that the number of deaths now all but equal the number of entries.

"The mortality has not existed in the same fearful proportion in those cases treated on board; for of twenty-six such cases (the whole number), two only, up to this date, have terminated fatally; whereas of the twenty-six cases received into the hospital since the 3d of this month, six only have been discharged cured, four of them being the first four received. Of those still in the hospital, four only at the most will recover, giving of recoveries, ten; deaths, sixteen. Along with the daily increasing rate of mortality, the disease has also been assuming a more malignant form, so that entry one day, death the next, has become the rule.

"We can account for this tremendous and daily increasing rate of



mortality, and the accompanying malignity of the symptoms, only on the supposition that the hospital itself has become saturated with the poison; and we are of opinion that the further reception of men into the hospital is equivalent to a sentence of death passed upon them.

"We can propose but one remedy, that the shipping be scattered as wide apart as possible—a measure the President has permitted to be adopted; and that every vessel be converted into its own hospital, the sick being all treated on board.

"We, on our part, guarantee that nothing shall be wanting, and no amount of labour spared, to render the medical attendance on the sick as efficacious as possible.

"Begging that those measures you may consider necessary be at once adopted, we are, etc.

"ALEXANDER PATERSON.

"JOHN PATERSON."

"Bahia, December 18th, 1849.

"The Committee of the British Burying-ground beg to inform the foreigners resident in this city, that, owing to the unexampled mortality within the last month—causing the number of interments to amount to thirty (one-half of whom have been foreigners); and the limited space of the ground being now nearly filled up, they are compelled by necessity, and most reluctantly, to withhold any further orders for interment, except to British subjects. They give this notice to the foreign merchants, in order that they may take what steps they may consider necessary under the existing emergency.

"PETER SIMPSON.

"JOHN ELLIOTT."

When at Bahia, Dr Fairbanks kindly took me to visit the institution called the "Leper Hospital," at a little distance from the city, where many people, male and female, old and young, white, black, and coloured, when disabled by a certain disease, are maintained comfortably for the rest of their lives. Visiting this place in the middle of the day, when such of the inmates as are not confined to bed are walking about outside, and congregated about the doors, the first sensation excited in the stranger is generally a ludicrous one, at the sight of so many people, perhaps four or five in succession, and apparently in good health, without a single finger or toe. The disease which produces this is a curious one; the phalanges of the fingers and toes drop off one by one by a kind of process like dry gangrene, the system making an effort more or less perfect to cover the exposed end of the remaining phalanx, until all the phalanges have disappeared. The disease often then becomes to all appearance stationary for a longer or shorter period, the soft parts heal up and cover the bones, and the patients can walk about without any covering for the feet, and without pain. Sometimes this takes place before all the joints have dropped off. The disease during all this

time does not appear to affect the constitution. After a time, however, puffy enlargements, without redness or pain, of the alae of the nose, the lobes of the ears, and the eyebrows, begin to be observed and increase; the lips and eyelids become affected. The disease extends also to the feet, generally on the plantar aspect, where slow, sloughing, deep ulcers, with little discharge, gradually form and deepen, until they open the tarsal or metatarsal joints. The general health now suffers, and in various ways; often the stomach is the first organ affected, the appetite gets bad, and frequently the sufferer is carried off by colliquative diarrhoea. All these changes take place very slowly. Some have been in the establishment fifteen and twenty years; and we saw two or three in the last stage of existence, without any fever, and all the important organs of the system to all appearance sound; but the poor sufferers were very feeble, and emaciated to the last degree: and we were told that even this last stage of the disease is often much protracted. The disease is not considered to be either hereditary or contagious; no effort is made to cure it, as it is looked upon as incurable. It attacks people of all colours. Mulattoes, and next to them blacks, were the most numerous in the establishment. There were several natives of Portugal in it. The ages of the inmates varied from fifteen to seventy; from twenty to forty were the most numerous, and there were more males than females. Many of the sufferers were from the Island of Itaparica, where many whales are caught, and the oil extracted from the blubber. The poor fishermen and others often eat the flesh of the whale, and some of those in the hospital attributed the disease to this; but there were others in the establishment from inland districts, where this cause could not operate.

Dr Danielsen, a Norwegian physician, gives some sketches of a disease resembling this; but I speak from memory, and am uncertain if I have even given his name correctly.

My remarks on Pernambuco shall be brief, as all my information in regard to it is derived from others. It is situated in  $8^{\circ} 3'$  south latitude, and  $34^{\circ} 51'$  west longitude. The coast here is low and flat. The old town of Olinda is beautifully situated on a cluster of eminences connected with the mountains to the westward of the city, and is about three miles from the modern town Recife, which is the mart of commerce, and is built on the southern extremity of a low and sandy peninsula, formed by the mouths of two small rivers, the banks of which, at a little distance, are studded with the mangrove. The population is estimated as follow:—Whites (native, 34,000; foreign, 6,000), 40,000. Blacks (native, 30,000; African, 20,000), 50,000. Mulattoes and people of various degrees of colour, 30,000. Total, 120,000.

The French bark *Alcyon* arrived from Bahia at Pernambuco on 17th December, with yellow fever on board. Four men were landed from her at Bahia with fever before she sailed. It is said that she lost two on the passage. A man landed from her died on shore at

Pernambuco on the 19th December. Ships from Bahia had to perform a quarantine of eleven days after this. The consul says, on the 21st December, "Two cases have been reported on shore." Dr Paton, young, healthy, and robust, and not acclimated, was attacked on the 25th, and died on 27th December. On this last day, Mr Pitt, the apothecary at the British Hospital, got the disease, and died on the fourth day, with suppression of urine and black vomit. Dr May says:—"One or two cases appeared in the town, and were speedily fatal; but nothing was said about them, in order not to create alarm." As early as the 6th January the shipping was attacked. By the 20th February not less than 30,000 persons had been attacked on shore by the fever; and the business of many of the public departments could scarcely be carried on from the number of *employés* affected. Early in March the fever had increased in intensity, no longer confining itself to persons newly arrived in the country, but proving fatal to natives as well as foreigners. Eleven of the British community, and probably 2000 residents, had perished. The disease was extremely fatal among the Europeans recently arrived, both on shore and in the ships. About 60 per cent. died; 16 per cent. of all foreigners attacked died, and only about 3 per cent. of the natives. Sardinians here, as in other places on the coast, suffered more severely, both afloat and on shore, than other foreigners.

Rio de Janeiro, the capital of the empire, is situated in 22° 53' south, and 43° 9' west, on the western side of a large landlocked bay, which forms one of the finest harbours in the world—about 24 miles long, by 15 miles extreme breadth, and 120 miles in circumference. The city proper contains a population of about 200,000; but if the suburbs are included, about 300,000,—of these, about a third are white, many of them being foreigners (Europeans); the rest consisting of blacks, and people of various degrees of colour—the pure blacks being by far the more numerous of the two, three to one perhaps. The city is surrounded on nearly all sides by mountains of various degrees of elevation; on the seaside, the Corcovado (the highest), is about 1600 feet above the level of the sea; on the other side, the Organ mountains, in some places, attain an elevation of 6000 feet, and are distant about 20 miles; those on the east side of the bay are not so high, not exceeding 1000 feet perhaps; the tide rises 8 feet at full and change in the harbour, which contains several islands. The city is, for the greater part, built on a plain, little above high-water level; there are, however, several hills of small elevation, partially built upon, which add to the beauty of the place, such as Gloria Hill, Signal Hill, the Sauda and Gamboa hills. The buildings of the city on the Corcovado side extend close to the foot of the hills, into narrow valleys, steep on both sides, and quite secure from the sea breeze; in these places there are many fine houses, which are occupied by many people who prefer such

localities to the more elevated and exposed situations, particularly natives, and such as have been resident long in tropical countries. They appear to have arrived at this conclusion from experience, and account for the fact by saying, that the frequent and sudden alternations of temperature, force of the wind, etc., in elevated situations, are injurious to the constitution.

Narrow streets, with high-built houses, give more shade from the sun in hot climates than a different arrangement would effect; and the inhabitants of many tropical countries appear to act on this view of the matter, yet it is questionable how far the increased protection from the sun will counterbalance the deficient ventilation; and there can be no doubt that the cleansing operations in towns, with narrow streets, are more difficult of execution, and ought to be very perfect. The streets of Rio are long, straight, and narrow, generally speaking, with open gutters; they are neither clean nor well-paved; and the greater part of the city being so level, any little inequality retains water, which generally remains there until dried up by the heat of the sun. There are three large open squares in the city.

There were some unusual and desultory efforts made to purify the city, at the time when all their fears in regard to the fever of 1850 were realised; and now they have a small body of men employed exclusively in keeping the streets clean, but not half enough to do the work effectually. The city is better lighted too, and there is even a prospect of seeing it lighted soon with gas.

The substance of which the mass of the hills around the city is composed is gneiss, in which numerous quarries are opened. The gneiss is often intersected by granite veins, of several feet in thickness; the rock, however, is rarely exposed, the hills being covered almost to the top with vegetation. To the westward of the city there is a rather extensive marsh, the water of which becomes occasionally mixed with the salt water of the bay. This is usually considered the most insalubrious kind of marsh; and, at the head of the bay, several small streams of short-course empty themselves. Their mouths are surrounded by marshy alluvial deposits, covered densely with *paulliniæ* and *rhizophoræ*; and occasional patches of marsh occur beyond this, as far as the foot of the Organ mountains.

This harbour, from the same cause which renders it so secure, having a narrow entrance, and being surrounded by mountains, is, compared with other places on the coast, considered hot. It is near the tropic also. The sea breezes are generally not so strong, do not set in so early in the day, and are less regular in their recurrence than at Bahia and other places to the northward of this.

The average heat of the two hottest months is about 84°, and of the two coldest about 72°. Thunder-storms, and great falls of rain, are said to be less frequent than formerly, and the land and sea breezes less regular. From my own observation also, I should say, that cases of dysentery and hepatitis are more frequent in her

Majesty's ships on this coast, than they were ten or eleven years ago. Rio, considering its position, size, and latitude, and the general neglect of all sanitary measures observed in it, has, until lately, as well as the whole of the Brazilian coast, been considered remarkably healthy; and as being, to the surprise of every one who attended to the subject, remarkably free from diseases of the zymotic kind. Agues have been endemic in particular localities; they never became epidemic, nor very fatal, consequently did not attract much attention. Bronchocele is not uncommon in the valleys among the Organ mountains; and no one can walk the streets of Rio for many hours, without observing how very common sarcocele is; hydrocele is rather a frequent disease also, and so is elephantiasis of the scrotum. There had been at Rio, during the four years previous to the appearance of yellow fever, a regular recurrence during the hot months of "Polka" or "Insolation Fever." The character of this fever has been already referred to; and the city was considered very healthy in October and November 1849. On the 13th December 1849, the Brazilian war steamer "Alphonso," arrived at Rio from Bahia with the news that yellow fever was raging at Bahia; that H.M.S. "Raleigh," which left Rio on 14th November, had touched at Bahia on the way to England, and got some cases of fever. On the 14th December, the Portuguese brig of war, "Don João," arrived at Rio from the same place, having lost two on the passage from this disease. This vessel was put in quarantine. Her Majesty's packet brig "Petrel," arrived at Rio on the 23d December from Bahia, with two cases on board: both died a day or two afterwards in Rio harbour.

The case, which was for a time generally considered the first one in the bay, was seen on the 28th December in the City Hospital, "Misericordia," an unequivocal and concentrated case of yellow fever, in a (Russian) Finlander, of the name of Enquist, who had arrived about two weeks previously direct from Russia, in the Russian brig "Volga." He had been ill some days; had then "a yellow colour of the skin, black vomiting, suppression of urine, hemorrhage from the mouth and bowels," and he died on the following night.

Dr Lallemand refers to a case earlier than this in the harbour. He says:—"When, on the 11th January 1850, at a sitting of the Medical Academy of this place, I submitted the cases of yellow fever which had come under my own observation, Dr Noronha Feitel stated a case, which had occurred at the Naval Hospital on the 9th December, when a man, who had come from Bahia, in the steamer 'Don Pedro,' was admitted into the Naval Hospital on the Ilha das Cobras; he died soon afterwards of black vomit and diarrhoea. Now, although this case occurred almost close to the city, no further consequences ensued; nor did the physician of the hospital attach any importance to it, for he did not speak of it until a fortnight afterwards, in consequence of my statement at the academy, and even



then he merely mentioned it casually." The Rua da Misericordia is a dirty narrow street, containing many lodging-houses for sailors. It is close under the lee-side of the hill on which the old hospital of the same name is situated. Enquist had been lodging in this street when attacked. Several other cases, from from the same part of the same street, occurred almost immediately after Enquist's case. For a fortnight or more, the disease in the city was confined to the above street, and chiefly to three lodging-houses for sailors, kept by people of the names of Frank, Wood, and Flourde. Certainly these houses were not remarkable for their cleanliness, or the regular habits observed in them. The next part of the city attacked by the disease was "Sauda," a dirty locality—the word means "health;" but the neighbourhood is anything but favourable to that great blessing. It is distant about one and a half miles from the Rua da Misericordia, almost on the opposite side of the city, close to the water, and scarcely elevated above the sea-level. Many of the merchant vessels load and discharge here. It is situated on a recess of the bay, and is protected from the full force of the tides and currents of the harbour by the island of Cobras, and the high and projecting point of land opposite to it; the sea-breeze, also, before it reaches this place, has to pass over almost the whole breadth of the city. In consequence of the place being so situated, almost all the filth discharged into the harbour finds here a secure lodgment, close to the shore; and at low-water the effluvia escaping from it is very unpleasant indeed. The streets in the neighbourhood are not clean, badly paved; and after rain, until dried up by the heat of the sun, are full of little pools of water. The sea-breeze, also, before it can arrive at either the Rua da Misericordia or Sauda, has to pass over a dirty place on the beach, where most of the cattle for the consumption of the city are slaughtered. The blood, etc., are allowed to soak into the unpaved ground, or to be dried by the sun. The stench at this place is at times unbearable. From these two quarters of the city, the disease soon extended over the whole, and over the suburbs also.

The first deaths afloat after the two who died in H.M.B. "Petrel," occurred on the 9th Jan., on board the Finland vessels "Niored" and "Norna;" at least these are the first we have any record of. These cases proved rapidly fatal. Several other ships, Russian and Swedish, were quickly attacked after these, and soon after the disease was very general and very fatal in the shipping; ships recently arrived from northern Europe suffering most. I would here remark that the precise date when an epidemic attacks any place, no matter what the epidemic is, is not easily determined; other diseases assuming its character more or less, during both its approach and its decline. This, however, is now so universally acknowledged that the present remark might perhaps have been omitted, were it not for its peculiar applicability to epidemic fever.

The disease continued to advance in the city and suburbs as well



as in the shipping, attacking first the natives of cold climates, particularly if recently arrived from those climates. After a time, acclimatised Europeans caught the disease, as well as the white natives; the blacks had the disease very generally, but very few of them died. The disease was most prevalent and had acquired its greatest force in March. By the middle of May the number of attacks were much reduced, but the mortality in proportion to the number attacked was very high. In July, cases were still occurring, and the last fatal case on shore in 1850, died early in August—a lady, a Brazilian subject, of English descent, about thirty years of age.

Dr Curtis, who was in practice in the city, a young man of much promise, who laboured hard during nearly the whole of the epidemic, and was daily acquiring fresh esteem, ultimately got the disease, treated himself, and died in May.

Dr Glass, also, who had been long resident in Brazil, and universally esteemed, had been actively employed combating this fever at Santos, 24° south, a town with about 6000 inhabitants, the port of San Paulo, died on the 5th of June—the last fatal case in the place.

San Paulo, in latitude 23° 33' south, distant about eleven leagues from Santos, its port, and about forty from Rio, elevated about 2464 feet from the sea-level, with a population which, in 1840, was said to number 40,000, but now supposed not to exceed 20,000, had daily communication with Santos; and after a good deal of inquiry, both at Santos and Rio, and from people who had been at San Paulo lately, I have not heard of a single case of yellow fever having been seen there. Certainly the climate of this place is cool; hoar-frost is occasionally seen on the ground early on winter days.

Petropolis, also, a new mountain city, about forty miles distant from Rio, at an elevation of about 2000 feet, beautifully situated in a gorge of the mountains, having more than daily communication with Rio, escaped the disease. It is the common resort of the more opulent inhabitants of Rio when in search of health, or relaxation from business, and at the time when fever prevailed in the city, many of these people went there to avoid the disease. In a very considerable number of them the fever became developed after their arrival, and they died; but the disease never extended to such as had not been to Rio, or the low grounds.

Constância is the beautiful residence and sanatorium of Don Ricardo Heath, about sixty miles distant from Rio, and at an elevation of about 3100 feet, where the hospitable owner has been so successful in cultivating the fruits, flowers, and vegetables of both the temperate and torrid zones, side-by-side in the same garden, and in the open air. The climate here is such, that the thermometer seldom falls to 32° on winter mornings, while at two or three P.M. in the summer it is sometimes as high as 80°—the diurnal difference, as might be expected from the latitude and elevation, being considerable, often 20°. This place, I need scarcely say, escaped the fever, notwithstanding that many had arrived there from Rio at the time of its

prevalence at the latter place ; as did also the Morro Quemada, a small Swiss colony somewhat similarly situated.

The mortality in the shipping in Rio harbour was great. Some of the merchant-ships lost all hands, and remained some time at anchor in the middle of the bay, in charge of the consul. Nor did the men-of-war escape—the Portuguese ship of the line “Vasco de Gama” remained in harbour all the time that the fever prevailed in 1850 ; she had a crew of 544, of whom 500 had fever, 167 had second attacks, and 83 died. The Portuguese corvette “Iris” was in harbour nearly as long, with a crew of 180 ; all had fever, 58 had second attacks, and 46, including the commander, died. The Brazilian frigate “Constituciao” was also in harbour all the time, and had a great mortality.

Her Majesty's steam-sloop “Cormorant” arrived at Rio from Monte Video on the 4th February 1850, and remained in the harbour, with the exception of a few hours, until the 15th. On the 10th, one, and on the 14th, another, case of mild remittent fever occurred ; both returned to duty in about seven days. The ship returned to harbour on the 25th February, and left again on the 28th, and during these three days she had frequent communication by the ship's boats with the coal-wharf at Sauda. On the morning of the 4th March, Lieutenant de Kantzow, and Mr Bunbury, a midshipman, left the ship in the boats to cruise about St Anne's Islands, about ninety miles to the northward of Rio, to intercept slave-vessels ; and when the ship returned to those islands on the 9th March, we found that Mr Bunbury had died that morning of yellow fever—having been attacked on the evening of the 4th. He had delirium, with a yellow skin and black vomit, and died in a tent on the beach. The ship returned to harbour on the 16th March, healthy, and anchored in the fairway, as far from the shore as possible, and exposed to the full influence of the land and sea breeze, and communication with the shore was restricted as much as circumstances and the exigencies of the service would permit.

I recommended that the ship should go to sea as soon as possible. The boilers, however, required repairs, and other duties detained us in harbour until the morning of the 23d. On the morning of the 19th, a mild case of fever occurred in a man who was subject to attacks of plethora, with headache, vertigo, etc. At this time the same symptoms were present. The case being a mild one, the type indistinct, and being unwilling to cause more alarm than necessary, I did not report this at the time as a case of the prevailing epidemic. Another similar case occurred on the 21st, and both, although not severe, had a prolonged convalescence.

On the afternoon of the 19th March, H.M.S. “Tweed” arrived at Rio from Pernambuco, where she had got the disease, and where she had lingered long, contrary to the advice of the surgeon of the ship : she had already suffered, and was still suffering, severely from this fever. She remained in Rio harbour until the 28th, then went

to Monte Video, and was put in quarantine there. She lost altogether 18, of whom four were young officers, from this fever. There is every reason to believe that the fever was disappearing in her when she arrived at Rio, and that a fresh outbreak occurred in her there, or soon afterwards. On the arrival of the "Tweed," I considered it my duty to communicate with the surgeon of that ship, to obtain as much information as possible about the prevailing epidemic.

On the afternoon of the 22d March, at one o'clock, a stoker of the "Cormorant," and at two P.M., the commander, who had just returned from visiting the "Tweed," complained. The latter had a severe attack, and always fancied he had got the disease in the "Tweed." Between this time and eight P.M., four more were seized, all with symptoms of fever of the worst type. There was no trifling with the disease any longer. We sailed on the morning following, proceeding to the southward, adding several new cases daily to the sick list, and arrived at Monte Video, in latitude 34° 55' south, on the 30th March, with 27 on the sick report, having already lost four on the passage. The ship was there placed in quarantine, and the inhabitants showed in various ways their great dread of the disease. Being still in quarantine, we proceeded on the 11th April up the Rio de la Plata, as far as the mouth of the Pabon River on the north side of the Plata, and about 45 miles above Monte Video. At this place, after having painted the ship, filled up with fresh water, etc., at my suggestion leave was given to the men to go on shore, there being no spirits to be had in the place, nor the means of indulging in any other irregularity, hoping that this indulgence would be the means of dispelling the gloom which every one in the ship had felt more or less during the previous three weeks. Such as felt inclined went on shore and returned again quietly in the evening. Some of the officers had good shooting. And I do not remember having ever felt so much relief as on the 14th April, when, with "gun in hand," after shaking off the "cares of office" for a few hours, I had a good walk on shore, and had bagged a few snipe, wild ducks, and *teru teros* (*vanellus cayanensis*), a bird very like our lapwing, with a spur on the wing. On returning to Monte Video on 16th April, we were allowed pratique, no fresh case of fever having occurred since the 31st of the previous month, and only four convalescents remaining on the list, the commander being one of them. At Monte Video, the men again had leave to go on shore, but the same regularity did not characterise their conduct as on the previous occasion. One man was drowned on the 2d May, by falling, when in a state of intoxication, off the pier into the water; another man, who was not quite sober, leaped into the water to try and save his comrade's life, and afterwards slept in the middle of the street all night in his wet clothes, the night being quite cold; and all the inconvenience he felt from it was a little stiffness of the neck, of which he never complained.

On the 20th April, another case of fever occurred ; and by the 2d May, five more ; and out of the six two died. I will afterwards remark on the difference of symptoms in these six cases. The weather during our stay in the Plata was clear and dry ; the middle of the day, for about five hours, being hot, with a dry and parching atmosphere, and at night it was quite cold to our feeling, with a strong breeze generally ; the thermometer often under 45°. On the 3d May we left Monte Video for Rio de Janeiro, having been absent all this time without orders ; and on arriving at the latter place on 12th May, had the satisfaction of finding that our going to the southward had received the entire approbation of our excellent commander-in-chief. We remained in harbour until the 16th of the same month. A boy of the name of Peel was, on the 15th May, made to stand aft on the upper deck for punishment. He deserted, and died on board a merchant brig on the 28th May of yellow fever. On the 18th May, a case of fever occurred in the ship in one of the men who had a previous attack ; another similar case occurred on the 20th, also a second attack : neither of these cases proved severe. In short, I may mention that during all the time this fever prevailed in Rio harbour, if we anchored for a few days in the harbour, we always had several cases of fever afterwards. These gradually became less frequent, and soon disappeared after our going to sea. The last case in the "Cormorant" occurred on 22d July. The last discharged from the sick report was attacked on 20th July,—a young man of twenty, who had a slight but long and lingering attack, with distinct remissions, and only returned to duty on the 11th September. During all this time he was never altogether confined to bed.

I may as well give here the following tabular views of the cases of fever in the "Cormorant," to avoid much repetition, between the 1st of February and the end of September 1850.

Ages, .....	Under 15.	15 to 20.	20 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	Over 50.	Total.	Average.	Youngest.	Oldest.
Number in the ship, } 15th March,	1	22	47	39	13	11	5	6	1	145	27	14	54
No. of cases of Fever,	...	6	13	10	6	5	...	2	...	42	26½	15	46
Do. Death,	...	3	6	1	1	...	...	...	...	11	22⅘	18	34
Do. Second attacks,	...	...	1	2	...	1	...	...	...	4	27¼	23	38
Do. Hepatitis fol- } lowing Fever,	...	...	1	2	...	1	...	...	...	4	28¼	24	37

sh. The first death happened on the 9th March, the last on 5th  
this

May. One died in three days and seventeen hours from the time of attack, seven on the fifth day, one on the seventh, one on the thirteenth, and one on the seventeenth of the disease; the immediate cause of death in this last case was sphacelation of the whole of the right upper extremity. The number (145) stated above as being in the ship on 13th March, may be taken as the average number during the whole period; that number having been altered only by the fever and its results. Thus, out of 145, forty-two were attacked, eleven died, two were invalided, three sent to hospital, of whom only one returned to the ship and to his duty, the other two having been subsequently invalided from the hospital-ship. In one of the cases invalided directly from this ship, the fever—of which he had two attacks, one in the “Crescent,” when imprisoned in that ship, the other on his return to the “Cormorant,” an interval of nine days, during which time he did his duty as a working carpenter, intervened between the attacks—had only aggravated an old complaint—palpitation from organic disease of the heart, to which he had been subject for years, and which would most probably have soon rendered invaliding necessary, even if no fever had interfered. One of the other cases invalided from the “Crescent,” after an attack of fever at Monte Video, had the most acute attack of hepatitis which—notwithstanding a good experience of the disease in the tropics and in India—I have ever seen. This man was again on the station in less than four months, having again joined the first ship fitting out after his return to England. All the four cases of hepatitis consequent on fever, occurred on the return of the ship to within the tropic from Monte Video, almost immediately on that return, and within a week of each other; three of the four were ultimately sent to England. The three cases sent to hospital were cases of hepatitis following fever. One was invalided directly from this ship on 13th May, for the same complication of diseases, having been on the sick report since 22d March.

It was remarked in the “Cormorant” on this occasion, as it has been elsewhere in other epidemics, that when the disease became prevalent in the ship, other cases of disease disappeared. Dr Wilson, in his Statistical Reports on the Health of the Navy, says “It happens frequently when one disease prevails epidemically, that other diseased actions are suspended, or are less frequent, the master-malady, so to speak, having the power of resisting the attacks of other occasional diseases.”

It will be observed that there were cases of all degrees of severity, from those who returned to duty in three days, to such as died in little more than the same period of time; and it may be objected to, that the former should be included in a list of cases of fever, more especially of yellow fever. They might easily have been excluded, as they form but a small fraction of the whole; but have

been retained, because they presented themselves at the time of attack with precisely the same train of symptoms as many of the more severe cases ; were reported at the time as cases of fever ; were included also under the same head in the official reports ; and, I feel convinced, proceeded from the same cause, administered perhaps in a smaller dose. And I must state that others were excluded for reasons almost opposite to the above, and because the result did not demand an alteration, which might with equal propriety have been included in the list.

The same objection will apply of course to one of the cases mentioned as a second attack, inasmuch as the first attack lasted only three days ; and I may as well state here that none of those who had two attacks had been considered dangerously ill, except one in whom the same intestinal irritability—which proved so troublesome in many other cases, and which will be referred to hereafter—caused for a few days some apprehension in this case also. In none of the cases in the “Cormorant” was there suppression of urine. In several it was diminished in quantity, and high coloured.

A yellow colour of the skin was a common symptom ; it generally appeared about the third day, and was more or less deep in tint in different cases,—of course being most readily observed in those who previously had been of a ruddy complexion. In several of the fatal cases, and immediately after death, a dark mottled ecchymotic appearance was observed about the epigastrium, breast, neck, and shoulders ; the colour resembling that of an ecchymosis when the diffused blood begins to be absorbed.

(*To be continued.*)

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ARTICLE IV.—*Diseases of the Nose and Pharynx.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

*Fibrous Polypus of the Nose.*

J. N., aged 22, from Berwickshire, was admitted into the hospital on the 20th of July, on account of a very formidable disease affecting the nose and throat. He stated that, about two years before, his breathing had become impeded through the left nostril, and soon afterwards also through the right one, and that for more than six months past he had noticed a swelling in the throat, which had constantly increased, and occasionally been attended with profuse bleeding from the nose. Upon examination, I found that respiration was entirely obstructed through both of the nostrils, and that a large round tumour, of firm consistence, occupied the upper part of the pharynx, so as greatly to interfere with articulation and deglutition. There was no thickening at the root of the nose, or widening of the space between the eyes, nor was there any purulent or foetid



discharge. By these characters, negative as well as positive, I recognised a "fibrous polypus," and readily undertook its removal.

The patient being seated on a chair, I introduced a pair of forceps through the left nostril, where the disease had originated, with the view of destroying any attachments that might be within reach; but not encountering the root from this direction, altered my plan of attack, and, fixing a hook into the pharyngeal part of the swelling, drew it sufficiently forwards into the mouth to permit its seizure by the claws of the strong double-hooked forceps, which bear the name of *Muzeaux*; then pulling and twisting with all the force in my power, and at the same time aiding the separation by means of my forefinger, introduced behind the palate into the posterior opening of the nostril, close to which I felt the attachment was, I succeeded in wrenching out the morbid growth. Profuse hemorrhage attended and followed this procedure, but was restrained by plugging the nostrils from behind and before; and the patient, quickly recovering from the rough treatment to which he had been subjected, returned home on the 2d of August, completely, and I trust permanently, relieved from his complaint.

The fibrous polypus, originally described by the late M. Dupuytren, is certainly a very interesting and important subject of surgical practice. It usually occurs in young and otherwise healthy persons, occasions the most distressing and alarming symptoms, and admits of effectual remedy. It is distinguished by the firmness of its texture, by the strength of its attachment, which is to the surface of sound bone, and generally that composing the posterior part of the lower passage of the nasal cavity, and by the profuse hemorrhage frequently proceeding from it. This tendency to bleed is apt to make the disease be mistaken for one of a malignant and incurable kind, so that the patient is left to his fate, instead of being easily and effectually relieved by evulsion of the tumour. Two years ago, Lord ——— applied to me, in almost precisely a similar state to that of J. N., above related. He had returned home from her Majesty's colonial service, on account of the excessive distress and hemorrhage caused by the disease, and had spent two months in London, under the care, successively, of Mr Bransby Cooper, Sir B. Brodie, Mr Travers, and Mr Cæsar Hawkins,—all of whom, separately and collectively, regarded his case as incurable and hopeless. He then applied to me; and, recognising the nature of the tumour, I at once undertook its removal, with the instant effect of transferring him from a state of constant misery and anticipation of death to the enjoyment of perfect health. The period that has now elapsed, without the slightest symptom of relapse, notwithstanding a life of the most energetic exertion, will, I trust, satisfy the most sceptical of his other attendants that the disease was not of a malignant kind. I should not think it necessary to say so much upon this subject, were it not for the silence in regard to it by those metropolitan authorities, who represent the surgical opinion of England. It cer-

certainly is desirable that the cases which at present are overlooked, should be recognised and properly treated.

*Fibrous Tumour of the Soft Palate.*

H. S., aged 35, from Perthshire, was admitted on the 27th July, complaining of a tumour in the throat, which had existed for six years, and gradually increased. It was seated in close connection with the right tonsil, which it nearly equalled in size. The substance was very firm, its surface slightly nodulated, and its connection, so far as could be judged from the natural consistence of the neighbouring parts, distinctly circumscribed.

Having lately had occasion to remove a similar, though smaller, tumour from the same region, and accomplished the object without any difficulty or bad consequence, I felt encouraged to attempt something for this patient's relief. The great danger, of course, was from bleeding in such an inaccessible situation, where a small amount would so much interfere with respiration; and, in the first instance, I thought of laying open the cheek, so as to facilitate the measures that might be necessary for arresting hemorrhage; but, upon reconsideration, I resolved to keep this step in reserve, and to begin by dissecting out the tumour, so far as might be found practicable, in the hope that the operation would perhaps admit of being thus accomplished, without the severe complication just mentioned.

On the 3d of August, having placed the patient in a good light, and depressed her tongue by means of the curved spatula used in applying caustic to the pharynx or larynx, I fixed a hook into the tumour, and drawing it forward, divided the mucous membrane covering it, by a vertical incision, throughout its whole extent. Having thus exposed the surface of the growth sufficiently, I fixed the double-hooked forceps into its substance, and proceeded to divide the connections by cutting them, not parallel with the morbid surface, but perpendicularly to it, while in a state of tension. In this way the extirpation was quickly completed, without the loss of more than a teaspoonful of blood; and the patient, who had no subsequent trouble, left the hospital on the 12th of August.

*Hypertrophy of the Nose.*

The respectable old man, whose aspect is delineated below, came from Dumfries in the beginning of July, to obtain relief from the monstrous excrescence, which had been growing for a number of years. Finding that he was upwards of eighty, and having expressed my surprise that at an age so advanced he should wish for interference, I was told that his existence had been rendered intolerable by the hooting and jeering of children in the street, when he went abroad. As I had operated, in the same circumstances, on a still older patient, with perfect success, there was no objection on

my part to the undertaking, which was accordingly carried into execution without delay.

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In performing the operation, Mr Liston has advised that the assistant should keep his finger in the nostril, and advertise the operator when he feels the knife coming too near the cavity; but I think it is a better plan for the surgeon to use as a guide one of his own fingers, which may be either the little or ring finger of the left hand, so as to leave the others free to manipulate the excrescence while it is separated by the knife. I have never met with bleeding in the slightest degree alarming or troublesome, and am surprised to find that apprehensions on this account have been entertained, since stuffing the nostrils, together with external pressure, would effectually command any amount of hemorrhagic tendency. It should be recollected that, as the swelling depends upon a morbid development of the cutaneous texture, there is no occasion for the contraction of granulation in the formation of a cicatrix, as when a portion of the skin through its whole thickness has been removed. The raw surface heals like that resulting from the action of a blister; and hence, while recovery is easily and quickly accomplished,

there is no change in the size or form of the parts allowed to remain, which should, therefore, be at once reduced to sufficiently limited dimensions.

Proceeding upon this principle, I afforded the relief desired, and by the end of a week had the satisfaction of dismissing my venerable patient securely protected against the indiscreet remarks of his juvenile neighbours.

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## Part Second.

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### REVIEWS.

*A Practical Treatise on Diseases of the Skin.* By J. MOORE NELIGAN, M.D., M.R.S.A., etc. etc. Dublin: 1852. Small 8vo. Pp. 439.

DR NELIGAN arranges diseases of the skin in ten groups, with two supplementary ones, as follows:—1. Exanthemata; 2. Vesiculæ; 3. Pustulæ; 4. Papulæ; 5. Squamæ; 6. Hypertrophie; 7. Hemorrhagiæ; 8. Maculæ; 9. Cancrodes; 10. Dermatophytæ. The supplementary groups are Syphilides and Diseases of the Appendages of the Skin. This modification of Willan and Bateman's arrangement is open to several grave objections, which we shall proceed to notice.

Among the Exanthemata, erysipelas is retained, whilst rubeola and scarlatina are struck out. Now, it is difficult to account for this. In our opinion, erysipelas is as much febrile in its character as the other two, and is often as infectious. In the order Vesiculæ, he places eczema, herpes, pemphigus, rupia, and scabies. Now, if Dr Neligan wishes to abolish the order Bullæ, rupia, at least, he should place among the Pustulæ. Unlike pemphigus, it forms a prominent scab, produces ulceration, and heals by a cicatrix, and ought never, therefore, to be grouped with such diseases as eczema and herpes. Scabies has long had a doubtful position in artificial classifications; but M. Bourguignon having at length proved experimentally that an acarus is the true cause of the disease, and that the eruption is accidental, it, with phthiriasis and some forms of acne dependent on the entozoon folliculorum, ought to be classed in a distinct group (Dermatozoa).

Dr Neligan has removed ichthyosis from the Squamæ, and placed it in a separate genus, the Hypertrophie, which also swallows up those disorders hitherto called Tuberculæ. But we would ask Dr Neligan whether psoriasis and pityriasis are not also hypertrophies of

the epidermis, and have not equal claims to enter the order *Hypertrophie*, and therefore still to remain allied? We also consider it unfortunate that the author introduces an order, *Hemorrhagiæ*, only for the purpose of admitting purpura. If this disease had been struck out altogether, on the ground of its dependence on an affection of the system generally, we could understand it; but in an artificial system of skin diseases it is to all intents and purposes a spot, or *Macula*. Now, as Dr Neligan retains this order with a view of including vitiligo and ephelis, we cannot but consider the introduction of a distinct order, simply because the spot depends on hemorrhage, to be unnecessarily complex. As well might he introduce another order to comprehend ephelis, because it consists of an increased amount of pigment. Neither can we approve of the introduction of an order, *Cancrodes*, to contain lupus and keloid. The former we regard as a scrofulous tubercular disease of the skin, whereas the latter would with more propriety have been placed among the *Hypertrophie*. Thus, if the author, following Hebra, feels it necessary to introduce the order *Dermatophytæ*, surely he is equally bound to introduce the order *Dermatozoa*, which, however, he has not done. There can, we presume, be little doubt that some forms of prurigo are owing to the rapid formation of body lice in the skin, and we now hold that scabies has also been proved to be of animal origin. We do not object, in a practical point of view, to the Syphilides and diseases of the hair and nails being considered in separate groups, although we believe that, as our knowledge advances, the former will be regarded as mere modifications of the simple affections, and the latter as their occasional sequelæ.

So much for classification, which those who seriously study skin diseases know to be a very essential matter—essential both to diagnosis and treatment. It may well be conceived that practitioners who cannot determine the difference between a pityriasis and a chronic eczema, between sycosis and prurigo of the chin, or between prurigo and scabies, may sneer at the labour bestowed on the arrangement of these disorders. But it is exactly on account of the attention directed to their grouping that the eye readily recognises the difference and similitudes between them, and that experience in treatment is at length enabled to interfere with benefit. How, for example, if a certain application be useful for scabies, can it be expected to cure a patient affected with prurigo? No doubt, our remedies are still for the most part directed on empirical principles, but nothing rational can be hoped for unless the nature, progress, and diagnosis of these diseases be first ascertained.

Did our space allow, it would give us much pleasure to comment on Dr Neligan's treatment of the various skin affections. In several points we differ from him; in most we agree. In our opinion he does not attach sufficient importance to keeping the surface moist with alkaline lotions, which in our hands has proved a more cleanly and useful mode of treatment in various skin diseases than any.

other. Neither does the employment of simple lard or oil receive much notice, although, whenever it is necessary to exclude air and remove irritation, nothing can be more valuable. Still Dr Neligan's treatise is a very useful one. It is evidently the result of much experience and thought, and from its conciseness will prove eminently serviceable to the practitioner.

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*The Physical Diagnosis of the Diseases of the Abdomen.* By EDWARD BALLARD, M.D., London. London: Taylor, Walton, and Maberly. 1852. Pp. 276.

DR BALLARD has produced a highly meritorious work, which we have no doubt will be favourably received, both by students, and by practitioners, for whose use it seems to have been especially designed. His aim has been to collect and arrange in a readable form all that has been positively ascertained regarding the application of means of physical diagnosis to abdominal diseases. The work has no pretension to originality, except in the mode of arrangement of its component materials. It is divided into three parts; whereof the first treats of the method and general results of physical examination of the abdomen by inspection, mensuration, palpation, percussion, and auscultation; the second enumerates the different visceral lesions, with the train of physical signs by which each is to be recognised and discriminated; the third is a mere catalogue of physical signs, under each of which is placed a list of the pathological conditions with which it *may* be associated,—an asterisk being prefixed to those lesions in which the sign is of diagnostic importance, and copious reference being made under each heading to the chapters of the first and second divisions of the work.

We need hardly reiterate here a conviction which we have frequently expressed, that skill in the employment of the means of physical diagnosis must be acquired by every physician who would practise his profession with satisfaction and credit to himself, or advantage to his patients. But the skill to which we allude is not easily acquired—it is not mere manual dexterity in feeling, kneading, punching, and percussing—it is not that practice of the eye which distinguishes the most trifling abnormalities in outline or colour, and appreciates the most minute spaces in a microscopic field of view—it is not that education of the ear which informs us of time and tune, and pitch, and timbre—but it is the combination of manipulative dexterity in appealing to the evidence of our senses, with sufficient anatomical and pathological knowledge, and habits of sound reasoning. The skill of which we speak must be acquired, by those who can acquire it, by the ordinary preliminary training of a liberal education, in the anatomical theatre, at the bedside of the sick, in the dead-houses of our hospitals, and in the writings of good medical authors.



Treatises like that of Dr Ballard, however excellent and useful in their way, are liable to be abused. The student is apt to consider them as short roads to knowledge which he finds otherwise difficult of attainment. On looking at the third division of the book, for example, an ignorant man might be disposed to imagine, that in a case of abdominal disease, a diagnosis might be arrived at, *ope* Ballard, just as the genus and species of a British moss might be discovered by referring to Hooker and Taylor's *Muscologia*. It might be so, indeed, if there were no signs but *physical* signs, if all signs were at all times appreciable—and if for the convenience of diagnosis, diseases would cease to embarrass us by their complications. But how different is the reality! There are no cases of disease more obscure than certain abdominal tumours, and we will venture to affirm that no physician or surgeon can have seen many such, without feeling that his powers of diagnosis, aided by every appliance of modern science, may occasionally lead him to an erroneous conclusion.

The very fact that there seems occasion for such an elaborate compilation as that of Dr Ballard, is perhaps sufficient evidence of the difficulties which beset the subject which his labours are intended to illustrate. He has executed his task in an able manner, extracting, condensing, methodising, and arranging, within very moderate compass, a vast amount of useful information. The style in which the book is written is simple and clear, contrasting favourably with that affectation of aphoristic accuracy which bewilders the readers of some of the most eminent of modern medical writers, and with that intolerable prolixity of detail wherewith certain less distinguished authors swell their volumes and shroud their meaning.

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*Military Surgery: or, Experience of Field Practice in India, during the Years 1848 and 1849.* By J. J. COLE, M.R.C.S.E., H.E.I.C.S.  
London: Highley & Son. 1852. 8vo. Pp. 224.

THERE are few terms which have been more misapplied than Military Surgery. It certainly should be understood to denote the duty of a military surgeon, who is charged with the medical care of some hundreds of men, women, and children, in the various situations, and throughout the various changes, which the exigencies of their country may require them to undergo, at home and abroad, on shore and afloat, amid the snows of Canada or under the vertical sun of a tropical region, in the burning plains of India or on the pestiferous shores of Western Africa. But, instead of this comprehensive and important meaning, it is too frequently limited to little more than the performance of operations, as if the daily duty of a regimental doctor were lopping off legs and arms. We have been assured that the

whole period of service in the medical department of the army has been sometimes completed without occasion having ever occurred for any other instrument than the lancet and tooth-key ; and we cannot too strongly impress upon gentlemen, whose views are directed to this field of professional employment, that what it chiefly requires from them, in addition to the ordinary skill of a general practitioner, will be a knowledge of medical topography, with the endemic and epidemic diseases incident to the different localities where troops may be placed ; the specialties of a soldier's life in barrack, in the field, and in hospital ; the changes of diet and clothing required by the different circumstances of climate and exposure ; the grounds of procedure in recruiting and invaliding, and the careful recording of experience. On the comparatively rare occasion of injuries being received from hostile collision, the surgeon's duty will be chiefly limited to the application of water dressing and the performance of amputation ; and, in the latter case, the operation will be much the same, whether it is required by the effect of a round shot or by the crushing of a railway-carriage wheel.

Very different ideas, however, seem to be entertained by the author of the book now before us. After dedicating it to the late treasurer of Guy's Hospital—whence we suppose he is a pupil of that school—he tells us :—

“ Having reached India in November 1847, in June 1848, by a combination of somewhat singular occurrences, I found myself in medical charge of Lieutenant Edward's army (18,000 strong), then encamped before Mooltan, into which it had driven the rebel Moolraj and his troops.”—P. vii.

It certainly was rather a severe trial for the equanimity of an assistant surgeon of six months' standing, without experience, or the preparation acquired by gradually ascending the scale of responsibility, to be thus suddenly invested with the principal, or rather, as it would appear, sole, medical charge of 18,000 fighting men, engaged in field warfare of the most active description. Such an unusual position might be expected to afford some remarkable fruits, and has assuredly done so, if in no other way, at all events in the production of this crimson-boarded, gold-decorated volume, of which the exterior is a fit emblem of the egotism and vanity of its contents.

The author, limiting his ideas of military surgery strictly to wounds and operations, writes as if no man had ever written on the subject before. Indeed, his course of instruction is so elementary, that, in regard to operations, he must needs begin with thus teaching his readers—

“ *How to hold Instruments.*

“ *Amputating Knives.*—The amputating knife should be held between the thumb and fingers ; its handle should be free of the palm.

“ *The Saw* should be worked in a horizontal line, and grasped with three fingers and the thumb ; the forefinger being thrown out, as in indicating, by the side of the handle.

"*Scalpels and Bistouries* should be held between the thumb, index and middle fingers.

"*Probes, Sounds, Catheters, Artery-Forceps, Needles, &c. &c.*, should be lightly held between the index-finger and thumb."—P. 52.

He then proceeds, apparently unconscious of all that has been said and written in regard to gunshot wounds, from the worthy old Ambrose Paré down to Mr Guthrie :—

"We may now proceed to the consideration of the manner in which we [the author] are accustomed to treat all and every lesion to which the soldier is obnoxious in the field."—P. 63.

In accomplishing such an extensive task within the compass of 150 pages, brevity was, of course, essential; and, accordingly, the directions for treating wounded men bear no small resemblance to those of a cookery-book,—as, for instance, in the case of an incised wound of the scalp, we have the following receipt :—

*"Treatment."*

"Cut and shave off the hair completely around, thoroughly cleanse the wound from all foreign matter, put lint dipped in blood upon it, and a wetted compress over. Apply a bandage, and keep the patient at rest, with the head raised. Give calomel and purgatives, and bleed fully if there be a tendency to cerebral excitement or inflammation. On the second or third day remove the dressing; apply warm water. Lastly, to complete the cure, substitute the bark ointment, F. 33."—P. 177.

And this for "Gunshot Wound of the Kidney" :—

*"Treatment."*

"Full and copious bleeding, in order to restrain hæmorrhage; digitalis, hemlock, and henbane, to diminish the heart's action; calomel, opium, purgatives, low diet, and profound rest, to keep off inflammation, and, if possible, prevent destructive suppuration and sloughing. Small quantities only of acidulated barley or gum water should be allowed for the first two days. Examine the state of the bladder often, and introduce a catheter on the least appearance of retention of urine."—P. 129.

Having sufficiently illustrated the self-confidence of the author, we may now give a specimen of the wisdom which characterises his work :—

"Of chloroform much has been written; in its use much has been done. Some surgeons tell us they cannot operate without it: others declare it *never* does *harm*: many imagine it really does *good*: some look upon it as the most merciful gift of Providence: many swear by it: the natives of India bow down to it, and worship it as a god; some properly consider it (amongst many others) a useful remedy. But the practical surgeon views it in the hands of the *military* medical officer as a highly pernicious agent, which unquestionably it is. . . . No place should be assigned to it. Leave it with the medical storekeeper; place it on a high shelf in his warehouse, and, if it be hot, remove the stopper from the bottle to keep it cool. . . . That it renders the poor patient unconscious of pain cannot be doubted. But what of that? What is pain? It is one of the most powerful, one of the most salutary, *stimulants known*."—Pp. 89, 90.

We hope that the authorities of the H.E.I.C.'s Service will remark the sentiments here expressed, and pause before they appoint gentlemen who entertain them to the medical charge of an army.

## Part Third.

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### LECTURES, REPORTS, ETC.

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#### MEDICAL GRADUATION

THE annual graduation of Doctors of Medicine in the University of Edinburgh took place on the 31st of July, in the great theatre of the chemical department, when the fifty-one gentlemen whose names appeared in our last Number had this honour conferred upon them. The *Senatus Academicus*, with some of the Magistrates, occupied the space behind the experimenting table; the candidates, wearing gowns, were arranged on the benches in front; and the remaining space was densely crowded by friends and relatives, chiefly of the female sex, whose gay attire and interested countenances contrasted curiously with the sombre aspect of the academic body. The proceedings were commenced and closed with prayer by the Principal (Dr Lee). The candidates then, in succession, after signing the "*Sponsio Academica*," were presented to the Principal by the Dean of the Medical Faculty (Dr Balfour), and received the degree of M.D., after which the following address was read to them by the Promoter (Mr Syme):—

GENTLEMEN,—In compliance with an old custom, and in virtue of my office as Promoter upon this occasion, I have now the privilege of offering a few words of congratulation and advice. Your long and laborious ascent to the highest medical honours which this University has the power of bestowing, has been at length accomplished, and you have all received a title to practise your profession, which will be received with respect in every part of the habitable globe. Knowing the difficulties with which you have had to contend, and the success with which you have overcome them, I felt great pleasure in proposing to the *Senatus*, on the part of the Medical Faculty, that you should be created Doctors of Medicine. And now, addressing you as such, I beg to offer my sincere and hearty congratulations on this happy and honourable termination of your academic labours. But, gentlemen, I have still another duty to perform, and I trust that you will once more listen with the attention I have always had the pleasure of receiving from you while I endeavour to discharge it. This duty consists in giving some advice for the guidance of your conduct as members of the medical profession, to which rank you have just been promoted from that of students, and as the occasion is one of a kind so memorable, I venture to hope that what may now be said will make a lasting impression; and therefore, instead of attempting an oration calculated merely to please the majority of the present audience, and wind up the proceedings of the day with some carefully rounded sentences, I will endeavour to say something that may exert a beneficial influence on your progress through life. In the first place, then, I beg to warn you against supposing that, because the course of your academic study has been successfully accomplished, you may now repose from exertion, and wait patiently until time and chance place some good thing within your reach. You may say, Look around, and show us any instance of distinction through either scientific or practical achievements before the age of thirty; and if, therefore, eight or ten years must elapse before we can reasonably expect to do anything of importance, why should we not devote at least a portion of this period to recreation and enjoyment? But, gentlemen, professional success, like the fruit of a tree, cannot be produced all at once, and requires a vigorous growth previously for obtain-

ing sufficient materials as well as the power necessary to perfect their elaboration. All that we have been able to do for you is to sow the seed, and the amount of produce will be determined by the soil upon which it has fallen, together with the care bestowed upon its future cultivation. If you inquire into the private history of any man who has distinguished himself in our profession, you will be almost sure to find some early indication of the character ultimately acquired. Many of you, I am happy to say, have already shown hopeful signs of this kind, and you are all, I doubt not, eager to enter upon the great field in which the aspirations of youth are to encounter the realities of life. Let me, then, in the next place, warn you that the medical profession is at present in a condition well calculated to disconcert and mislead those entering upon its practice. The great feature of society in our day is credulity; which need not surprise us, when we consider the succession of unprecedented and apparently incredible events that have occurred in recent times. Formerly, it was deemed imprudent to take anything upon trust, or without due inquiry into its truth, and those who did so were regarded with compassion as the facile victims of quackery and imposture. But now we have passed to the opposite extreme, and come to view all such caution as unphilosophical, and indicative of a narrow understanding, which has not kept pace with the spirit of the age. The more new, the more startling, and the more inexplicable on known principles, any proposal may be, the more favourable is its reception with the public; and even in regard to the management of health—that most valuable of earthly possessions—a large proportion of patients, if offered the choice for their attendant between a man of talent, education, honour, and experience, and the presumptuous professor of some new-fangled folly, would not hesitate to prefer the latter. In entering upon practice, as you are about to do, deeply impressed with the responsibility you undertake, calling up to your assistance all the varied and extensive information you have acquired, and zealously devoting your whole energy to discharge aright the duties confided to your care, you will be apt to resent such unworthy treatment, and, under the influence of indignation naturally excited by it, be led into various errors, but especially two, against which I now beg to warn you. These are—opposing the follies that find favour with the public, and, what is still worse, adopting them. Suppose that, instead of the potent drugs which it has been the province of my colleague, the Professor of *Materia Medica*, to teach, you were to find faith professed in the globules of a homœopathic magazine—of which the whole might be carried in a waistcoat pocket, and swallowed without disturbing the patient's appetite for dinner; or suppose you were to find the careful diagnosis, founded upon accurate acquaintance with the symptoms and structure of morbid derangements, less trusted for the discrimination of diseases than the rambling revelations alleged to be suggested by placing a lock of the patient's hair in the hand of a mesmeric dreamer, you would naturally attribute such preposterous belief to an insane delusion on the part of those who entertained it; but they, of course, if thus perverted in their views, would refer your want of faith to a similar source; and hence, mutually looking upon each other as labouring under delusion, you could never be brought to the same mind by argument on the subject. The madman who believes that all the people in the world have lost their wits except himself, could never be persuaded of his error through reason; and remembering that "when the judgment is weak the prejudice is strong," you will, I trust, be satisfied that no good can come of attempting to argue your neighbours out of their faith in any of the forms which quackery may assume. It may be said that, in giving this advice, I am guilty of inconsistency between precept and practice, since the homœopaths have represented me as one of their bitterest opponents. But the fact is not so. I have never attacked the doctrines of this delusion, as it has always appeared to me, and I have never quarrelled with any one for adopting it either as practitioner or patient; but when members of our profession have taken up this system, as they call it,



and thereby declared that the principles which they previously professed were false and injurious, I have not hesitated to protest against their retaining titles and associations indicative of their still possessing the faith thus openly abjured. If you, for instance, who this day, in consequence of having completed a certain prescribed course of education, afforded evidence of having acquired the information which it was intended to supply, and solemnly vowed a dutiful regard to your *alma mater*,—have received a title and authority to practise medicine from this University, should afterward adopt a delusion utterly irreconcilable with all your professions, I think it is quite plain that your first duty would be to strip off the doctor's gown, and frankly confess that you were about to practise upon a different authority, as well as upon different principles. But the question here, you see, is one, not of homœopathy, but of morality, and in regard to which every man of common honesty is not only entitled, but called upon, to form and express a decided opinion. Thus far I have opposed homœopathy, but no further; and I would advise you to follow a similar course. But a much more serious error than opposing popular delusions would be their adoption, and I beg to express my earnest hope, that no views of expediency—no desire to promote your advance in practice—and no unmanly yielding to the caprice of an influential patron, will ever induce you, even in the slightest degree, to tamper with the sincere convictions of truth. You should recollect that no amount of money can ever compensate for the loss of honour and self-respect; and if you trust for happiness to the mere acquisition of gold, you should have entered upon some more lucrative occupation than is afforded by our profession. For the accumulation of wealth, or what is called making a fortune, the practice of physic and surgery is about the worst employment that could be chosen; and if therefore you feel no decided inclination for this calling,—if you take no pleasure in the pursuits upon which it is founded, and to which it leads,—and if you derive little satisfaction from relieving or palliating the distresses of your fellow-creatures, it would even now be the most prudent course to select some other field for the exercise of your talents. But if, on the contrary, you entertain a genuine desire to cultivate the various departments of medical study, can be happy in discharging the duties of our profession, without being rendered uneasy by the scanty remuneration accorded to them, and are willing to accept the respect and gratitude of your patients, instead of the observance and deference usually paid by society to the possession of wealth, you may safely enter upon practice, with the reasonable expectation of its yielding more true happiness than any amount of riches could possibly afford. I think it right, however, to remind you, that although the mere possession of money is not sufficient to make you happy, the want of it may render you in the utmost degree miserable; and of all the misfortunes that can befall you in entering upon the business of life, there can hardly be one greater than getting into debt. It at once destroys all peace and comfort—distracts the mind from its proper objects—substitutes anxious cares and gloomy forebodings in the stead of confidence and alacrity—blunts the feelings in regard to truth and honour—and on the ground of expediency or stern necessity is apt to suggest courses of conduct no less questionable in character than degrading in their tendency. Whatever moralists may say as to the connection between virtue and poverty, you may be assured that, in so far as the deportment of a gentleman is concerned, it is much more easy to be virtuous in comfortable circumstances than under the pressure of want. And even should fortune afterwards relent, so as to afford relief from early embarrassment, the baneful effect of its blighting influence may still appear in the habits acquired from it, which, I am sorry to say, occasionally render the practice of medicine more like the occupation of a needy tradesman than the exercise of a liberal profession. And here, gentlemen, if it was not for the fear of incurring disapprobation from some of the present audience, I would earnestly warn you against the most fruitful source of debt and difficulty, which is an early and imprudent marriage. You may be told that nothing can so much promote your respectability and professional success



as a wife ; but with all respect for matrimony as essential to the completion of your happiness, you may be assured that such an acquisition at the outset of life could hardly fail to prove an encumbrance of the most adverse and serious kind. For a good many years to come the whole of your attention should be devoted to professional pursuits ; so that, even if circumstances in other respects permitted such a step to be taken without impropriety, it would still be wrong to distract the mind from its proper objects, by burdening it with the cares and anxieties of a family. And if you were to venture upon a matrimonial partnership, without making due provision for the consequences, it is needless to describe the misery that might ensue. The claims which were expected to act as a stimulus to exertion, proving to be beyond their reach, so far from doing so, would prostrate your energies, and chill every honourable feeling. No means of procuring the necessities of life would then appear too mean or despicable ; and it is even possible that, without believing in the truth of some popular delusion, you might be led to adopt it for relief from the pangs of hunger, and thus becoming entangled in the snares of quackery, remain prisoners for life. Such things have happened, I am sorry to say, under my own observation ; and therefore I beg the more earnestly to warn you against them. It would be impossible at present to offer any advice appropriate to each of the various employments upon which you are about to enter, at home and abroad, in the public service, and in private practice. But whatever may happen to be the object of your wishes, let me impress upon you the importance of engaging in its pursuit with energy and resolution,—since there is nothing so opposed to success as indifference, and no more certain path to its attainment than the firm purpose of a determined will. When, therefore, any difficulty requires to be surmounted, instead of being satisfied with surveying it from a distance, or quietly sitting down to consider it at your leisure, you should give it the full occupation of your mind, thinking, talking, reading, and dreaming of it, until you either accomplish or find it impracticable. It would be no less curious than instructive, to ascertain the times and circumstances under which ideas, that have proved most conducive to the progress of art and science, occurred to their respective authors. Few of them, I suspect, would be dated from the study, with its books and papers ; and so many traced to the social board, the festive scene, the public conveyance, or the midnight watch, as to show how constantly their search had occupied the mind. The achievements usually ascribed to the efforts of what is called inventive genius, ought rather perhaps to be attributed to this power of concentrating the attention, without which talents of the brightest kind are of little practical utility, and with which those of the most ordinary quality may produce results no less creditable to their possessor than beneficial to society. It is the power of rapidly concentrating and justly comparing all the facts and ideas bearing on the case under treatment, that constitutes what is called a good practitioner. For this purpose it is necessary, in the first place, that you should possess the requisite information ; and secondly, that you should be guided in applying it by a sincere regard for accuracy. With the first of these qualifications we do not profess to have supplied you,—all that was within our reach being the preparation of your own powers to obtain it by careful and long-continued observation. You should now make every case that presents itself a subject of the most diligent study ; examining every symptom, watching every change, and trying the soundness of your principles by their application in practice. Every step thus taken will be sure to promote your progress in acquiring useful experience. And now, as the last and best advice which it will ever be in my power to offer you, I beg to recommend, that in all the difficulties which may beset your journey through life, whether in treating the maladies of your patients, or in steering through the intricacies of social and professional relations, you will constantly, earnestly, and honestly endeavour to take truth for your guide. By the course thus indicated, you may not always attain the object in view most quickly ; but will be more sure of doing so in the end, than by trying any of those

short roads to fortune, which are based on falsehood and deception; and if ever embroiled with those troublesome and unprincipled individuals who more or less infect every profession, may comfort yourselves with the reflection, that "Magna est veritas, et prevalebit." But, what is of more consequence than either professional success or social position, you will be sure of retaining that self-respect and peace of mind without which there can be no real happiness in this world. In the earnest hope that you may all be able to comply with this advice, and with every good wish on the part of my colleagues and myself, that health, success, and happiness may attend you in the various conditions which you are destined to occupy, I now bid you farewell. You may believe that the best reward of our past exertions will be your future success; and that you will always find us anxious to use every means in our power for the promotion of your welfare.

### FIRST REPORT OF THE MEDICO-STATISTICAL ASSOCIATION.

BY W. T. GAIRDNER, M.D., AND J. W. BEGBIE, M.D., SECRETARIES.

*(Read to the Association, July 31, 1852.)*

Number of returns,	75
Of these there are, of males,	41
of females,	34
Number of patients, single,	43
married,	25
widowed,	6
Condition not ascertained,	1
Number resident in Edinburgh at death,	68
in Leith,	6
elsewhere,	1
Ages of patients:—	
From birth to 15,	11
15 to 60,	51
60 upwards,	13
Average age of males,	41
of females,	31
Average age of total patients,	36
Of the 75, there died in the Royal Infirmary,	34
in Blind Asylum,	1
in Private houses,	40
Of the 40, there died in Dispensary practice,	4
in Private practice,	36
Of the 36, there died in the Old Town,	10
New Town,	17
Leith,	6
At a distance from Edinburgh or place of death not stated,	3
Position of patients:—	
Persons of rank, gentry, and professional,	10
Master tradesmen,	14
Journeymen tradesmen,	28
Labourers,	12
Destitute,	4
Not ascertained,	7

In comparing the returns of the Medico-Statistical Association with those of the Registrar-General, or with any of the hospital or other reports which have

been hitherto submitted to the public, it will be necessary constantly to keep in view the peculiarities in the system of registration adopted by the Association. The practitioners who have furnished the data on which this report is founded, have been in all cases invited to inscribe in the schedules, not, indeed, trivial details, but the fullest possible statements as to the more important morbid phenomena, and particularly the causes of death; and all of the phenomena so detailed have been separately registered (distinguishing between admitted causes of death and accidental symptoms, or morbid appearances). The schedules embracing the details of individual cases are, therefore, in no respect like the very brief and unsatisfactory data on which bills of mortality, and other registers of causes of death, have hitherto been furnished. They are rather to be viewed as a series of compendious, but authentic and interesting, medical histories,—in themselves, replete with materials for study and reflection, and rendered accessible for this purpose by the copious and complete index to be found in the register. The interest of these cases is, therefore, by no means exhausted in a general numerical statement of nosological details; on the contrary, they will form a series of records constantly accumulating, and open to consultation on special subjects of pathological inquiry,—thus enabling every member of the Association not only to recur to his own experience, but to call in aid of it that of his fellow-members, for the solution of questions which may arise to his mind in the course of practice, or the pursuit of scientific investigations.

The number of cases reported at present is too small to justify much numerical detail, or to lead to many secure conclusions as to the frequency of special diseases, or the relation of morbid phenomena to one another. It has been from the first the aim of the Association, not so much to add to the number of its records, as to secure well-weighed statements and careful observations; and they have, therefore, addressed themselves only to those who, though engaged in the laborious duties of medical practice, are yet the willing and able friends of scientific progress. The aggregate returns of the Association can thus never represent the entire mortality, either of every particular locality, or of any fixed circle of practice; they will necessarily be subject to considerable fluctuations, and can never, under any circumstances, be used without large qualifications for the numerical estimate of the endemic or epidemic prevalence of disease, the influence of season, etc. The sporadic forms of disease, on the contrary, which, from previous returns on a larger scale, and from the general experience of medical men, are known not to present sudden fluctuations to any considerable extent, may be expected to be subject to such laws in respect to their causation, combination, and phenomena, as shall be a legitimate subject of investigation in any series of miscellaneous cases, after due allowance has been made for accidental causes of disturbance of the results. And it is to these diseases, accordingly, and especially to the consideration of a few of their more frequent and well-recognised forms, that the present report is chiefly directed.

It must further be borne in mind, that the numerical proportion of individual causes of death to the whole number of deaths is, in the returns of the Medico-Statistical Association, generally much higher (*cæteris paribus*) than in the Registrar-General's returns, in which only one cause of death is stated in each case. Indeed, it is abundantly evident that, according to the latter system, many diseases, and some of these the most important to be known as influencing the mortality, must necessarily be lost sight of in order to allow of the registration in each case of the more palpable or more immediate, though possibly not more real or efficient, cause of the fatal catastrophe. In the returns of mortality hitherto furnished to the Association, the assigned causes of death are probably not less than six or seven times as numerous as the cases; that is to say, the morbid conditions bearing on the fatal event are in each case (on the average) stated at six or seven, instead of only one, as in most other returns. The effect of this mode of statement is, as will be seen immediately, not only to raise the apparent numerical frequency of almost all causes of death, but especially to

bring to light, and to place in a prominent position, some causes of death, well known to practitioners as of great frequency and importance, but apt to be overlooked by the general public, and all who draw their information in any degree from the ordinary returns of mortality.

In estimating the proportion of individual causes of death to the whole mortality, it has been considered most convenient to reduce the numbers representing this proportion to decimal fractions of unity, disregarding in the meantime, on account of the small aggregate of cases, all fractions and fractional differences less than  $\frac{1}{10}$ th. When the numbers dealt with become larger, and the second decimal place is employed, the per-centage in any given case will be found by removing the decimal point two places to the right: thus,  $\cdot 05$  of a decimal, or  $\frac{5}{100}$ th (unity being taken as a standard), represents 5 per cent.,  $\cdot 75$  or  $\frac{75}{100}$ ths represents 75 per cent., etc.

*Sources of the Returns.*—The schedules have been received from the following medical practitioners in Edinburgh and Leith:—

From Dr Alison.

Dr Andrew.

Dr Begbie.

Dr J. W. Begbie.

Mr Benjamin Bell.

Mr Bickersteth.

Dr Christison.

Dr Coldstream.

Dr Halliday Douglas.

Dr W. T. Gairdner.

Dr J. Gillespie.

Dr Gordon.

From Dr Keiller.

Dr R. Mackenzie.

Professor Miller.

Dr Moir.

Dr Murchison.

Dr J. Robertson.

Dr W. Robertson.

Mr Sidey.

Dr J. Sidey.

Dr Struthers.

Dr Thom.

### I. Cause of Death unknown or imperfectly understood.

There occurred four instances of unexpected and unexplained death. One of these occurred in the case of a young and generally healthy man, aged 21, under circumstances unexplained, either by the history of the symptoms or by a careful examination of the body. The details will be adverted to in a future report. The three remaining deaths are fairly referable, in some degree, to old age, possibly complicated with some affection of the heart or coronary arteries. The patients were respectively 76, 80, and 85 years of age; death was in all of them more or less sudden, apparently by syncope. No disease of the internal organs had been known to exist, and no dissection took place in any of the three cases. One of them (a female, aged 76) is stated to have been of very fat habit of body, and to have had for many years a cancerous growth on the forehead. She had likewise suffered many years ago in India severely from dysentery. She "had retired to bed in apparently even better health than usual, when she complained of a sudden pain in the precordium, with a feeling of suffocation, and shortly afterwards lapsed into a state of insensibility. She was seen by me (Dr James Gillespie) about ten minutes after the seizure, when I found her lying in a state of syncope, pulse quite imperceptible, extremities cold, face suffused with perspiration, very pallid, and breathing laborious. By the use of stimulants and rubefacients she was roused for a time to consciousness, complained of difficulty of breathing, and again gradually relapsed into her previous state. No abnormal sounds could be heard in the chest: but the heart's action and impulse were very feeble, and latterly very irregular." In this case it seems not improbable that there may have been some cerebral affection. Another (male, aged 80) died suddenly in bed,—having been subject to attacks of fainting on at least two previous occasions. After the first attack of fainting the heart was examined, and found to act very feebly; "first sound very weak; second, sharp and distinct. From that time up to death the state of the heart continued much the same; pulse weak, 80, and occasionally fluttering" (Dr Struthers). The third case of the above

series was a male aged 85. He had been for many years very corpulent, but enjoyed remarkably good health, was not confined till the day previous to his decease, and (although in a good position in society) was not seen by a medical man till after his decease. "On the afternoon preceding his death he complained of a sudden feeling of weight at the precordium, his pulse becoming very feeble, extremities cold, and a clammy perspiration appearing on the forehead. He retained his consciousness till within an hour of his decease, the respiration never becoming laboured, but diminishing in frequency, and he died so quietly that the persons present could not note the precise period when he breathed his last" (Dr James Gillespie).

*Death from Old Age.*—The termination of human life by pure senile exhaustion and decay, uncomplicated by any special functional or organic derangement, is undoubtedly a rare phenomenon. In<sup>1</sup> Dr Begbie's report of the mortality of the Scottish Widows' Fund and Life Assurance Society, only 6 out of 642 deaths, 38 of which were in persons upwards of 75 years of age, are returned as from old age,—being about 1 per cent. of the aggregate mortality, and 8 per cent. of the deaths beyond 75 years of age. In the annual returns of the Registrar-General the deaths from old age vary from 3.2 per cent. (in 1849) to 7.7 per cent. (in 1838); and it is further to be observed, that in all the earlier years the proportion is greatly in excess over that of the later annual returns; so that, from 1838 to 1851, the proportion of deaths returned as from old age presents, on the whole, a gradual decline to the extent of fully a half. The older bills of mortality for the city of London do not, on the whole, differ very materially from the higher of the two estimates given above. In twenty years of the seventeenth century (1629 to 1658) the deaths returned under the head of "Aged" were 15,757, the total deaths being 219,250; the proportion, therefore, is 7.1 per cent.<sup>2</sup> In the eighteenth century the bills of mortality show a proportion sometimes higher and sometimes lower, but nowhere so much reduced as in the latter years of the Registrar-General's returns.<sup>3</sup>

Taking these facts into account, and especially considering the small proportion of deaths from old age returned among the carefully selected lives of the assurance company above mentioned, it becomes obvious that the apparently high proportion borne by this cause of death in the ordinary returns of mortality is merely the result of inaccurate and random statements, and disappears whenever any series of deaths is submitted to more careful scrutiny. This conclusion is not without importance, both to the medical practitioner and the actuary; and Dr Christison has already drawn attention to it in commenting upon the returns of the Standard Assurance Office. The above facts amount to a demonstration, that a large number of deaths from old age, so far from being a cause of satisfaction in the returns of an assurance office, is in general to be looked upon merely as an indication of want of care or of skill on the part of those furnishing such information.

In the nosology adopted by the Medico-Statistical Association, it has not been considered advisable to assign a separate position to deaths from old age. In conformity with the general scheme of the Association, it is intended that the circumstances attending such deaths should be recorded in such a manner as to permit of their being otherwise classified. Deaths from pure exhaustion, without special symptoms or organic lesions, which may be considered as probably the only instances of the strictly natural termination of human life, will find their appropriate place under "Exhaustion or Debility" (IV. 12); while those in which any special phenomenon occur will be registered according to the nature of these phenomena. The diseases of advanced age, however, and the mode of termination of human life, when it has been protracted beyond its usual limit, form a subject well worthy the attention of the Association; and it is proposed

<sup>1</sup> Monthly Journal for 1847.

<sup>2</sup> Observations, etc., on the Bills of Mortality. By Capt. John Grant, Fellow of the Royal Society. Oxford, 1665. Fourth edition. Table opposite p. 172.

<sup>3</sup> Willan's Diseases of London. Miscellaneous Works, pp. 422 to 429.



in future reports to give very special attention to the circumstances of all deaths occurring beyond the age of 70 years. The members of the Association are, therefore, requested to hold this inquiry specially in view in making their returns of such cases of death, and, in particular, to seize every opportunity of communicating in some detail cases of death at advanced ages, where post-mortem examination has revealed the state of the different internal organs, or where careful examination has been made during life. Two cases besides those already mentioned occurred in the present series,—a male, aged 76, and a female, aged 81. In both of these cases there was such an amount of internal organic disease as fairly to remove them from the category of simple senile decay, although there can be little doubt that the mode of death was in both cases modified by that condition. The female patient died exhausted by vomiting, occurring along with symptoms of mitral valvular disease of the heart. The male patient (No. 68) had been very frequently salivated in early life on account of hepatic disease, and the liver, as well as the kidneys, were found after death much atrophied from old disease; the heart excessively flaccid, thin in its parietes, and its muscular fibre presenting everywhere considerable granular degeneration. This patient was long under medical treatment before his death, on account of dyspeptic symptoms. He lived on exceedingly small quantities of solid food, and had latterly taken large quantities of stimulants (chiefly brandy); and, whether in consequence of this peculiar regimen, or in spite of it, the fat, both external and internal, had accumulated to a great extent (being in some places upwards of an inch in thickness), while the muscles were much attenuated. The cardiac symptoms were remarkable in connection with the morbid appearances above mentioned. The patient had been subject to repeated faintings, but without any angina; and his death, which was probably hastened by the formation of several large boils in the epigastric region, was an example of syncope or of sudden prostration in an extremely debilitated subject. The heart presented no abnormal murmurs, but the pulse, which was always very soft, was seldom many beats above twenty in the minute, and was frequently counted as low as twenty (Dr Begbie), and even (on one occasion) nineteen in a minute (Mr Alexander). There was no dropsy, notwithstanding the old-standing disease of the liver and kidneys.

II. and III. These headings in the nosology, comprising Injury, Privation, Neglect, Accident, etc., and Surgical Operations, present nothing which it is worth while to dwell on in this report.

#### IV. *Constitutional or General Disease.*

The cases registered in this class amount to 59, or about  $\frac{1}{5}$ ths ( $\cdot 8$ ) of the whole number. This very large proportion depends on the great variety and importance of the pathological phenomena registered under this head, having nothing in common but the absence of definite local manifestation. Besides the "Diseases of Uncertain Seat," the class includes many of the "Zymotic Diseases," and the class of "Tubercular Diseases" in the nosology of the Registrar-General. The materials before the Association warrant a few remarks on the inflammatory and tubercular affections.

*Inflammation.*—Although not admitting of very precise definition, this term is extensively, and on the whole clearly, understood by practitioners, as indicating the acute and febrile types of disease, attended with much local and general disturbance of function. It has been thought proper, on various grounds, to place the "pyrexiae" in a group by themselves. Admitting into this category only the well marked and unequivocal examples of inflammation, they are found to number 27, or a little more than one-third of the deaths. This list comprises—

5 Cases of *Pleurisy*: 3 with tubercle of lung (1 also with Bright's disease), 1 with pneumonia, 1 with hydrothorax and disease of heart.

8 Cases of *Pneumonia*: 2 from purulent infection, with laryngeal ulceration, 1 after hooping-cough (broncho-pneumonia), 1 with pericarditis, disease of liver, and dropsy (no dissection), 1 with considerable pleurisy, 3 with marked bronchitis.

6 Cases of *Bronchitis*: 1 with typhus fever, 3 with pneumonia (1 after hoop-



ing cough) 1 with dropsy, diseased heart, and incipient disease of liver and kidney, 1 without distinct complication in a child of eighteen months, in whom a blister, applied without medical advice, appeared to hasten death by its severe effects.

1 Case of *Pericarditis*, with *pneumonia*, etc. (see pneumonia). Another case of well-marked asthenic pericarditis occurred in Bright's disease, with general dropsy of the serous cavities; but this presented no symptom of inflammation.

3 Cases of *Peritonitis*: 1 with dysentery, 1 from perforating tubercular ulcer, 1 from corroding ulcer of uterus, involving the peritoneum.

2 Cases of *Dysentery*: 1 with peritonitis, the other also with slight peritonitis, and secondary abscesses of liver.

2 Cases of *Enteritis*: 1 from strangulated hernia; 1 apparently spontaneous (gangrene of an appendix to the small intestine).

2 Cases of *Meningitis*: 1 after scarlatina in a child of seven years, 1 tubercular in a child of three years. .

1 Case of *Acute Rheumatism*, fatal by exhaustion from uncontrollable bed-sores, with incipient purulent infection (multiple abscesses of lungs).

1 Case of *Croup*; apparently uncomplicated (child of three years).

1 Case of *Carbuncle*, fatal by typhus fever, caught in the hospital ward at commencement of convalescence.

1 Case of external inflammation from *scald*.

From the above synopsis of the cases of acute inflammation, it will be seen how very small a proportion of these is composed of uncomplicated cases; and it is to be observed, that no complications are here mentioned which were not of sufficient severity and importance to be distinctly recognised as collateral causes of death. Hence it is obvious, that in the ordinary returns of mortality a vast number of acute affections must be passed over in the recording of what is arbitrarily termed the principal or primary disease; and, on the other hand, numerous important chronic diseases must either be merged in the acute affections of which they are really the causes, or must render the returns of the latter extremely imperfect. In no case is this more evident than in the acute pulmonary affections. In the Registrar-General's annual returns, from 1847 to 1851, excluding 1849 (the cholera year) the sum of deaths from pneumonia, pleurisy, and bronchitis, varied from a little below  $\frac{1}{4}$ th to somewhat above  $\frac{1}{4}$ th of the total mortality. If, in the above list, the sum of cases of the same diseases be taken (15), and the cases of chronic bronchitis, etc., be added (about 6 or 7), the proportion of these affections to the whole mortality will approach  $\frac{1}{4}$ d. If, on the contrary, the cases be rigidly excluded in which the chest affection was not the principal or primary disease, the proportion is reduced much below that of the Registrar-General, probably to about  $\frac{1}{12}$ th, or even lower, had all the cases been completely investigated; for the records of post-mortem examinations in hospitals show that fatal inflammations of the respiratory organs are, in the great majority of cases, complicated with chronic affections. It will be at once seen that the difference between these two estimates of proportion is made by a series of cases, which, in the Registrar-General's returns, might have been placed (almost at random) under the heads of pleurisy, pneumonia, bronchitis, rheumatism, typhus fever, hooping-cough, phthisis, pericarditis, dropsy, and disease of heart.

*Tubercular Disease.*—In the Registrar-General's return, scrofula, tabes mesenterica, hydrocephalus, and phthisis, form the order of tubercular diseases, and constitute from one-fifth to one-sixth of the annual mortality in ordinary years. The first three are emphatically diseases of the infantile period; and as children have hitherto formed a small proportion in the returns of the Medico-Statistical Association, a very large relative number of the tubercular deaths is, as might be expected, registered under the head of phthisis pulmonalis, almost to the exclusion of the other diseases above mentioned. The whole order of tubercular diseases gives a mortality of twenty cases, or somewhat more than one-fourth of the aggregate mortality, the deaths from phthisis being seventeen in number. It thus appears that tubercular diseases, and in particular, phthisis pulmonalis, have, in the returns now submitted to the Medico-Statistical Association, a consider-

ably more prominent position as to frequency than in the general mortality lists. It would be premature to attempt to determine at present whether this discrepancy is apparent or real; for it may be fairly presumed, that, in the ordinary method of registration, a considerable proportion of tubercular and phthisical cases disappear from the returns by being merged in other diseases of apparently predominating importance. By way of vindicating the accuracy, however, of the facts on which the above numerical statement is founded, and with a view, at the same time, to illustrate the natural history of a most important order of diseases, the following details are extracted from the returns made under the head of tubercular disease.

Of the twenty cases of death from tubercular disease, there were three (as above mentioned) which were not included in the register under the head of phthisis pulmonalis. One of these was a case of tubercular meningitis, in a child of three years, who had been affected a year before death with scrofulous ophthalmia, which recurred two months before death, succeeding an eczematous eruption of the scalp and face. The fatal symptoms, which in most points resembled those of acute hydrocephalus, were of eight days' duration; and there was found after death lymph at the base of the brain, with slight tuberculisation of the pia mater. There were no tubercles in the lungs, which presented numerous portions of lobular atelectasis or collapse, without trace of inflammation; mesenteric glands slightly and generally enlarged; epithelium both of liver and kidney much loaded with oil granules (Dr Struthers). The second case of tubercular disease (not phthisical) was that of a woman, aged 27 (No. 27), who died of coma following hemiplegia, of three and a half months' duration, from a tubercular mass in the right crus cerebri; there were likewise found tubercles of the lungs (very slight), and tubercular ulcers of the intestines, with considerable chronic peritoneal adhesions, including tubercles; and to these lesions none of the symptoms observed during the fatal illness can be referred. A third case presented a very singular, if not a unique, variety of tubercular disease, of which the details will be found in the Appendix (No. 46). In a boy of 12 years, an attack of chronic tubercular peritonitis, with tubercle of the mesenteric glands, but not of the mucous membrane, resolved itself into a foecal fistula, by opening on the one hand externally, and on the other into the ileum (No. 44, see Appendix).

*Phthisis Pulmonalis.*—Among the seventeen cases of phthisis pulmonalis, the great majority pursued the ordinary chronic course, ending in emaciation and exhaustion, generally with more or less diarrhoea and hectic fever, and occasionally other complications. Hectic fever was well marked in nine cases, diarrhoea in ten cases, having in one or perhaps two of these a somewhat dysenteric character. In all the cases of well-marked diarrhoea which were examined post-mortem (with one exception, to be afterwards mentioned), there were found tubercular ulcers of the intestines; on the other hand, these ulcers were found in one case (No. 9) in an exaggerated form, notwithstanding the ascertained absence of diarrhoea throughout the disease, which, in this case, terminated by peritonitis from perforation (Dr J. W. Begbie). In yet another case (No. 58), the cicatrices of extensive ulceration of the intestine existed; but the ulcers were entirely healed. There was no diarrhoea, and very inconsiderable emaciation. There were, however, nocturnal perspirations. Dyspnoea was a very marked symptom, and greatly implicated in causing death, which was perhaps scarcely if at all hastened by a profuse hæmoptysis, occurring six weeks before the fatal event. In strict conformity with these symptoms, the lungs were found to present much emphysema, and the tubercles were mostly obsolete or retrograde; the cavities numerous, small, contracted, and scattered. The patient, a man of 41 years of age, had attempted suicide by cutting his throat about eight years before death. The fatal disease had run a course of several years, and was evidently in progress of cure, so far as the tubercular affection was concerned; but the injury sustained by the respiratory organs had been too great to permit of a protracted life. The emphysema was probably in part chronic, and partly due to a recent attack of bronchitis, the evidences of which were found after death. The hæmoptysis too

may have been caused by the violent respiratory efforts made under these circumstances. The case is every way remarkable, but chiefly as showing the extent to which phthisis may proceed, the patient escaping from all its ordinary dangers, though with permanently injured organs; and after a partial cure, becoming the victim of another form of pulmonary disease.

Pleuritic effusion is mentioned as a complication of phthisis in three cases, pneumonia in none, evidently because the relations of this disease to tubercle prevent its being recognised and recorded as a separate affection. Hæmoptysis to a marked extent occurred in five cases, but did not prove directly fatal in any. Laryngeal affections are recorded (not as a cause of death) in three cases. Bronchitis occurred as a considerable and important complication in two cases, one of which has been detailed in the last paragraph.

Vomiting is mentioned as a serious complication in two cases. Very extensive aphthæ contributed to the fatal issue in one case (a woman, aged 28). Hemorrhage from the intestines was observed in one case of acute phthisis (see below) shortly before death.

Delirium is recorded in three cases. In one case of acute phthisis there were a few small tubercles of the cerebral hemispheres, not affecting the membranes, but imbedded in the nervous tissue. In another case, likewise very rapid in its course, no apparent tubercular lesion of the brain was found on dissection, but three or four ounces of limpid fluid in the ventricles (Mr B. Bell). In this case (male, aged 45) "the chest symptoms had ceased to trouble him ten days before death. The cough and expectoration were gone; but he had become restless and sleepless, and complained of pain in his head and complete retention of urine; the urine high coloured, not affected by heat or nitric acid; he required the catheter until his death. The pulse for a few days calm and rather slow; it then became accelerated; he was confused, and like a person in advanced typhus; the respiration became oppressed, and he died rather rapidly." The tubercle of the lungs was recent and extensive, chiefly crude, but partially softened; there were a few tubercles in both kidneys; duration of the whole affection three months. In the third case delirium was an occasional symptom in an ordinary case of chronic phthisis. There was no post-mortem examination.

In one case (No. 21) phthisis pulmonalis was complicated with diabetes, which had probably been the original affection. In one case (No. 50) it was immediately preceded by hooping-cough, caught during the convalescence from scarlatina. In one case (No. 55) it was accompanied by Bright's disease.

In one instance (No. 56) very obstinate vomiting and diarrhoea, with some pain of abdomen and pain in micturition, are probably to be referred to tubercular abscesses of the kidneys, with inflammation of the pelvis and ureters. The intestines in this case were quite free from disease.

In another case, which will be found in the Appendix (No. 46), the disease ran a rapid course, apparently of not more than a few weeks, in a woman, aged 34, the symptoms being marked febrile oppression, vomiting, diarrhoea, and latterly a typhoid condition, very similar to that of typhoid fever, the resemblance to which disease was increased by abdominal tenderness, and by the discharge of some ounces of blood from the bowels. Tubercles were found in great abundance in the lungs, in most of the glands, and in the substance of the brain, as well as on all the thoracic and abdominal serous membranes.

To sum up the preceding details, it may be said, that out of seventeen cases of tubercular phthisis, thirteen followed more or less closely the ordinary course of that disease to the close; two were remarkable for rapid course, and for a termination by typhoid delirium, masking the pulmonary symptoms; one terminated by perforation of the intestine and peritonitis; and one by emphysema and bronchitis, after exhaustion of the tubercular diathesis.

V.—*Disease of Nervous System.*

VI. ... *Circulation.*

VII. ... *Respiration.*

VIII.—*Disease of Digestion.*IX. ... *Secretion and Excretion.*X. ... *Generation, Pregnancy, and Childbirth.*XI. ... *Bones and Joints.*XII. ... *Integuments.*

In these local classifications there is so much which differs from the system adopted by the Registrar-General, as to render a material discrepancy of numerical results unavoidable. The exclusion, in that system, of diarrhoea and dysentery from the diseases of the digestive organs, of phthisis pulmonalis from those of the respiratory system, of hydrocephalus from the nervous system, of scrofula and rheumatism from the diseases of bones and joints, will always to a great degree prevent its most general results from being harmonised with those of the Medico-Statistical Association. Nor does it appear, on the whole, that the comparison could ever be of much importance, even were the two systems of classification more similar; for the differences in the method of registration are more than sufficient, when acting over so wide a field as is presented in any of the above orders, to preclude all rational comparison.

With respect to the special diseases included under these orders, some of the more frequent have already been mentioned under the heads of inflammation and tubercular disease. Many others it is considered advisable to overlook in the present report, as their numbers are still too small for profitable consideration. A few of them, however, deserve a moment's consideration.

*Diarrhoea.*—The cases of diarrhoea as a cause of death amount, in the returns of the Medico-Statistical Association, to nineteen, or very nearly one-fourth of the whole number. In this list the cases of diarrhoea from phthisis or other organic affections are included. The Registrar-General's returns profess to account for those cases of diarrhoea only in which it is a primary or idiopathic disease; and that it is considered in this view a disease of great importance, may be inferred from the classification, which removes it from its natural place in the nosology, and places it among the diseases produced by zymotic causes, and subject to epidemic laws. Diarrhoea so classified in 1851, numbers 2271 victims out of 54,966 deaths from known causes, or about one-twenty-fourth of the whole number; and this is exclusive of dysentery, enteritis, and cholera, which have all separate places in the returns.

The experience of many careful and well-informed practitioners will detect in this principle of classification, and in these numbers, an enormous amount of probable fallacy. When it is presumed that the idiopathic or essential varieties of diarrhoea are separated from the forms which are the result of organic or other disease, on what principle can even the most moderate amount of accuracy be expected? A large proportion of cases of fatal diarrhoea are ascribed to the infantile period. Can it be supposed that infantile phthisis, which so often terminates by the abdominal complications, almost to the exclusion of the pulmonary affection, is in any reasonable measure separated from these cases? And at more advanced periods of life, is there any probability, *a priori*, that the instances of diarrhoea from Bright's disease, or even from phthisis, are excluded from the returns of simple diarrhoea?

The returns of the Registrar-General are in themselves sufficient to show the fluctuating character of the medical opinion by which the separation of diarrhoea as a primary and epidemic form of disease, professes to be guided. During the fourteen years for which returns have been published, the cases of idiopathic diarrhoea have apparently multiplied in England more than six-fold, and this with a regular and graduated increase, which precludes the idea of epidemic influences. The extremes of this graduated scale are as follows:—In 1838, 393 cases of diarrhoea; in 1851, 2271 cases. The whole return shows, in a manner not to be mistaken, that diarrhoea, as an independent disease, owes its apparent numbers far more to the state of medical opinion, and the mode of classification adopted by the Registrar-General, than to any other cause.

The nature and prevalence of diarrhoea, therefore, whether as an independent primary disease, or as a secondary affection, can only be exhibited with an approach to correctness by a careful series of returns on the principle adopted by this Association. Indeed there is no reason to suppose, nor does ordinary medical experience bear out the idea, that the secondary forms of diarrhoea are less prone than the primary, to be influenced by periodical or epidemic predisposing causes. The cases hitherto recorded are too limited in numbers to throw much light on this question; nor is the season one in which diarrhoea has been unusually prevalent; but an analysis of the nineteen cases will serve the purpose of showing the small proportion of cases in which the affection can justly be pronounced an idiopathic disease.

Among eighteen cases of diarrhoea recorded, eleven were certainly connected with tubercular disease,—viz., ten from phthisis, as already mentioned above; and one in consequence of perforation of the intestine from without, in a case formerly alluded to, and detailed in the Appendix (No. 44); one other case of diarrhoea was in all probability tubercular, occurring as it did in a child of 16 months, affected with marasmus, and dying comatose. In three cases diarrhoea was apparently due to Bright's disease, once in combination with phthisis; in one case it was in connection with ascertained organic disease of the pancreas, and was superseded by constipation some time before death (No. 17). Two cases were from dysenteric ulceration of the colon; in one with abscess of the liver, ascertained by dissection (No. 23); in the other, combined with acute peritonitis. In one instance diarrhoea occurred in the fatal termination of a case of acute rheumatism, along with very large and foul sloughs on the sacrum, and with distinct evidences of purulent infection of the blood (No. 42). In the only remaining case (No. 4), the diarrhoea was probably the result of perforation of the intestine, arising from a foetid accumulation of pus in the peritoneal cavity from a corroding ulcer of the uterus.

It thus appears that, in the whole series of seventy-five cases of miscellaneous disease, nineteen of these cases being affected with diarrhoea to a marked degree, *there has hitherto been no one unequivocal instance of fatal idiopathic diarrhoea*; and the only one which could, with any propriety, have been registered as such, according to the information received, was a case of infantile diarrhoea with marasmus, very probably of tubercular origin, though not distinctly ascertained to be so.

*Dropsy.*—This affection occupies a most prominent position in the returns of the Registrar-General; and in the returns of the Medico-Statistical Association, it is registered in about one-seventh of the whole cases, although it was not probably a distinct cause of death in that number. The fluctuations in the numbers over a series of years, which was above remarked in the case of diarrhoea, are nearly as considerable in that of dropsy, but in the reverse order; for whereas the apparent frequency of the former affection has been on the increase, the numbers of dropsical cases is less by nearly two-thirds in the latter than in the earlier returns of the Registrar-General. The dependence of this fact upon the increased amount of recognition of the organic conditions on which dropsy generally depends, is so obvious to every one as to require no illustration.

*Diseases of the Heart and Kidney.*—It is intended, in a future return, to analyse these forms of disease with some care. In the meantime it may be remarked, that nowhere is the imperfection of the ordinary returns of mortality more clearly displayed than in the absurdly small numbers assigned to these affections, and particularly the latter. That diseases of such unquestionable importance as causes of death should be recognised as such in the returns of the Medico-Statistical Association in the respective proportions of one-seventh and one-ninth of the whole cases; while, in the returns of the Registrar-General, they are represented by the diminutive proportion indicated by the following numbers, may well excite surprise and distrust of the principles, as well as of the accuracy of the details, upon which these returns are founded.



In 1850, out of 48,579 deaths from all causes, only 1754 are stated to have resulted from disease of the heart, and 417 from disease of the kidneys. In 1851, the total mortality being 55,354, disease of the heart is represented by 1955, and that of the kidneys by 417.

Making all reasonable allowance for the possible errors arising from small numbers in the case of the returns of the Medico-Statistical Association, it is quite evident that an enormous proportion of the diseases of heart and kidney, in the national registers, are placed under the secondary affections; and that the virtual omission from these registers of two of the most frequent and most fatal classes of disease that affect humanity, is thus due to the arbitrary system which requires the selection of one cause of death in each case to the exclusion of others. That the general experience of the medical profession on this point is opposed to the inferences that might be drawn from the returns of the Registrar-General, is well known; but the extent to which the general mortality can be ascertained to be influenced by cardiac and renal organic diseases, and the manner and degree to which these are complicated with other affections in producing death, are subjects on which, notwithstanding multiplied investigations on these diseases of late years, much still requires to be done. The importance of these inquiries to medical men, and to insurance companies, would seem to give them a high claim on the attention of the Medico-Statistical Association; and it is hoped that succeeding reports may furnish contributions of some value to their solution.

APPENDIX, INCLUDING THREE ILLUSTRATIVE CASES SELECTED FROM THE SCHEDULES RETURNED DURING THE FIRST QUARTER.

**CASE 21.**—*Diabetes; Phthisis Pulmonalis; Syphilis (?) ; Œdema of Lower Extremities; Death by Exhaustion, with Emaciation.*—[Reported by Dr W. Robertson.]

Patient, a female, æt. 42; had probably suffered from syphilis of old standing; diabetes had existed for upwards of two years; phthisis for a somewhat shorter period. Fourteen days before death an eruption of purpura covered the whole body. Some days before death the sugar disappeared from the urine.

*Dissection.*—Tubercle and cavities in lungs; old adhesions of liver, spleen, and diaphragm; complete atrophy of left kidney; pancreas very small; almost complete obliteration of ascending vena cava; brain normal.

**CASE 44.**—*Chronic Peritonitis with Effusion; Ulceration of Umbilicus, and Discharge of Feculent Matter at Opening there; Subsidence of Abdominal Swelling; Rapid Emaciation; Diarrhœa; Death by Exhaustion.*—[Reported by Dr Gordon—Dissection by Dr Howden.]

Patient, a boy, æt. 12, was at school, though in weak health, for a very considerable period before his fatal illness. Three months before death, symptoms of chronic peritonitis with effusion were manifested; the fœcal abscess was established a few weeks thereafter, and three weeks before death diarrhœa and emaciation commenced.

*Dissection.*—Over the umbilicus an opening capable of admitting a filbert, its edges quite free and sharp; through it a probe passed quite freely into the peritoneal cavity. On opening the abdomen, the surface of the intestines presented a raw appearance and red colour, while over many parts there were masses of feculent lymph. The lower pelvic cavity contained a quantity of feculent matter mixed with lymph. An opening, capable of admitting a crow quill, existed between the umbilicus and the ileum; another and larger opening was found two feet from the caput cœcum, the edges everted, and of a dark purple colour. Numerous tubercular spots were detected under the peritoneal coat of the intestines; mesenteric glands enlarged; liver, dark and congested, contained numerous small tubercular depositions; mucous coat of intestines and other abdominal organs normal.



*Thorax.*—Tubercle in all stages in both lungs, which were generally adherent; small tubercular cavity in lower and posterior part of middle lobe of right lung; bronchial glands much enlarged.

**CASE 46.**—*Acute Deposition of Tubercle in Lungs, Pericardium, Pleuræ, Peritoneum, etc., simulating Fever; Death by Asthenia with Typhoid Oppression.*—[Reported by Dr Andrew, Dr Gairdner, and Dr W. Clarke.]

Patient, a female, æt. 34; for about a year intellect had given way, and for same period had had indifferent health, with diminished strength, and tendency to reject food. For some weeks febrile symptoms, with gastric irritation and tenderness on pressure over abdomen, much vomiting, occasional diarrhoea, great prostration; skin usually dry; pulse at first hard, afterwards quick, weak, and irregular; slight cough with purulent expectoration (in no great quantity); no marked dyspnoea. Two days before death some ounces of blood passed from bowels. Died exhausted, after typhomania, not very apparent, had continued for some days.

*Dissection*—by Dr Gairdner.—Tubercle mostly yellow miliary, very extensively deposited in lungs; tubercular adhesions of pericardium, pleuræ, and of the peritoneum, between diaphragm and spleen; incipient tubercle of liver, spleen, and kidneys; tubercle in mesenteric glands; three small tubercular deposits in brain, two in white matter of hemispheres, one in left crus cerebri.

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## Part Fourth.

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### PERISCOPE.

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#### MEDICAL JURISPRUDENCE AND TOXICOLOGY.

##### INFANTICIDE—PLEA OF PUERPERAL INSANITY.

**NORTHERN CIRCUIT.**—*Newcastle, Feb. 27.*—*Crown Court; before Mr Justice Cresswell.*—Hannah Ridley, aged 28, was indicted for having, at Allendale, on the 10th of December last, wilfully murdered her new-born female child.

Henry Philipson stated, that he engaged the prisoner as his servant, in May 1851, for half a year. She remained with him till the 10th of December. About the end of October witness told her he had heard she was pregnant, and she said it was not the case. On Monday, the 8th of December, witness left home, and remained away till six o'clock on Tuesday morning, when he returned. On reaching the house he met Margaret Davison, his present housekeeper, and from something she said he went away again to feed some cattle. He returned again at nine o'clock, and found that the prisoner had been delivered of a child. Witness knew William Pruddah, a chemist, at Hexham. He bought threepence worth of arsenic from him, in the beginning of the year 1851, for the purpose of destroying rats. The arsenic was wrapped in a paper, which had the words "Arsenic—poison" upon it. He took it home, and used more than half of it. He mixed what he used with grease. He ordered his housekeeper, Hannah Green, to destroy the rest, and did not afterwards see it, or say anything about it. Cross-examined.—Prisoner was a good, kind-hearted woman, while in his service. She was subject to great depression of spirits, and often in "deep

studies." Hannah Green is dead. The arsenic was wrapped in whitey-brown paper.—Margaret Davison said, that she was the last witness's present housekeeper. He engaged her on the 11th of November of last year, and she went on the 18th. The prisoner was then in the house, acting as housekeeper. She observed that she was in the family-way. On the morning of the 9th of December, witness was called up at five o'clock. The prisoner had been sleeping with her that night, but when Michael Stobbs called her the prisoner was not in bed. Witness went in search of her, and found her by the bedside, in her master's bedroom, and in labour. Witness asked if she was in labour, and prisoner said it was not her time. The doctor and Mary Anne Philipson came. The child was born shortly afterwards. It appeared to be a fine healthy child. On the morning of the 10th the child was sick, and Mrs Philipson gave it some milk. The child got worse, and died that evening. Mrs Philipson came before the child died. It was a girl. Mr Hewitson, surgeon, was there after the child died. He spoke to the prisoner, and gave witness a cup, in which was some milk and water and sugar, to take down stairs. The prisoner appeared distressed when the child died. Directly after the child was born, witness lifted prisoner's pocket, and two small whitey-brown paper parcels dropped out. The papers seemed much soiled by being carried in the pocket. When the surgeon came after the child was dead, he went up stairs a second time, and witness accompanied him. He asked prisoner if she had given the child anything, and she replied that she would rather beg with it. Mr Hewitson said, that if she had given it nothing she would not be against having her pocket searched; prisoner held out her pocket, and he searched and found one loose paper in it. It was the same kind of paper as that which witness had seen fall from prisoner's pocket. Mary Anne Philipson was present at the time, and shortly afterwards asked the prisoner if she had given the child anything, as the doctor was going to open the stomach, and analyse it. Prisoner seemed much distressed, and put a handkerchief over her face. After the child was born, the prisoner said it was a comely child, and said, "God bless it."—Mary Anne Philipson said, she had known the prisoner since May last. She was sent for to Mr Philipson's, on the morning of the 9th of December, and on getting there found the prisoner in labour. There were no clothes to dress the child with, and witness went to her own house to get some. She went again the next morning, and found the child vomiting. In the evening the child died, and after its death witness went up stairs with Margaret Davison, and asked the prisoner if she had given the child anything. In answer the prisoner said she had given it a little arsenic. She said she got it from a shelf. Before she said this, she put her handkerchief over her face, and appeared distressed. She had not sufficient milk for the child. Witness went to clean the house before the prisoner went to Mr Philipson's, and found a small brown parcel on a shelf in the kitchen. There was some writing on it, which Jane Stobbs read. The parcel was left upon the same shelf. There was also a plate with some grease upon it. Mr Philipson afterwards made some inquiry about this plate, and it was taken away and destroyed. Cross-examined.—The prisoner had no milk for the child. She often seemed low-spirited, and in deep study. After she was confined, she asked witness to get her some flannel, and said she wished she had got home, for she had plenty there.—Margaret Davison, being recalled, said that she heard the prisoner say she had given arsenic to the child, and that the arsenic was in her pocket, mixed with sugar. She said that she had put it into a spoon, and then given it to the child.—Jane Stobbs, a niece of Mr Philipson, said that in May last year she was helping to clean Mr Philipson's house. Mary Anne Philipson and Mary Stokoe were there. She saw the plate of grease, which was taken away by Mary Stokoe.—Mary Stokoe deposed, that when at Mr Philipson's in May last she received a plate of grease from the last witness, and threw it down the burn.—Mr Hewitson, surgeon, said, that on the 9th of December he went to Mr Philipson's, and assisted at the birth of the prisoner's child. It was a stout healthy child. In the evening of the next day he went to the house, and found that the child was dead. He examined the body,

and charged the prisoner with having given it something, but she denied it. He asked to search her pocket, and she said he was quite at liberty to do so. In the pocket he found a piece of whitey-brown paper, and in it were what appeared to him to be crystals of sugar. He asked the prisoner what the paper contained, and she said it contained sugar. He then went down stairs, and directed that the body should not be interred until the coroner had been communicated with. He went up stairs after Mary Anne Philipson came down, and asked the prisoner what caused her to do such a thing, to which she made no reply. He asked her how much arsenic she gave the child. She replied that she gave it "a pinch." He made a post-mortem examination of the body on the 12th of December, and sent the stomach and bowels and their contents to Dr Glover, in three separate jars. The prisoner always appeared perfectly rational. Cross-examined.—When women have suppressed milk it has occurred that they have had puerperal mania. Such an attack frequently causes them to have a desire to kill their own offspring. Fits of delirium sometimes come on when they are in that state, and last only a few hours. A person of low desponding habits would be more likely to have delirium than any other. If the woman was in fear after confinement when left alone, that might produce delirium. Re-examined.—The prisoner's case appeared to me one of want of fluid, rather than of suppressed milk.—Charles Christopher Hewitson, brother of the last witness, said he visited the prisoner after the birth of the child, but never saw any symptoms of puerperal mania or insanity.—Dr Glover deposed, that he had had very considerable experience in making analyses. He received from Mr Hewitson some jars containing the stomach and intestines of a child, and also a piece of paper. The intestines exhibited marks of extensive inflammation. Witness described the appearances and the process to which he subjected the stomach and intestines, the result of which was the detection of arsenic. He also subjected the piece of paper to a test, but the indications were so slight that he could not speak decidedly as to there being any arsenic upon it. The result of the analysis was, that he found a sufficient quantity of arsenic in the child to cause death.

Mr Overend, in addressing the jury for the prisoner, said that he did not pretend to disguise from himself the fact, that there were circumstances of very considerable suspicion affecting the prisoner. A great deal of evidence had been brought forward, not to show that the prisoner gave poison to the child, but to show that there was something antecedent to the act to make them believe that she designed the murder. The first thing suggested was, that she ought to have left her master's house early in November, whereas she lingered about; but he thought that was satisfactorily explained by the fact of her master asking her to stay, and the carrier being unable to take away her box. The next thing was, that she had prepared no linen for her child; but the defence was, that she was taken in labour before she anticipated it, and having had a child previously, there was linen which had been used for that, remaining with her parents, and therefore there was no occasion to provide for more. There was nothing to show that she behaved unkindly to the child; but that, on the contrary, she had manifested the greatest tenderness. That poison was found in the child he did not deny, but the question was—Who administered it? If the prisoner did administer it, under what circumstances was it done? Did she do it by mistake, by design, or under the influence of delirium common to women after confinement, when she was not accountable for her own actions? He did not think it possible that she could have made a mistake, and as she had not suggested that, he did not think he could urge it. She was a person of low desponding habits; and after witnesses had spoken to her character and manners, he thought they would see that the most reasonable mode of solving this difficulty would be to suppose, that if she administered the poison it was at a time when she was not accountable for her actions.—Witnesses were then called to speak to the prisoner's character and habits, all of whom said she was frequently in a low state of mind.—William Montgomery, surgeon, was then called, and said that he attended Hannah Ridley in August 1847, four or five days after her first confinement. She was then suf-

fering from puerperal insanity, which lasted for a fortnight. It is a disease women are sometimes subject to after confinement, caused by a suppression of milk. Having suffered from it at her first confinement, she would be more disposed to it than another at a second confinement. Being left alone, or being in a desponding state, would dispose to it. It might not last more than twelve hours, or perhaps less, though he could not speak to it from experience. Persons thus suffering have been known to have a tendency to destroy their offspring.—Dr Dawson, of Newcastle, said there was such a disease as puerperal mania, which developed itself in various ways. It was met with in women of low melancholy temperament, and was preceded by lowness of spirits. In some cases it manifested itself in expressing dislike to friends, and very often in a tendency to destroy their offspring. It might pass off in a very short time, perhaps in one or two hours. Insanity to the extent of taking away life might exist for that time, and then pass away. If poison was in the house, and a person was in that state of mind, and she knew it was in the house, she might be as likely to make use of that as anything else.—His Lordship, in summing up, told the jury that they must first be satisfied whether the prisoner administered the poison to her child wilfully, and if they were, then unquestionably she was guilty of murder, unless it was made out to their satisfaction that she was not at the time what is called a responsible agent. Every person who wilfully causes the death of another without some excuse was guilty of murder, and in this case, if the prisoner was guilty, he could not point out any circumstances making it less than murder, unless they were of opinion that she was not a responsible agent. He would read the opinion of Lord Chief Justice Tindal:—"A jury ought to be told in all cases that every man must be presumed to be sane, and to possess a sufficient degree of reason to be responsible for his crimes, till the contrary be proved to their satisfaction; and to establish a defence on the ground of insanity, it must be clearly proved that at the time of committing the offence the party was labouring under such defect of reason, from disease of the mind, as not to know the nature and guilt of the act he was doing, or not to know that he was doing what was wrong." Therefore, if they were satisfied that the prisoner wilfully destroyed the child, they must presume that she was competent to know what she was doing, and a responsible agent, until the contrary was proved on her behalf. If it was made out on her behalf to their satisfaction, that she was in such a state as to be incapable of understanding the nature of the act, she would be entitled to an acquittal. His Lordship then read the whole of the evidence, and at the close, remarked that he was bound to tell them that there was undoubtedly no direct proof that the prisoner was otherwise than in her perfect senses, as no person saw her labouring under delusion or insanity.—The jury retired for some time, and then returned a verdict of *Not Guilty*. His Lordship.—Do you find her insane? The Foreman.—We do, my Lord. His Lordship then ordered the prisoner to be detained during her Majesty's pleasure.—*Legal Examiner and Journal of Medical Jurisprudence*, March 6th, 1852.

#### MURDER BY STRANGULATION—LONGITUDINAL RUPTURE OF TRACHEA.

MIDLAND CIRCUIT.—*Northampton, Feb. 27.—Crown Court; before Jervis, L.C.J.*—Elizabeth Pinckard, aged 51, was indicted for the wilful murder of Elizabeth Pinckard, her mother-in-law. The prisoner is the wife of a farmer residing at Thrupp, near Daventry, in this county, and the deceased was the second wife of the prisoner's father-in-law. The deceased and her husband lived in a cottage on the Daventry road, about three-quarters of a mile from the house in which the prisoner and her husband lived; and it appeared that early in the morning of the 3d October last, the two Pinckards, father and son, went together to Daventry fair, leaving their wives at home. About half-past five o'clock in the afternoon of the same day, a man of the name of Bird was passing the cottage of the deceased, and observing the door open, went in. His evidence was extremely important in the case, because he was the first person who examined carefully the position in which the body of the deceased was found after death. When

he went in, he found the body of the deceased stiff and cold. It was sitting on the floor (in a corner), with the back against the front wall of the cottage, the left side against a corner cupboard, and tape round the neck. The other end of the tape was fastened to a hook in the cupboard, and the head was inclined away from the cupboard, so that the tape was on the stretch; but the loop round the neck of the deceased was so loose, that Bird could easily insert his fingers between it and the neck. Her legs were stretched out on the floor perfectly straight; her hands were lying in front of her, also perfectly straight; and her clothes were down over her legs, not at all disordered. Bird easily broke the tape with his hand, but when he had done so, the body remained still in the same position. It was, however, removed before the post-mortem examination took place; and upon that examination a contused wound was discovered upon the right eyebrow; and under the skin on the upper part of the head, on the right side, there were a few spots of extravasated blood, which might have been occasioned by slight blows. A spot of blood was found on the wall above the head of the deceased, and one or two on the floor. The face was swollen and discoloured. There was a mark round the neck as if produced by the tape; but the remarkable feature of the case was, that the trachea or windpipe was ruptured longitudinally on the right side,—that is, the rupture extending up and down the trachea. This being the condition in which the deceased was found, the impression at first was, that she had committed suicide; and upon the part of the prisoner, the same suggestion was still made.

Dr Alfred Taylor, the lecturer on medical jurisprudence at Guy's Hospital, was examined upon this subject, and stated that, in his opinion, the facts proved were inconsistent with the supposition that the deceased committed suicide. The rupture of the trachea longitudinally was a most extraordinary circumstance. He had never known an instance of it. It was impossible that the tape round the neck of the deceased could have done it. He had examined the tracheas of five persons who had died under the hands of the public executioner, and in no instance was the trachea ruptured longitudinally. He could only conceive such a rupture being produced by the application of lateral force compressing the whole tube. If a ligature was drawn tightly round the neck, and a stick introduced to act as a lever, the trachea might probably in that way be lacerated longitudinally. Dr Taylor also stated that the effect of strangulation was a spasmodic contraction of the hands and limbs, which might, however, be restored to their natural position by another person, if it was done immediately after death. Upon cross-examination, Dr Taylor said that there existed considerable differences of opinion in the medical world upon suicidal or homicidal cases. In the case of the Duke de Bourbon, there was a difference of opinion, but the majority believed that he destroyed himself. There were on record instances of suicide by strangulation effected in almost every possible attitude, and after the person had inflicted upon himself other acts of violence. If the rupture of the trachea had been produced by lateral pressure on the throat, he should have expected to find some external marks of that violence.—Two other medical gentlemen were examined—Mr Sharman, of Daventry, and Mr Nash, of Northampton. They made the post-mortem examination, and the former, although he was not aware that the longitudinal rupture of the trachea was at all remarkable, was of opinion that, taking all the circumstances into consideration, the deceased could not have committed suicide. The latter appeared inclined to think it possible that she might; but expressed himself with great hesitation and uncertainty. The chief proofs against the prisoner were, that she had been seen about the house of the deceased, near the time of the supposed murder; and one man swore that at about a quarter past eleven he heard groans and a scuffling, as of several persons in the house; and three women, who, just about the same time, were standing at a distance of 200 yards, heard cries of "Murder" proceeding from the cottage. At a quarter to twelve the prisoner was seen to enter her own house. She had then no shawl or apron on, her gown was torn in the gathers, and in the afternoon she changed her dress, and put the one which she had taken off into the wash-tub. Small spots of blood were afterwards found on her apron and shawl, and some tape



was found in a drawer in the prisoner's house. A mallet was also produced, with regard to which the prisoner herself had said that that was such a thing as was likely to have given the blows to the deceased. In order to show a motive for the commission of the crime, it was proved that, upon the death of the deceased, the prisoner and her husband would become entitled to a sum of L.900 odd; and that shortly before the 3d of October they had been pressed by their landlord for payment of arrears of rent.

The Lord Chief-Justice summed up the evidence, and the jury, after remaining in deliberation about a quarter of an hour, returned a verdict of *Guilty*. His Lordship then sentenced the prisoner in the usual terms.—*Legal Examiner*.

ON TATTOOING: A NEW MEDICO-LEGAL QUESTION. THE TRIAL OF SCHALL: A CAUSE CELEBRE. BY CASPER OF BERLIN.

(Translated from the *Vierteljahrsschrift für gerichtliche und öffentliche Medicin*. 1 band, s. 274.)

The legal investigation of the charge of robbery and murder preferred against Schall (formerly a postilion), which occupied the district jury court which sits in Berlin, for the first eight days in March, naturally caused great excitement in the capital, and for a week was almost the sole subject of conversation. And even now, when judgment has been pronounced, competent judges and the general public still talk with interest of the trial, reconsidering and weighing the many doubtful points with which it was complicated. Seldom, however, has a great crime been so shrouded in mystery as in the present instance,—and certain it is, that since the procedure in such cases by jury-trial was first introduced here, no case of such consequence has been the subject of public investigation. It involved, besides, in a scientific and medico-legal point of view, a number of important and partially decisive questions, for the solution of which I was summoned by the counsel for the Crown. Among these questions there was one, entirely new, and of which I did not hesitate in open court to affirm, that it had never been scientifically examined, and hence that, in general, “medical men did not know much more about it than others.” I mean the question: Can tattooing-marks become effaced during life?

All these grounds will justify me in undertaking here to give a short account of this true *cause célèbre*, as I have become acquainted with it from the study of the legal documents and public proceedings connected with it; and in doing so I shall regard the case specially in a scientific and medico-legal point of view, and accurately communicate what I have myself on this occasion observed and learned regarding tattooing.

On the 10th September 1849, not far from Berlin, and in the immediate vicinity of a game preserve and a branch of the Spree, there was found among the reeds the body of a man, from which the head had been removed by a clean incision, passing, as the medical witnesses afterwards affirmed, in a straight line between the first and second cervical vertebræ. The head was found at a distance of fifteen paces, and displayed a double shot-wound below the right ear, directed from below upwards, many apparent wounds caused by cutting the face, and complete comminution of all the bones of the head causing it to fall into a flattened shape when lifted and again laid down. It was obvious that the murderer or murderers had thus disfigured and mutilated the head, to conceal the deed, and prevent the identification of the murdered man—an object which was indeed perfectly effected. The attempt had, besides, been obviously made, for the same reason, to throw or drag the head into the river; this had not been accomplished, probably in consequence of the extremely swampy nature of the ground about the banks. Near the body a little crooked stick was found sticking in the earth; near it a gray cap, and an open match-box, in which some sulphur matches still remained.

The body was clothed with a shirt (marked G. E. 4), two chemisettes, drawers, and summer trousers, worsted stockings, boots, a satin vest, and leather



embroidered braces, which were found unbuttoned. As was acutely remarked by the Crown counsel, they had probably been loosened when the good cloth upper trousers, which, together with money, a watch, and signet-ring, had been stolen from the murdered man, were removed. On the ring-finger of the right hand there was a golden marriage-ring, marked H. H., 1843; and in the course of the oral evidence the important fact was elicited, that this ring had (as usual) made a deep impression on the finger, which was also observed on the body.

Next day, the physicians A. and B. performed the medico-legal examination of the body. They found the signs of "incipient putrefaction;" *post-mortem stains were, however, nowhere observed.* Of the borders of the shot-wounds on the head, it is said that "they are ragged, of a darkish hue, and turned inwards." The borders of the incised wounds on the face were not described in the first report; in a subsequent document, one of them is merely said to have had "smooth, bloodless edges, like those of an incision made upon a body several days after death." The colour of the skin was not mentioned in the first report (Obductions—Protocoll<sup>1</sup>), but in the second (Obductions—Bericht<sup>1</sup>) "the remarkable whiteness of the skin" is alluded to. "On the arms there were visible small, bluish-red, and swollen spots, of irregular size," which the reporters termed "sugillations." Different unusual markings, to which I shall presently direct attention,—viz., cupping scars and tattooed marks,—were not either observed on the body, or alluded to in the documents. Both medical reporters declared, at the final hearing of the cause (Audienztermin<sup>1</sup>)—three years afterwards—in answer to repeated questioning, that *cupping marks*, had such existed, might possibly have escaped their observation; but certainly *not tattooed marks*, which must have attracted notice in the course of their protracted and careful examination of the body. The magistrates (Gerichtspersonen<sup>1</sup>), too, as the record of their proceedings bears, "in the course of a careful examination of the naked body for an hour, observed *no tattooing marks* on the arm. As for the internal examination of the body, I need only mention here, that it yielded no result bearing on the case more directly than the important observation of the complete and general want of blood. In their second report, the medical witnesses stated their opinion—(1), that the deceased had sustained an absolutely fatal double shot-wound in the skull, and met his death in consequence; (2), that the removal of the head was subsequently effected by means of a cutting instrument, skilfully used; and (3), that the removal of the head must have taken place *immediately after death from the shot-wound.*

When the first judicial steps were taken for the identification of the unknown person of the murdered man, and for the detection of the alike unknown murderer, an unmarried woman, Gl.—who afterwards proved to be a person of very evil repute—presented herself under the assumed name of a married woman, and asserted that the description of the body given in the newspapers had awakened the suspicion that the deceased was her husband. The clothing taken from the body was laid before her, and lo! she recognised it, piece by piece, as the property of her husband, and in part as her own work. Hereupon, and nine days after the medical examination, the body was disinterred (for the first time), and the woman recognised it positively (by the genitals! among other marks), as the body of her husband. The result of inquiries, which were immediately and most carefully made, proved that there never had been such a husband in existence. It was not made perfectly clear whether this woman was of weak intellect, or a cunning deceiver, perhaps interested in the profits of the robbery and murder. The incident was and remains an episode in the fearful drama, destined to be followed by several others.

In further investigating the case, several grounds seemed to indicate that the *murderer* of the unknown individual was a cattle-dealer, named Gottlieb Ebermaun, a dangerous criminal, for whose apprehension warrants had been formerly

<sup>1</sup> We give the German terms, not being enabled to find their technical significance in English law phraseology.—*Editor.*

issued; and the magistrate who conducted the inquiries accordingly publicly offered a reward for his capture. This suspicion did not subsequently gain force; on the contrary, as we shall afterwards find, the accumulation of additional evidence substituted for it the exceeding probability that Ebermann was the *murdered man*! Certainly an unheard-of case in the annals of criminal trials! The doubts as to the identity of the body with the person of Ebermann were, however, originally and continued so considerable, that the determination of this point occupied half the time of the long sitting of the court, and in consequence of the obstinate lying and silence of the former postilion, Schall (a notorious poacher and very dangerous highwayman, to whom the crime was, after many long attempts, brought home), exercised the utmost ingenuity of the president of the court, and of the counsel both for the prosecution and defence. My own assistance was called in at this juncture. But, before proceeding to my own evidence, or to the proper subject of this treatise, which it includes, I shall make a few cursory observations. It became afterwards known that Ebermann, while alive, had cupping marks on one of his wrists, and a *red heart* and the *letters G. E.* (his initials) tattooed upon his left fore-arm. Thus one witness, a long time before, had seen the tattooing on Ebermann while bathing; and two surgeons of Mecklenburg—one of whom had cupped the deceased, and the other several times bled him at the arm—both deposed, that at the times when they last saw Ebermann—i. e., respectively eight to nine, and three to four, years before—they had remarked the tattooing on his arm. But Ebermann's three married sisters and his wife, when examined, stated that they had never observed the tattooed marks. The last witness had been married to him only for a short time, and even during that period had been for the most part separated from him while he was undergoing imprisonment in jail: she asserted, however, on oath, that very lately she must have observed any such marks, for that her husband was both a poacher and highwayman; that she helped him to clean his arms, and used to turn back and bind up his shirt-sleeves, preparatory to this occupation. At this period of the trial, although the medical witnesses had, as we have above stated, affirmed positively that they had seen no tattooing marks on the body, it was thought essential to justice to disinter the body again. This was done on the 13th February 1850, consequently five months after death, in presence of the medical witnesses. The medical report, however, bore, that "decomposition was already too far advanced to permit cupping or tattooing marks to be made out on the body, and that none such were visible."

Meanwhile the report was spread, that Ebermann was alive still, and had been seen! In following out this rumour, its source was arrived at in the witness N. He told with much simplicity how he had gone to see Ebermann,—had spoken to him, but without obtaining any answer, etc. It soon turned out that this man was a *ghost-seer*, who with the utmost earnestness assured his auditors that he had likewise seen and conversed with a burgomaster, who had been long dead, and at whose funeral he had himself, by his own admission, assisted. This was a new episode!

Ebermann's mistress, an unmarried woman, named N., to whose evidence great importance was now attached, declared positively, that the short stick found stuck in the earth near the body was the property of Schall—a man of about five feet four inches high,—and that the long stick, found with other property of Ebermann's in Schall's house, was the stick of her deceased paramour, a man of greater stature, about five feet eight inches high. She had further recognised all the articles of clothing found on the body as Ebermann's; and in the prosecution of the inquiry, added, that his teeth were so unusually broad and long, that she could recognise them if placed before her. In consequence of the ever-recurring doubts as to the identity of the body, it was deemed expedient to exhume the head again on the 11th December 1851 (third disinterment!), and H. now recognised, in the most positive manner, the teeth in the lower jaw, which were perfectly entire, as also what remained of the reddish beard, which, curiously enough, still were attached to the denuded bone, as belonging to her paramour

Ebermann. (In the course of one of the hearings of the case, the president of the court called upon me to compare these teeth with those of Ebermann's brother. I reported that there was certainly a resemblance between them, but that I could deduce no inference from the observation.) On the 12th August 1851, a murderous attempt on the life of the witness N. was made by an unknown hand, very probably, however, as she asserts, by a notorious associate of Schall, now in prison. Episode the third!

The wife of Ebermann recognised with distinctness the different portions of clothing found with the body, not only as the property of her husband, and in part made by herself; but, in particular, the wedding ring, marked with her initials, which, as above mentioned, had caused an indentation on the finger, as the ring which she had changed with Ebermann at her marriage. Her own ring, marked with Ebermann's initials, and the same date, G. E. 1843, corresponded with it exactly. With equal distinctness she recognised the hairs, and the watch found in Schall's possession as belonging to her husband.

The inquiry seemed at an end, and the accused was consequently placed before the court for trial in October 1851. From the proceedings which then took place, and which I did not personally witness, I shall only extract such as are recorded in the judicial papers, and may interest the reader, as well as illustrate the subject of this communication. The first medical witness at the hearing of the 30th October, deponed, in addition to his former report, "the head must have been cut off, *immediately after death*, for a large quantity of blood has escaped from the body, which is not the case when the head is removed at a later period when the blood has become stagnant. The head *cannot* have been removed sooner, or the amount of hemorrhage would have been even greater. . . . If the clothes found had been put upon the body after death, the shirt could not have been drawn on without being made all bloody, unless it were drawn from below upwards. The shirt was not placed upon the body after death, or it must have been soiled behind, for the change of clothing must have been effected in the wet meadow." . . . Cupping scars become faint in process of time, but always leave little white scars for the rest of a patient's lifetime. *When tattooing is very effectually performed, it never becomes effaced.* The depth of the punctures must be taken into account. If they are only superficial, so as only to implicate the cuticle, the tattooing assuredly becomes effaced in process of time. If one of the witnesses saw the marks on Ebermann at ten paces distance, the tattooing must have been well performed. When in cupping the operation is so performed as to cause a copious depletion, durable scars, which never become obliterated, are the consequence.

The second medical witness declared, on this head, "Cupping scars may vanish, if the scarification has been superficial, in a few years, perhaps in two or three years. As for *tattooing*, I can give no decided opinion."

Circumstances occurred which caused an adjournment of the trial, and on the 1st March 1852 this important case was again submitted to a jury. As already remarked, the advocate for the Crown wished me to be called as an expert (*Sachverständiger*). From his letter to me of the 27th February I extract the following passages. "The points to which you must specially attend are the following:—*a.* The body of Ebermann was found (in the condition already described). The medical men have reported, that his death was caused by the double shot-wound, which fractured the skull and injured the brain, and that the decapitation and infliction of the incised wounds took place after the death occasioned by the shot. According to this report, the cuts on the face and cutting off of the head were acts perpetrated on a dead body. Can this be with absolute certainty confirmed?

"*b.* The body was found clothed completely in Ebermann's clothes. A report has been spread, that the man presumed to be murdered still lives. In this case, Ebermann must have had some part in the murder of the 10th September 1849, and must either have put all his own clothes, shirt, stockings, etc., upon the body, or must have caused the murdered man before his death to put on his clothes. The latter supposition is quite improbable. Is it possible and

probable, looking at what the judicial proceedings contain regarding the dress of the body, that it was so attired after death?

"c. Witnesses of unimpeachable credit assert, that Ebermann had a red heart and letters tattooed on his left arm, also that he bore cupping marks on his nape and wrist joints. On the other hand, Ebermann's wife and relations say that they never observed on him marks of cupping or tattooing; and in the proceedings of the criminal court of Spandau the description of Ebermann is given, without allusion to tattooing or cupping marks as furnishing any means of recognising him. As the whole proceedings force on me the conviction that the body found was Ebermann's, I hold the above-mentioned medical reports, and the explanations of the two physicians, to be erroneous. Have the tattooing and cupping scars really escaped their observation?"

Never, in the course of a long series of years, rich in the practice of medico-legal investigations, have I ever felt so strongly the responsibility attaching to an opinion required of me, as in this final trial; being well aware, even without the explanations which had been made to me, that my opinion would be considered almost decisive, both as to the active and passive agents in this crime, and especially as to the doubtful identification of the murdered man, a point with which the guilt or innocence of the accused was intimately connected. And a theme was introduced on which all written authorities on medical jurisprudence, all compilations are silent, and on which I had only found a short notice in the "Dictionnaire des Sciences Médicales," which, in its article on tattooing, treats of the "caractères ineffaçables" of this operation. But perfectly at rest as to the accuracy of all the statements which I made before the jury, I now submit my evidence, premising the grounds on which it was founded, for the decision of those acquainted with the subject.

The process of tattooing, which, as is well known, is in this country practised almost exclusively by men, chiefly on the arms, but also sometimes on the breast (while certain savage nations apply it to the whole body, indicating degrees of rank by the pattern engraved), is accomplished by means of three or four sewing-needles stuck into a cork or piece of wood, and turned up at the point, which are deeply bored into the skin on which the intended pattern has been traced. Such of our men as have the taste for tattooing (almost all of the lower orders of society, such as sailors and soldiers), commonly choose as a decoration one or two hearts, their own initials, or those of their lady-love (*Herzendame*), a date in memorial of some remarkable occurrence, or, even as I have seen among soldiers, crossed swords or cannon, a face, an entire diminutive figure, and so forth. When the bleeding from the small punctured wounds has subsided, some coloured stuff is rubbed into the recent wounds, consisting usually of cinnabar or gunpowder, or generally of both, in order to make a pretty pigment—rarely of China ink. The common opinion is well known to be, that these marks never become effaced during life, and, consequently, not after death, and a theoretical explanation of this doctrine at once suggests itself. But this general belief can only be founded on the fact that old, even very old, people have been seen with marks of tattooing performed in their youth many years before, still distinctly visible. *Ergo!* But an *ergo* of this sort would not satisfy me in the case which was under investigation. It was necessary to attempt to discover, whether a large number of men, for instance of old soldiers, could be found, who had been tattooed at an early period of life, and then to inquire whether the marks on any of them had at a later period disappeared! For were this the case even in a single individual, the opinion would be not only justified but rendered probable, that in the case of the cattle-dealer Ebermann the tattooing might have disappeared. For my investigation, the Royal Invalid Establishment presented an admirable field; and the results I consider it my duty to detail here at length, not only on account of their novelty, but because, in the course of the trial, I considered it necessary to give a mere summary of them.

In the invalid establishment of Berlin, and with the assistance of one of my

pupils, Staff-Surgeon Dr Transfeld, I examined the following old tattooed soldiers, in number six-and-thirty :—

Name.	When, and with what, Tattooed.	Present State of Marks.
1. Stadler.	1798. Lamp black and Brandy.	Faded; on the left arm 1798 is still distinctly visible; on the right the letters S and R of Stadler distinct, T and E indistinct; nothing else visible.
2. Siedschlag.	1807. Powder and Cinnabar.	On the left arm two cannon distinct; on the right a mere bright red stain of the red heart, once distinct upon it.
3. Schlottman.	1808. Cinnabar.	J. F. A. S., very well preserved.
4. Schreiber.	1808. Powder and Cinnabar.	On left arm a black heart; the cinnabar marks have disappeared.
5. Burschinsky.	1809. Cinnabar.	M. B. and two sabres very well preserved.
6. Klatt.	1809. Do.	A heart and 1809, much faded.
7. Senfft.	1811. Cinnabar and Powder.	Tattooed in Spain; very well preserved.
8. Kerl.	1811. Do.	Do. Do.
9. Korthaus.	1813. Cinnabar.	Well preserved.
10. Beyser.	1813. Cinnabar and Powder.	Heart and letters, very well preserved.
11. Tapper.	1813. Cinnabar.	Well preserved.
12. Hanne.	1814. Do.	A heart on the left arm remained visible for two or three years, and then totally vanished; at present there is not a trace visible.
13. Albrecht.	1814. Do.	On the left arm a little soldier; on the right E. A. and a heart, distinctly visible.
14. Ribenhausen.	1814. Do.	Well preserved.
15. Hesselbach.	1814. Do.	Good and distinct.
16. Steinert.	1814. Cinnabar and Powder.	Figures, numbers, and letters, well preserved on both arms.
17. Meissner.	1814. Cinnabar.	Very distinct.
18. Zappe.	1815. Cinnabar and Powder.	Well preserved.
19. Schulz.	1815. Cinnabar.	F. A. S. and a heart, very distinctly visible.
20. Lehmann.	1815. Powder.	Well preserved.
21. Kolenz.	1815. Cinnabar.	Much faded.
22. Batzlaff.	1816.	Heart and 1816 lasted only two or three years; at present no trace of tattooing visible.
23. Pluta.	1817. Do.	Well preserved.
24. Giesler.	1817. Cinnabar and Powder.	Signature distinct.
25. Marschner.	1817. Cinnabar.	Well preserved.
26. Grandscheck.	1818. Do.	Do.
27. Pitsch.	1820. Do.	Do.
28. Schmidt.	1822. Do.	Rather faint.
29. Schröder.	1823. Cinnabar and Powder.	Well preserved.
30. Krisek.	1825. Cinnabar.	Do.



Name.	When, and with what, Tattooed.	Present state of Marks.
31. Stein.	1825. Cinnabar.	Well preserved.
32. Schmidt.	1831. Do.	Do.
33. Hofbauer.	1837. Do.	Do.
34. Bölke.	1841. Do.	Do.
35. Schmidt, II.	1845. Do.	Do.
36. Meissner.	Red ink (?)	The tattooing (on right fore-arm) was destroyed by suppuration in six weeks after the operation; it has now disappeared, and left no trace behind.

These facts are very surprising! While in case 1, after the lapse of fifty-four years, there are still at least some tattooed marks distinctly visible, they have perfectly faded in cases 12 and 22, after the respective periods of thirty-eight and thirty-six years; while in several other instances they are still quite distinctly visible after more than forty years. That the material used causes no difference in this respect, is proved by a glance at our table; No. 36 must be excluded, in which the tracing was effected with red ink. But it is far from certain, "that the depth of the punctures in tattooing," on which the first medical witness gave an opinion (erroneous, yet in the circumstances excusable), can cause the difference in durability; for in cases, such as that marked 22, in which the tattooing remains for several years, and then disappears, any other supposition may be adopted than that the operation has been imperfectly performed. In general, we learn from our cases that in *three* (Nos. 6, 21, and 28) out of thirty-six, the marks faded in process of time, that in *two* others they became partially (Nos. 1 and 2), and in *four* perfectly obliterated (Nos. 4,<sup>1</sup> 12, 22, and 26).

Consequently, since in one of every nine cases the tattooing had disappeared in process of time, it may also have faded on Ebermann's body during life, and hence not been visible after death. My answer to the question put could not consequently be doubtful. Founding upon my observations, and relating them, I accordingly asserted with confidence, *that tattooed marks may disappear*, and hence the most important *doubt as to the identity of the body of the murdered man was removed*. It was likewise instructive for those engaged in, and present at, the trial, that in the course of the proceedings there came forward a witness, belonging to the more educated class, who distinctly related, how in his fifteenth year he had had his left arm tattooed with cinnabar, and how the marks had in a few years faded. I examined the arm in open court, and there were *no traces of tattooing* upon it—a point on which judge and jury likewise satisfied themselves. As for the cupping marks, I must, however, dismiss the question of their existence or non-existence on the body. They might indeed have been quite appreciable, and yet overlooked by the medical witnesses, and nothing is more explicable and excusable in a case where the cause of death was so apparent, and in which the marks (which when old are difficult to see upon a dead body), were situated in so unusual a region as the back of the wrist.

A second point referred to me (see above) was this:—Whether, as the medical evidence bore, it could, with absolute certainty, be affirmed that the separation of the head from the trunk, and infliction of wounds on the face, took place after death had been caused by the shot? To my sorrow I was obliged, in making my statement on this head before the jury, to preface it with a critique of the medical evidence, which, although upon the whole creditable, yet in many points more or less important, was defective. We had not been informed of the state of the throat, genitals, trachea, larynx, œsophagus, etc. The medical witnesses had positively affirmed that no post-mortem staining was visible on the body. But as they had reported that the signs of "incipient putrefaction" were present,

<sup>1</sup> In this case at least cinnabar had been used.



I was compelled to differ from them here, inasmuch as post-mortem stains are well known to be the first and never-failing sign of "commencing putrefaction," and when once formed never vanish, but rather continue distinct till the body can no longer be recognised. Hence such stains *must have* existed on the body of Ebermann. Another and more suggestive conclusion of the witnesses, I was obliged to dispute. They had spoken of suggillations on the foot and arm of the deceased, "as if the murdered man had been firmly grasped by some one by these parts;" and Schall's counsel had endeavoured to found a part of his defence upon this statement, as it would seem to lead to the conclusion, that at least several persons had taken part in the crime; and hence, that what passed on that occasion was different from what the indictment alleged. For my own share, I could not on oath accept the statement, that true bruises had been observed on the body; for it was justified by nothing else than the assertion of the medical witnesses—their opinion unconfirmed by facts or proper post-mortem examination of the parts. Neither the preliminary nor the subsequent reports (Obductions-Protocoll und Obductions-Berichte) made mention of any examination of the so-called *suggillations* with the knife—the only criterion by which bruises can be distinguished from post-mortem staining, blood being in the former case found extravasated in the subcutaneous cellular tissue, which in post-mortem staining is never observed.

Hence I declared that there was no proof that "suggillations" had existed on the body; and hence, that all conclusions based upon the alleged existence of these suggillations were destitute of scientific value. The defender now forsook this part of his defence. Medical jurists may, however, take warning from this apparently trifling circumstance, which, in the criminal trial in question, assumed such high importance, and learn how careful should be their conduct in performing the medico-legal examination of a dead body.

Could it now be affirmed, "with absolute certainty," that the cuts on the face were inflicted after death? By no means. However improbable the contrary—for it must have been very difficult to cut with a knife, as in a scuffle or fight, the face of the tall and powerful Ebermann, unless he were fast asleep—"certainty" on this point could only have been attained if the margins of the incisions had been examined and accurately described. The first report (Obductions-Protocoll) left us here completely in the dark; for it only described the borders of the gunshot-wound, which was hence shown to have been inflicted during life, without a syllable of description of the edges of the incised wounds of the face! Hence, that these wounds had been inflicted on a dead body was not only uncertain, but could not even be asserted. Was a surer basis for opinion furnished by the proceedings of the inquest (Obductions-Verhandlungen) relative to the separation of the head from a living or dead body? If my conclusions on this head were directly opposed to those of the medical witnesses, I at least never entertained a moment's doubt as to their correctness. These witnesses had with great care noted the fact of excessive *bloodlessness* (Blutleere) of the whole body, including all its separate organs. We may remark in passing, that this condition could not be attributed to advanced putrefaction, of which anæmia is a never-failing accompaniment,—for the dissection was performed only two days after the murder. Besides, as the proceedings showed, on the spot where the body of the murdered man was found, a great puddle of blood (Blutlache) was visible on the grass. That this quantity of blood could not have flowed from the body through the shot-wound, which, from the consequent destruction of the head, must have caused death much more rapidly than could have taken place by hemorrhage, is indubitable. But this absolutely anæmic condition of the body becomes more explicable, if it be assumed that the head was cut off while the heart still continued to beat,—i.e., *while the man was still alive*, and in the condition of one mortally wounded; while, if we suppose the removal of the head to have been effected when circulation had ceased, and the man was dead, it is impossible to account for the amount of blanching of the body and of organs like the lungs—of which the medical witnesses affirm, that they were perfectly void of blood and

(air ?)—of the liver, kidneys, etc. The grounds—not very clearly expressed—on which these witnesses formed a contrary opinion (see p. 305), viz. that the head was cut off immediately *after* death might be regarded as insufficient, and I confidently asserted that the deceased, mortally wounded no doubt by the double gunshot-wound, was *still alive* when his head was severed from his body. Such cannibal-like bloodthirstiness is perhaps unparalleled in the annals of criminal jurisprudence; and, on psychological grounds, it might have been permitted to doubt (had not excellent grounds for a contrary opinion existed) whether an accountable human agent could have perpetrated such cruelty; but the Crown counsel said very truly in the course of his able pleading, while describing the personal character and antecedents of the poacher Schall,—“Roebuck or man, ’tis all one to him” (Rehbock oder Mensch, das gilt ihm gleich).

“Is it possible or probable to assume, in accordance with the written evidence, that the body was after death dressed up in the articles of clothing found upon it?” On this last of the three principal questions addressed to me, there was, as is obvious, little to say in a medico-legal point of view. I had only to call the attention of the court to the subject of cadaveric rigidity. I consequently reminded the jury, that bodies are, as is well known, every day stripped, washed, and then completely clothed again in male or female attire, including even uniforms, gloves, and so forth. This can, however, only be done either very soon after death, and before the stiffening has taken place, which is accomplished at periods varying from six to eight, and twenty-four to thirty hours after death, or after the subsequent cessation of rigidity and restoration of flaccidity of the body, an event which is not usually observed for several days after the rigidity of death has set in. I consequently left it for the consideration of the court,—whether the great improbability could be accepted, that the murderer or murderers had stripped the body instantly after the murder, and then dressed it completely in Ebermann’s clothes;—in which case, as the bleeding does not cease immediately with life, the articles of clothing would have been of necessity far more soiled with blood than reality showed,—or whether they could admit the far more improbable supposition, that the murderers (several days after the stiffening of the body) returned to the spot where the deed was committed, and now undressed and dressed the body, which had meanwhile become pliant.

“In resumé, I may sum up my evidence on the points submitted to me at the final trial, as follows:—

“1. Contrary to the medical evidence, there must have been death-stains on Ebermann’s body.

“2 Contrary to the same evidence, I hold it not proved that there were ‘sugillations’ on this body.

“3. Contrary to the same evidence, it is *not* shown that the incised wounds on the head were inflicted after death.

“4. It cannot be assumed (as the same witnesses hold) that the head was severed from the trunk after death; on the contrary, it must rather be assumed, that Ebermann still lived when his head was being cut off.

“5. I agree with the witnesses, that the clothing was not put on the body after death.

“6. *Marks of tattooing may become effaced during life.*

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“The jury declared, ‘by more than seven voices’ (their written verdict says, *unanimously*), the postilion Franz Schall *guilty* of having, between the evening of September 9 and noon of September 10, with the previous intention of killing the cattle-dealer, Gottlieb Ebermann, inflicted on him such injuries as in the ordinary and commonly known course of events must cause death,—and hence of having murdered the deceased;” and the sentence awarded by the court, after this verdict, was death by decapitation.

*Note by Translator.*—In the account above given of this very curious trial, we have followed, perhaps too closely, Casper’s own words. Still, we confess

our utter ignorance of all the technicalities of Prussian criminal law, and may, consequently, have confounded the proceedings and persons of the coroner and his jury, with those of the judge and jurymen at the trial for murder. In a medical point of view, such mistakes are probably of little consequence.

OBSERVATIONS UPON A GENERAL METHOD FOR DETECTING THE ORGANIC ALKALOIDS  
IN CASES OF POISONING. BY PROFESSOR STAS, OF BRUSSELS.

Whatever certain authors may have said on the subject, it is possible to discover in a suspected liquid all the alkaloids, in whatever state they may be. I am quite convinced that every chemist who has kept up his knowledge as to analysis, will not only succeed in detecting their presence, but even in determining the nature of that which he has discovered, provided that the alkaloid in question is one of that class of bodies, the properties of which have been suitably studied. Thus he will be able to discover conia, nicotine, aniline, picoline, petinine, morphine, codeine, narcotine, strychnine, brucine, veratrine, colchicine, delphine, emetine, solanine, aconitine, atropine, and hyoscyamine. I do not pretend to say that the chemical study of all these alkaloids has been sufficiently well made to enable the experimenter who detects one of them to know it immediately, and affirm that it is such an alkaloid, and not such another. Nevertheless, in those even which he cannot positively determine or specify, he may be able to say that it belongs to such a family of vegetables,—the Solanaceæ, for example. In a case of poisoning by such agents, even this will be of much importance. The method which I now propose for detecting the alkaloids in suspected matters, is nearly the same as that employed for extracting those bodies from the vegetables which contain them. The only difference consists in the manner of setting them free, and of presenting them to the action of solvents. We know that the alkaloids form acid salts, which are equally soluble in water and alcohol: we know also that a solution of these acid salts can be decomposed so that the base set at liberty remains either momentarily or permanently in solution in the liquid. *I have observed that all the solid and fixed alkaloids above enumerated, when maintained in a free state and in solution in a liquid, can be taken up by ether when this solvent is in sufficient quantity.* Thus, to extract an alkaloid from a suspected substance, the only problem to resolve consists in separating, by the aid of simple means, the foreign matters, and then to find a base which, in rendering the alkaloid free, retains it in solution, in order that the ether may extract it from the liquid. Successive treatment by water and alcohol of different degrees of concentration, suffices for separating the foreign matters, and obtaining in a small bulk a solution in which the alkaloid can be found. The bicarbonates of potash or soda, or these alkalies in a caustic state, are convenient bases for setting the alkaloids at liberty, at the same time keeping them wholly in solution, especially if the alkaloids have been combined with an excess of tartaric or of oxalic acid.

To separate foreign substances, animal or otherwise, from the suspected matters, recourse is commonly had to the tribasic acetate of lead, and precipitating the lead afterwards by a current of sulphuretted hydrogen. As I have several times witnessed, this procedure has many and very serious inconveniences. In the first place, the tribasic acetate of lead, even when used in large excess, comes far short of precipitating all the foreign matters; secondly, the sulphuretted hydrogen, which is used to precipitate the lead, remains in combination with certain organic matters which undergo great changes by the action of the air and of even a moderate heat; so that animal liquids which have been precipitated by the tribasic acetate of lead, and from which the lead has been separated afterwards by hydrosulphuric acid, colour rapidly on exposure to the air, and exhale at the same time a putrid odour, which adheres firmly to the matters which we extract afterwards from these liquids. The use of a salt of lead presents another inconvenience, viz., the introduction of foreign metals

into the suspected matters, so that that portion of the suspected substance is rendered unfit for testing for mineral substances. The successive and combined use of water and alcohol at different states of concentration permits us to search for mineral substances, whatever be their nature, so that in this way nothing is compromised, which is of immense advantage when the analyst does not know what poison he is to look for.

It is hardly necessary to say that in medico-legal researches for the alkaloids, we ought never to use animal charcoal for decolorising the liquids, because we may lose all the alkaloid in the suspected matters. It is generally known that animal charcoal absorbs these substances at the same time that it fixes the colouring and odoriferous matters.

[This is no doubt true; we must not use animal charcoal to decolorise, and then look for the alkaloid in the *liquid*, but we may use it, at least in the case of strychnia and some of the non-volatile alkaloids, to separate them, and then we look for them in the *charcoal*. See notice of Graham and Hoffmann's Process for Detecting Strychnia : Monthly Journal, August 1852, p. 140.]

The above observations do not proceed from speculative ideas only, but are the result of a pretty long series of experiments which I have several times employed for discovering these organic alkaloids. To put in practice the principles which I have thus explained, the following is the method in which I propose to set about such an analysis:—I suppose that we wish to look for an alkaloid in the contents of the stomach or intestines; we commence by adding to these matters twice their weight of pure and very strong alcohol;<sup>1</sup> we add afterwards, according to the quantity and nature of the suspected matter, from 10 to 30 grains of tartaric or oxalic acid,—in preference, tartaric; we introduce the mixture into a flask, and heat it to 160° or 170° Fahrenheit. After it has completely cooled, it is to be filtered, the insoluble residue washed with strong alcohol, and the filtered liquid evaporated in vacuo. If the operator has not an air-pump, the liquid is to be exposed to a strong current of air at a temperature of not more than 90° Fahrenheit. If, after the volatilisation of the alcohol, the residue contains fatty or other insoluble matters, the liquid is to be filtered a second time, and then the filtrate and washings of the filter evaporated in the air-pump till nearly dry. If we have no air-pump, it is to be placed under a bell-jar over a vessel containing concentrated sulphuric acid. We are then to treat the residue with cold anhydrous alcohol, taking care to exhaust the substance thoroughly; we evaporate the alcohol in the open air at the ordinary temperature, or still better, in vacuo; we now dissolve the acid residue in the smallest possible quantity of water, and introduce the solution into a small test-tube, and add little by little pure powdered bicarbonate of soda or potash till a fresh quantity produces no farther effervescence of carbonic acid. We then agitate the whole with four or five times its bulk of pure ether, and leave it to settle. When the ether swimming on the top is perfectly clear, then decant some of it into a capsule, and leave it in a *very dry place* to spontaneous evaporation.

Now, two orders of things may present themselves: either the alkaloid contained in the suspected matter is liquid and volatile, or solid and fixed. I shall now consider these two hypotheses.

#### *Examination for a Liquid and Volatile Alkali.*

We suppose there exists a liquid and volatile alkaloid. In such a case, by the evaporation of the ether, there remains in the inside of the capsule some small liquid striæ which fall to the bottom of the vessel. In this case, under the influence of the heat of the hand, the contents of the capsule exhale an odour more or less disagreeable, which becomes, according to the nature of the alkaloid, more

<sup>1</sup> When we wish to look for an alkaloid in the tissue of an organ, as the liver, heart, or lungs, we must first divide the organ into very small fragments, moisten the mass with pure strong alcohol, then express strongly, and by further treatment with alcohol exhaust the tissue of everything soluble. The liquid so obtained, is to be treated in the same way as a mixture of suspected matter and alcohol.

or less pungent, suffocating, irritant; it presents, in short, a smell like that of a volatile alkali masked by an animal odour. If we discover any traces of the presence of a volatile alkaloid we add then to the contents of the vessel, from which we have decanted a small quantity of ether, one or two fluid drachms of a strong solution of caustic potash or soda, and agitate the mixture. After a sufficient time, we draw off the ether into a test-tube; we exhaust the mixture by two or three treatments with ether, and unite all the ethereal fluids. We pour afterwards into this ether, holding the alkaloid in solution, one or two drachms of water, acidulated with a fifth part of its weight of pure sulphuric acid, agitate it for some time, leave it to settle, pour off the ether swimming on the top, and wash the acid liquid at the bottom with a new quantity of ether. As the sulphates of ammonia, of nicotine, aniline, quinoline, picoline, and petinine, are entirely insoluble in ether, the water acidulated with sulphuric acid contains the alkaloid in a small bulk, and in the state of a pure sulphate; but as the sulphate of conia is soluble in ether, the ether may contain a small quantity of this alkali, but the greater part remains in the acidulated watery solution. The ether, on the other hand, retains all the animal matters which it has taken from the alkaline solutions. If it on spontaneous evaporation leaves a small quantity of a feebly-coloured yellowish residue, of a repulsive animal odour, mixed with a certain quantity of sulphate of conine, this alkaloid exists in the suspected matter under analysis. To extract the alkaloid from the solution of the acid sulphate, we add to the latter an aqueous and concentrated solution of potash or caustic soda, we agitate and exhaust the mixture with pure ether; the ether dissolves ammonia, and the alkaloid is now free. We expose the ethereal solution at the lowest possible temperature to spontaneous evaporation; almost all the ammonia volatilises with the ether, whilst the alkaloid remains as residue. To eliminate the last traces of ammonia, we place for a few minutes the vessel containing the alkaloid in a vacuum over sulphuric acid, and obtain the organic alkaloid with the chemical and physical characters which belong to it, and which it is now the chemist's duty to determine positively.

I applied, on the 3d March 1851, the process which I have described, to the detection of nicotine in the blood from the heart of a dog poisoned by two cubic centimetres [0.78 C.I.] of nicotine introduced into the œsophagus, and I was able in a most positive manner to determine the presence of nicotine in the blood. I was able to determine its physical characters, its odour, taste, and alkalinity. I succeeded in obtaining the chloro-platinate of the base perfectly crystallised in quadrilateral rhomboidal prisms of a rather dark yellow colour, and to ascertain their insolubility in alcohol and ether.

I have applied the same process for the detection of conia in a very old tincture of hemlock, which my friend and colleague M. De Hemptinne was so kind as to put at my disposal; and I was equally successful in extracting from the liquid colourless conia, presenting all the physical and chemical properties of this alkali. I was also able to prove that the ether which holds conia in solution, carries off a notable portion of this alkaloid when the solvent is exposed to spontaneous evaporation.

#### *Examination for a Solid and Fixed Alkaloid.*

Let us now suppose that the alkali is solid and fixed; in that case, according to the nature of the alkali, it may happen that the evaporation of the ether resulting from the treatment of the acid matter, to which we have added bicarbonate of soda, may leave or not a residue, containing an alkaloid. If it does, we add a solution of caustic potash or soda to the liquid, and agitate it briskly with ether. This dissolves the vegetable alkaloid, now free and remaining in the solution of potash or soda. In either case, we exhaust the matter with ether. Whatever be the agent which has set the alkaloid free, whether it be the bicarbonate of soda or potash, or caustic soda or potash, it remains, by the evaporation of the ether, on the side of the capsule as a solid body, but more commonly a colourless milky liquid, holding solid matters in suspension. The odour of the



substance is animal, disagreeable, but not pungent. It turns litmus paper permanently blue.

When we thus discover a solid alkaloid, the first thing to do is to try and obtain it in a crystalline state, so as to be able to determine its form. Put some drops of alcohol in the capsule which contains the alkaloid, and leave the solution to spontaneous evaporation. It is, however, very rare that the alkaloid obtained by the above process is pure enough to crystallise. Almost always it is soiled by foreign matters. To isolate these substances, some drops of water, feebly acidulated with sulphuric acid, are poured into the capsule, and then moved over its surface, so as to bring it in contact with the matter in the capsule. Generally we observe that the acid water does not moisten the sides of the vessel. The matter which is contained in it separates into two parts, one formed of greasy matter, which remains adherent to the sides,—the other alkaline, which dissolves and forms an acid sulphate. We cautiously decant the acid liquid, which ought to be limpid and colourless, if the process has been well executed; the capsule is well washed with some drops of acidulated water, added to the first liquid, and the whole is evaporated to three-fourths in vacuo, or under a bell-jar over sulphuric acid. We put into the residue a very concentrated solution of pure carbonate of potash, and treat the whole liquid with absolute alcohol. This dissolves the alkaloid, while it leaves untouched the sulphate of potash and excess of carbonate of potash. The evaporation of the alcoholic solution gives us the alkaloid in crystals.

It is now the chemist's business to determine its properties, to be able to prove its individuality. I have applied the principles which I have just expounded to the detection of morphine, iodine, strychnine, brucine, veratrine, emetine, colchicine, aconitine, atropine, hyoscyamine,—and I have succeeded in isolating, without the least difficulty, these different alkalies, previously mixed with foreign matters.

I have thus been able to extract, by this process, morphine from opium, strychnine and brucine from nux vomica, veratrine from extract of veratrum, emetine from extract of ipecacuanha, colchicine from tincture of colchicum, aconitine from an aqueous extract of aconite, hyoscyamine from a very old extract of henbane, and atropine from an equally old tincture of belladonna. Thus it is in all confidence that I submit this process to the consideration of chemists who undertake medico-legal researches.—*Bulletin de l'Académie Royale de Médecine de Belgique*. Tom. vi., No. 2.

## MIDWIFERY.

### HOW TO FORM A CORRECT ESTIMATE OF THE DIMENSIONS OF THE FEMALE PELVIS.

BY G. VROLIK.

Vrolik asserts that no writer on obstetrics, from J. L. Baudeloque to J. E. Rosshirt, whose works he has referred to, proceeds upon a sure principle in estimating the dimensions of the female pelvis, and, in particular, of the inlet of the lesser pelvis. On the contrary, the oblique measurements should not be taken in a direction to which it is certain that the foetus cannot naturally be accommodated, nor should the transverse diameter be taken so far back or forwards as to bear no necessary relation to the head of the child. To exhibit that correspondence between the foetus and the aperture necessary for the passage of the former, all the lines of diameter should, according to Vrolik, be so drawn as to intersect each other in the central point of the brim of the lesser pelvis. This holds good no less of the transversely oval and obtusely heart-shaped pelvis, than of the round pelvis of the Javanese and of some European women. The direction of the oblique diameter, in order to correspond with the place which the head must occupy at the inlet, should not be from the sacro-iliac synchondrosis to the upper part of the pectineal eminence, or ischio-pubal synostosis of the opposite side, but to a point a few lines further forward on the horizontal ramus of the pubes, *i. e.*, excluding about three-fourths of the length of the ramus. (In



his Illustrations of the different forms of pelvis in different races, published in 1826, Vrolik has already pointed out this.) If a foetal head of average size be applied to the pelvic inlet with its long diameter in any other oblique direction, whether more forwards or backwards, resistance will immediately be experienced either against the promontory of the sacrum, or one of the superior rami of the pubes. What is true of the inlet, continues true of the passage and outlet, for the centre of the inlet is to be regarded as the axis of the lesser pelvis. Vrolik further points out, that the antero-posterior diameter of the outlet is by most authorities represented by a line touching the extremity of the coccyx while that bone is directed forwards; if the line be drawn to the coccyx directed backwards, its middle point will, as at the brim, be also the centre of the transverse diameter. Ritgen, who assumes four diameters drawn through the middle point of the inlet, cavity, and outlet of the lesser pelvis, has, in the determination of his measurements, proceeded upon an erroneous principle. The head in the successive positions which he believes to be necessary for natural labour, must, before leaving the mother's body, describe a half circle; but as the body firmly embraced by the contracted uterus, cannot follow all these movements of the head, displacements of the cervical vertebræ or rupture of their ligaments would frequently ensue.—*Nederlandsch Lancet*, February 1852, p. 516.

PREGNANCY REPUTED ANORMAL, AND TERMINATING NATURALLY. BY M. HUGUIER.

M. Huguier recently communicated the following instructive case to the Parisian Société de Chirurgie:—A woman under his charge in hospital was believed to be the subject of extra-uterine, or, at least, of anormal pregnancy, for the following reasons:—1. The abdominal tumour rose very high. 2. There was a distinct depression between the tumour and the pubes. 3. The child was felt through the parietes of the belly. 4. The form of the tumour was quite peculiar. 5. There was no placental souffle. 6. The tumour, instead of occupying the median line and bulging forwards, occupied the whole right flank. 7. In the vaginal *cul-de-sac*, a tumour was felt, and believed to be part of the child. 8. Fecundation had been “la suite d’un acte violent,” and had been followed by circumstances which have been noted as often symptomatic of extra-uterine pregnancy. 9. Of the two physicians to whom the patient had first applied, one considered her to have organic disease of the liver, while the other inclined to the belief that she had a tumour of the right ovary.

M. Huguier noticed, in addition, a continual increase of the transverse diameter of the abdomen—persistence of tumour in epigastrium—and the peculiar carriage of the patient, who, instead of throwing her body backwards, bent forwards while standing. Latterly there had been febrile disturbance and intermitting lumbar pains, causing suspicion of peritoneal irritation. In addition, up to the last moment no sign of approaching parturition: no uterine contraction, no dilatation of the cervix.

On the morning of the 30th of June, when M. Huguier saw the patient with M. Danyau, “the cervix was behind the pubes; it was conical and undilated. At this moment M. Danyau inclined so strongly to the idea of extra-uterine conception, that he pointed out the length and direction of the incision to be made.

“It was at three A.M. next day that labour commenced; the first pains manifested themselves on the 1st July, in the morning; they were very frequent, recurring every five minutes; up to this time, no alteration of the cervix. Some persons who were present urged me to interfere; but not being sufficiently satisfied of the existence of extra-uterine pregnancy, I refused, saying, that at two P.M. I should revisit the patient, and perform gastrotomy, after catheterising the uterus. At fifteen minutes past two, MM. Danyau and Roux having arrived, I examined the patient with the finger. The cervix was not open, but I felt something give way, and recognised the head presenting. From this moment all doubts were removed; a simple labour was approaching; the head presented in the first position; nothing unusual occurred, and in two hours the woman was delivered.”

On the third day the milk fever was observed; for ten days the uterus remained

high in the right flank, above the umbilicus, but finally resumed its normal position. Behind the uterus is felt a tumour of the size of an apple, which M. H. conceives may have been more voluminous during the pregnancy.

"I am inclined," says M. Huguier, "to attach the more importance to this case, because it presented numerous points of difficulty of diagnosis, and because all my confrères who saw the patient shared my doubts. It was therefore an abnormal pregnancy. If M. Paul Dubois did not admit the case to be one of extra-uterine pregnancy, he at first thought that the pelvis was presenting. As for M. Moreau, he likewise inclined to the idea of extra-uterine pregnancy."—*Gazette des Hôpitaux*, 1852, No. 94.

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## Part Fifth.

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### MEDICAL NEWS.

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#### CASE OF LUPUS CURED BY EXHIBITION OF COD-LIVER OIL. BY M. TEIRULICK.

A farm-servant, aged 23 years, presenting all the features of the lymphatic temperament, came under treatment on the 6th of December 1850. At the upper part of the sternum there existed an ulceration of the size of a five franc piece; under the border of the lower jaw of the left side there was another, of about the same size; a third was situated on the left side of the neck near the ear; and a fourth, greatly larger than any of the others, occupied the whole of the left cheek. These ulcerations presented the same characters,—of irregular form, their surface coated with a scab of a deep brown colour, adherent, and at points yielding a foetid ichorous discharge. The patient had, for a long period, been much exposed to cold and wet, and for years had subsisted entirely on black bread, potatoes, and poor milk. Struck with the scrofulous appearance of the patient, and by his apparently profound debility, M. Teirulick prescribed on the 11th of December half a pound of cod-liver oil to be taken during the day. Five days afterwards the dose was increased to a whole pound. On the 25th of December a pound and a half was taken in two doses; and on the 6th of January the dose per day was two pounds. This treatment was continued till the 13th of July, when, cicatrization being complete, the remedy was omitted, and the patient dismissed. The only other treatment employed was occasionally touching the healing ulcers with a little of the tincture of iodine. On the 15th of April the patient had become quite stout, and presented a healthy appearance. The largest amount of the oil taken on any one day was three pounds, and the whole amount consumed during the course of treatment, from the 6th of December to the 13th of July, was 265 pounds! The only occasion on which the remedy was suspended was in the month of June, and then only during five days, on account of a slight diarrhoea.—*Bulletin de Thérapeutique*, February 1852.

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#### EDINBURGH ROYAL INFIRMARY.

The new Surgical Hospital is now completed, and will be immediately opened for the reception of patients. It contains 120 beds, which, added to those of the present surgical wards, will afford accommodation for upwards of 200 patients. This part of the establishment is under a separate roof from the medical department, which contains between 300 and 400 beds. The kitchen and other offices lie between the two buildings, and have been furnished with every convenience afforded by the improvements of modern architecture. The operating theatre of this hospital has long been admitted to be the largest and best in existence; and the provision now made for surgical cases will, we believe, be found to surpass that of any other hospital in Great Britain.

## MULLAR v. THE "MONTHLY JOURNAL."

Dr Mullar having raised an action for defamation against the Conductors, Publishers, and Printers of the "Monthly Journal," the Lord Ordinary (Wood) required him to prepare an issue for trial, when he gave in the following:—

"It being admitted that the defenders are responsible for the 'Monthly Journal of Medical Science,' No. 128, dated August 1851, No. 20, New Series, and No. of Process:

"It being also admitted that, under the title 'Medical News,' p. 198, the following article appears in the said Journal:—

[Here follows the article as it appeared in the Journal for August 1851.]

"Whether the statements contained in the foregoing article of the said Journal are, in whole or in part, directly or indirectly, of and concerning the pursuer, and were intended to represent, and do falsely, calumniously, and injuriously represent, the pursuer as the intimate friend, assistant, and associate of a person described as arrogating to himself the title of a professor, without having any right thereto; and who is said to be so disgraced and sunk in the estimation and respect of the medical profession generally, and of his brothers or fellow-practitioners, as to be unworthy of credit, and to have lost all respect and claim to courtesy in their eyes; and as being united with the said person in joint medical undertakings, and in the propagation of false statements, regarding diseases and medical operations, for selfish ends and purposes of their own? And further, as a culpable propagator of falsehood and false statements, regarding medical facts and diseases, of the greatest importance to the health and lives of individuals? All to the loss, injury, and damage of the pursuer."

Lord Wood has decided that the defenders were not answerable for the meaning which the pursuer thought proper to attach to the words in question, and dismissed the case.

## CORRESPONDENCE.

## LETTER OF DR GEORGE PATERSON, OF TIVERTON, REGARDING MSS. IN LIBRARY OF COLLEGE OF PHYSICIANS OF EDINBURGH.

My Dear Mr Editor,—Are you aware that there are in the Library of the College of Physicians two MS. volumes of "Clinical Lectures," an. 1750? It does not appear, further than from what evidence the date supplies, by whom delivered; but I have always set them down as Dr Rutherford's, and marked them so in a list of MSS. in our Library, now beside me, which I drew up when assisting to prepare the last edition of the Catalogue for the press. The reference to the volumes is M<sup>o</sup>, 9-10.

People generally, and even the Fellows of the College themselves, are not aware of the amount of treasures of this kind, illustrating the early history of the Edinburgh Medical School, which our Library contains.

There are, besides these "Clinical Lectures," notes of *other* lectures of Dr Rutherford, in nine volumes, entitled "Prælectiones Medicinæ Practicæ" (M<sup>o</sup>, 31-39). There are also notes of those of Alston, St Clair, Plummer, Francis Home (including *his own* MS. of "Lectures on Materia Medica," M<sup>o</sup>, 29-30), Cullen (among which notes of his "Clinical Lectures," 1771 and 2),—Monro *primus*, John Hope, Thomas Young, Black, John and James Gregory, with four volumes of "Notes of Clinical Lectures" by the latter.

The greater part of the above were purchased by Dr Duncan, sen., in 1772, from Mr John Murray, bookseller in London, for seven guineas, and were bequeathed to the College after his death, together with about one hundred volumes of "Practical Observations in Medicine," in his own handwriting, which he had employed as notes for his "Clinical Lectures." But some of them have come to us from other quarters, and chiefly from the libraries of Drs Moncrieff, James Home, and Abercrombie.

In addition to these, there are two volumes of "Notes of Clinical Lectures" by *Boerhaave* (M<sup>o</sup>, 21-26). Also MS. notes of lectures by Albinus and Van Sweiten, and John and William Hunter.

To crown my list of curiosities, I must not omit to add seven or eight volumes of MSS. on Alchemy, presented to the College in 1707-8, by George Earl of Cromartie, styled "Fellow of the Royal College of Physicians," and which it appears were collected by Lord Cromartie's grandfather, Sir George Erskine, of Inverclyde.

Altogether there are many curious materials for professional history lying dormant in the Library and minutes of the College of Physicians of Edinburgh. Your last number of "Colloquia" has again drawn my attention to them, and induced me to take up papers which I brought with me unfinished from Edinburgh, and which, if I can complete them, as I hope to be able to do, will form a classified index to the minutes of the College during the first sixty or seventy years of its history. Meanwhile, it is natural to desire to vindicate, for the College, the prior possession of such relics of many of its most distinguished Fellows, and founders of the Medical School of Edinburgh; and this I hope I have succeeded in doing, without claiming too much of your editorial attention. . . .  
—I am, etc.

G. PATERSON.

Tiverton, August 14, 1852.

LETTER OF DR R. MORTIMER GLOVER, OF NEWCASTLE, ON THE BEER-PUFF.

46, Westgate Street, Newcastle-upon-Tyne, August 11, 1852.

Sir,—I perceive in the Number of the Monthly Journal for this month, some observations on the certificates given recently for Allsopp's beer, and I am referred to amongst others.

I beg to state, that I never gave Mr Allsopp any certificate whatever. When the statement about the use of strychnia in beer was made, I looked on the thing as unlikely (independent of the obvious danger) from the price of strychnia; but, on trying a few experiments, I found that, as far as the price was concerned, the thing was possible, inasmuch as a small quantity of strychnia would bitter (especially along with other bitters) a very large quantity of beer. I wrote to the "Lancet" stating this, and that, *as far as the price was concerned*, an unprincipled person might use it as an adjunct in bittering beer. Perhaps my letter was too brief, and I did not explain myself enough.

Mr Allsopp wrote to me, and called upon me to retract what I had said, as if I had calumniated his beer. I wrote to him that I had nothing to retract; and, in point of fact, the report of Hoffmann and Graham corroborated what I had said in the "Lancet." Out of this correspondence, Mr Allsopp has picked the not-over-dignified sentence which he has published as a certificate from me of the merits of his beer, which, certainly, I do not doubt, but to which I had no idea I was contributing publicly. I hope, therefore, as far as I am concerned, you will do me the justice to admit, that I am free from any blame as regards countenancing the advertisements of Mr Allsopp.—I am, &c.

Dr W. Robertson.

R. M. GLOVER.

PUBLICATIONS RECEIVED.

On Animal Electricity. By Emil Du Bois, Reymond. Edited by H. Bence Jones M.D. London: Churchill. 1852.

Medical Jurisprudence. By Alfred S. Taylor, M.D., F.R.S. London: Churchill. 1852. Fourth Edition.

Practical Remarks on Palpitation and other Functional Diseases of the Heart. By John Calthrop Williams, M.D. London: Churchill. 1852. Second Edition.

Disease in Childhood. By Robert Ellis, F.L.S. London: G. Cox. 1852.

The Grievance of the University Tests as applied to Professors of Physical Science in the Colleges of Scotland. By George Wilson, M.D., F.R.S.E., etc. Edinburgh: Sutherland & Knox. 1852.

Electricity and the Electric Telegraph, to which is added, The Chemistry of the Stars. By Dr George Wilson. London: Longmans. 1852.

Braithwaite's Retrospect, January to June, 1852. London: Shnpkin, Marshall and Co. 1852.

A Practical Treatise on Diseases of the Skin. By J. Moore Neligan, M.D., M.R.S.A., etc. etc. Dublin: 1852. Small 8vo. Pp. 439.

On a New Method of Treating Certain Diseases of the Cervix Uteri. By Robert Ellis, F.L.S., Surgeon to the National Society's Training Institution for Schoolmistresses, to the Hans Town Industrial School, etc. London: G. Cox. 1852.

## Part First.

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### ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Temporary Albuminuria, more particularly as occurring in the course of certain Febrile or other Acute Diseases.*  
By J. W. BEGBIE, M.D., F.R.C.P.E., Physician to the New Town Dispensary.

(Read to the Medico-Chirurgical Society of Edinburgh, 5th May 1852.)

"It appears to me most remarkable," writes Dr Adams, in his learned translation of the works of the father of medicine, "that the important observations made by Hippocrates on the state of the urine in febrile diseases should have been lost sight of in an age when the chemical characters of the urine have been so much studied."<sup>1</sup> M. Littré has made a remark to the same effect, in his edition of Hippocrates, and it will, I think, be acknowledged by all to be a just one. The observations on the urinary secretion contained in the aphorisms are such as to fill the attentive reader with wonder, as well at the amazing observation as at the vastness of the knowledge possessed by their immortal author. It has, indeed, been contended by some, that the fact of the occasional albuminous nature of the urine was known to Hippocrates,<sup>2</sup> and, if this notion be correct, then, more wonderful still, he had already associated it with a protracted disease of the kidney.

Whether known to Hippocrates or not, this connection of disease and symptom long lay neglected, and it is indeed only within our own days that Blackhall,<sup>3</sup> by showing the frequent association of coagulable urine with dropsies, and then the well-known researches of Drs Bright, Christison, Gregory, and many others, allying albuminuria with alteration of the structure of the kidney, have added to our knowledge regarding it. At the present time, though *well known*, there is perhaps no symptom the value of which is more frequently misunderstood than the existence of albumen in the urine. Unless the occasion of its presence can be referred to some such

<sup>1</sup> Hippocrates. Sydenham edition. Vol. i. p. 98.

<sup>2</sup> On this point, see Dr Adams' Observation on Aphorism 34, Section 7; also Dr Adams' translation of Paulus Ægineta, vol. i. p. 552—(this page is erroneously printed 352).

<sup>3</sup> Observations on the Nature and Cure of Dropsies. London, 1813.

certain cause as the admixture of pus or blood, etc., it is apt too generally to be linked with organic change in the kidney. That the difference of opinion which prevails as to the pathological importance to be attributed to the existence of albumen in the urine arises mainly from two sources, most will, I apprehend, be ready to admit. These are—either from an imperfect knowledge as to the causes of, or conditions under which, the coagulability of the urine occurs; or from an incorrect appreciation of the indications that coagulability, even when observed, affords. In illustration of the first of these, I need only here refer to the condition of the urine in a febrile disease, to which I shall afterwards have occasion in this communication more fully to direct attention—I mean scarlatina; and now speak more particularly of the condition of the urine in the dropsy which so frequently follows that disease. In such cases I have found the urine almost *always* to contain a considerable, in some a very large, amount of albumen; Becquerel<sup>1</sup> appears to have had the same experience; and Dr Anthony Todd Thomson,<sup>2</sup> in whose posthumous work on skin diseases there occurs this passage:—“The urine is *always* found albuminous where dropsical symptoms appear.” Opposed to this are the statements of Simon,<sup>3</sup> and Dr Scott Alison,<sup>4</sup> and the observations of Phillip,<sup>5</sup> in Berlin,—the former speaking of the disease under the two heads of albuminous and non-albuminous. In illustration of the second reason which I have assigned as causing difference of opinion on this subject, I need only here remind the members of the Society of many cases which must have fallen under their own observation, in which they did experience difficulty in referring the occurrence of albumen in the urine to its proper cause, where perhaps they too rashly concluded its dependence on organic change, and only after further reflection, and more careful examination, detected a much more simple cause, which had been previously overlooked, and which neglect had led them, in the first instance, unreasonably to magnify the importance of the disease. This is no supposititious case—such a one I have not unfrequently encountered—and the class of which it is the type will appear by examples in the sequel.

In the present communication I propose directing the attention of the Society to the condition of the urine in certain febrile or other acute diseases. Though the inquiry is important, the subject is too vast to permit me to discuss it wholly. I have, therefore, selected for present investigation one of the most striking characteristics of the urine in such diseases—its frequent coagulability. In a word, my object is not so much to deal with the aphorisms of Hippocrates

<sup>1</sup> Semeiotique des Urines, p. 267.

<sup>2</sup> Diseases of the Skin. Edited by Dr Parkes. P. 8.

<sup>3</sup> Simon's Chemistry. Sydenham edition. Vol. ii. p. 313.

<sup>4</sup> London Journal of Medicine. March 1849.

<sup>5</sup> Casper's Wochenschrift, 1840, No. 35; and Simon's Chemistry, Sydenham edition. Vol. ii. p. 280.



on the state of the urine near and at the crisis of fevers, as it is to illustrate the observation of M. Martin Solon,<sup>1</sup> that at the resolution of diseases the urine is apt to become albuminous.

By *Temporary Albuminuria*, I mean the manifestation and continuance of albumen in the urine during a limited period, and unconnected with any serious organic change in the kidney. This albuminuria, as occurring in the course of certain febrile or other acute diseases, I shall at present consider under the three following heads—*Desquamative Albuminuria*, *Inflammatory Albuminuria*, and *Critical Albuminuria*; and these I shall best illustrate by an immediate reference to the diseases in which they occur.

Under *Desquamative Albuminuria*, then, I shall speak shortly of the urine in Scarlatina, Asiatic Cholera, and Erysipelas.

Under *Inflammatory Albuminuria*, of the urine in the Dropsy following Scarlatina.

And under *Critical Albuminuria*, of the urine in Pneumonia and certain cases of Typhus.

First, then, of *Desquamative Albuminuria*, and of the urine in Scarlatina. In speaking on this subject, we have to deal with a mass of conflicting evidence; for unlike the condition of the urinary secretion in most febrile diseases, the characters presented by the urine in scarlatina have of late excited the attention of many accurate observers. Of continental authorities, I may quote the names of Martin Solon, of Simon, Romberg, and Phillip, with whose statements most of the members of the Society are probably familiar, and to which I need not allude more particularly than to say, that while some afford evidence of the frequent coagulability of the urine during the period of desquamation after this disease, others from very careful observations oppose it: thus Dr Frerichs,<sup>2</sup> in his very excellent work, says it is not the rule to find the urine albuminous in simple scarlatina. In a short paper, published in the "Monthly Journal of Medical Science," for January 1849, I gave the results at which, after careful examination of the urine in many cases of scarlatina, I had at that time arrived. Particularly referring to twenty-one cases, because the experiments in regard to these were free from all conceivable sources of fallacy, and were possessed of this additional value that they had been performed by me *coram publico*—in the Infirmary, in the presence of, and frequently aided by, my then fellow-clerks, more especially Drs Littlejohn and Absolon. These experiments and others led me to entertain the belief, "that if careful examination of the urine were made, albumen in small amount would be found to exist in every case of scarlatina." The prevalence of the disease in this city and in many parts of Scotland, about and since the same period, afforded opportunity for

<sup>1</sup> De l'Albuminurie, etc. Paris, 1838.

<sup>2</sup> Die Bright'sche Nierenkrankheit und deren Behandlung,—in a note at foot of p. 207.

the further investigation of the subject, which was not lost sight of. From Dr Fleming and Dr Chalmers, then house-surgeons of the Dundee and Perth Hospitals respectively, I early received communications corroborative of my own experience. In June 1849, Dr Patrick Newbigging communicated to this Society an interesting history of an epidemic of the disease, as observed in John Watson's Hospital, and from his own experience there in regard to the urine, expressed his concurrence in the belief I have just quoted.<sup>1</sup> I conclude this short historical account, by referring to a most interesting paper, from which I have derived much instruction, giving an account of an epidemic, observed under very much the same circumstances as that by Dr Newbigging, and also communicated to this Society by Mr Benjamin Bell.<sup>2</sup> Mr Bell's experience antagonises Dr Newbigging's and my own; the urine, however, was not tested from day to day—a condition I had been led to regard and to express as a *sine qua non*. Mr Bell, I feel sure, will pardon me if I now state, that, to a certain extent, the want of daily examination, in my opinion, invalidates his experiments. I am, however, willing to admit, that, even had the diurnal testing been executed, the albumen might not have appeared—an opinion held in deference to Mr Bell's, and to that entertained by others, as well as formed from my own more recent experience. In speaking a little more in detail, in regard to the urine in scarlatina, let me direct attention more particularly to the following points:—

I. The period of the occurrence and duration of the albumen in the urine, and its amount.

II. The microscopic characters of the urine with which the albumen is invariably associated.

III. The pathological import which its existence denotes.

IV. Whether it possesses any diagnostic value.

I. As to the period of its occurrence, etc. It is just after the commencement of desquamation of the cuticle that the albumen first makes its appearance. In most of the cases I have seen, the third and fourth days after desquamation had set in were the most common. I have found it, however, on the first day of desquamation, and as late in its appearance as the eighth and ninth: this was the case in the urine of a patient I had occasion to attend lately. I had been examining the urine from day to day, and finding no albumen on the eighth day after the desquamative process had commenced, I had begun to doubt the likelihood of its appearing, when on the morning of the ninth day I detected the albumen; it continued visible on the tenth, and passed entirely away on the following day. Here let me say a word as to the examination of the urine: in this, as indeed in all cases, both tests—*i. e.*, the application of heat, and the addition of nitric acid, *must* be employed; nor

<sup>1</sup> Monthly Journal of Medical Science. September 1849.

<sup>2</sup> Since published in Monthly Journal of Medical Science. August 1851.

must the examination be made once or twice, and only on those days when the presence of the albumen is deemed most likely,—it should be made every day, beginning before desquamation has commenced, and continued till that process is nearly completed. As a general rule, the duration of the albumen in the urine will be short, probably not longer than a few days; differences, however, exist in this respect,—I have known it to disappear in thirty-six hours, and have found it to continue for ten days. There is one interesting fact in regard to its continuance,—that after its disappearance it will not return, at least so I have always found; in other words, whenever the albumen was not detected after being first seen, its disappearance was a final one. The amount of albumen will generally be small, seldom more being present than to allow the urine to be called slightly coagulable,—a feature of great importance in distinguishing the urine of the simple from that of the dropsical scarlatina.

II. The *microscopic character* of the urine, with which the albumen is invariably associated, is the presence of a considerable amount of epithelium, derived from the different parts of the urinary apparatus. Sometimes the entire epithelial lining of the small tubes of the kidney was present, though certainly not frequently. I do not remember to have ever seen in the urine of simple scarlatina the albuminous or fibrinous casts of the small tubes of the kidney, the appearance of which is so common in the urine of the dropsical affection. Besides epithelium, the urine generally contained amorphous urate of ammonia, sometimes crystalline uric acid; and occasionally, though very rarely, the urine, though examined very soon after micturition, contained crystals of the ammoniaco-magnesian phosphate. In all such there existed a greater than usual amount of epithelium and mucous sediment. It is not uncommon to find octahedral crystals of oxalate of lime in the urine at the same stage of the disease.

III. The pathological import which the existence of albumen in the urine denotes. This is a point on which difference of opinion must still be expected to exist, seeing not only how very different are the facts recorded in regard to the occurrence of albumen, but how varying is the estimation of the importance which is awarded to its presence. While many believe its manifestation to be accidental, and of no importance, there are others who conceive it, if at the time unaccompanied with dropsy, to be its certain prelude. Both of these opinions I have attempted to show are erroneous, and, at least as far as my own observations go, founded on incorrect data. What, then, is the cause of albumen in the urine in simple scarlatina, and what its pathological import? I conceive it to be as essential a symptom of the disease as is desquamation of the cuticle,—to be associated to a certain extent with that desquamation,—to be, in fact, the result of a desquamative process, which the mucous membranes in this disease, equally with the skin, are subject to.

Granted, then, that this desquamation occurs, when such a change is taking place in the epithelial membrane lining the minute tubes of the kidney, the office of the cells composing which is to eliminate from the blood the matters, solid or fluid, which in the normal exercise of the renal function, compose the urine, it surely is not surprising that the albumen from the former should, to a slight amount, enter into the latter. Such I believe to be the cause of its occurrence; nor can I regard its presence as indicating any pathological condition, further than the separation of epithelial cells and their passage in the current of the urine. No symptoms referable to any such condition occur, no febrile reaction, no lumbar pain, no non-elimination of urine, no suppression of its watery parts, not even any diminution in its quantity, and, with the exception alone of the presence of albumen, no marked alteration in any of its sensible qualities. I have said that this albuminous condition of the urine in scarlatina is associated with the cuticular desquamation, it is so in the time of its occurrence, and so it is also as regards its amount; for I have noticed the albumen in the urine to be greatest in amount, and to continue longest, in those cases in which the process of desquamation had taken place to the greatest extent. In those cases in the urine of which no coagulability has taken place—for my more recent experience has shown me a few such—there has been no very marked desquamation, and no direct evidence of any epithelial separation, as shown by examination of the urine. We know that in many cases of scarlatina, especially in those where the eruption, though well-marked, has not been brilliant, extensive, or lasting, it is not uncommon for the desquamative process not to take place at all, or at most to a comparatively very slight extent. Such are the cases in which the coagulability of the urine will perhaps not occur. I say *perhaps*, for in some such I have, notwithstanding, found it. I am still, therefore, disposed to regard the temporary albuminuria of scarlatina as probably as frequent in its occurrence, and of somewhat of the same importance as a symptom, as the desquamation of the cuticle.

IV. Does the existence of albumen in the urine of scarlatina possess any diagnostic value? I shall best illustrate this point, by relating shortly the particulars of a case which came under my own observation, at the time I was engaged in making these investigations.

A female servant, æt. 28, was admitted, early in December 1848, into the Royal Infirmary, under the care of Dr Paterson, with whom I then acted as clerk. Her chief complaint was of sore throat, which, however, was less severe than for some time before her admission, and was then not very characteristic, and of feebleness and incapability for exertion. She mentioned that she had lately undergone much fatigue and anxiety, having acted as nurse to a lady, who died while she was so employed. I need not detain the Society by relating all the particulars of the case, suffice it to say, that a few days after admission the cuticle, which had been

dry, but the skin free from eruption, began to desquamate. The idea then occurred to me that, probably, this had been a case of scarlatina. I examined the urine; on the first and second occasions, and these on different days, detecting no albumen, but on the third found it slightly coagulable, and confess that, upon doing so, I felt less difficulty in deciding on the nature of the case. The albumen continued for three days, and no dropsy occurred. The interest of this case was increased by my being informed, on making careful inquiry, that the mistress of the patient, in whose house she resided, and on whom, when ill, she attended, was supposed to have died of scarlatina, before, however, the eruption had been fully developed, so that the servant had remained in ignorance of the cause of death. I conclude what I have to say in regard to simple scarlatina, by urging the importance, as well as interest, attending the existence of albumen in the urine of that disease. It is during that period when desquamation is taking place, not so much from the cuticle, I believe, as from the kidneys, that dropsy, from exposure to cold or other causes, is apt to be developed. Let regard then be had to the condition of the urinary secretion; till the stage of temporary albuminuria is gone by, the patient should be strictly confined, and no hygienic rule relaxed; but after that, even though the old cuticle is as yet only slowly separating, I am inclined to think the danger of a dropsical attack is passed; the renal function, never inactive, but only slightly involved, is again entirely healthy and efficient.

*Second.*—In regard to the occurrence of albumen in the urine in Cholera. The very frequent occurrence of albumen in the urine of persons suffering from this disease was, I believe, first noticed in the Cholera Hospital of Edinburgh, during the epidemic of 1848-49. At nearly the same time Dr Parkes and others in London found the first urine passed in cholera to be coagulable. In the end of spring 1849, the disease became epidemic in Paris, and the same character of the urine speedily attracted attention—Messrs Levy, Martin-Solon, Rostan, and others, making observations in regard to it.<sup>1</sup> During the prevalence of cholera in Edinburgh, I had, thanks to Dr William Robertson, the opportunity of examining the urine in nearly 100 cases.<sup>2</sup> On the general morbid characters of urine in cholera I shall not now enter, except in so far as these are connected with, or illustrate its coagulability. One of the most invariable symptoms of Asiatic cholera, is the entire, or almost entire, suppression of the urine; and one of the most favourable symptoms throughout the whole course of the disease, is the return, or the decided increase, in quantity of that secretion. The mode of fatal termination in many cases of cholera had satisfactorily shown, that the cause of death might reasonably be attributed to the existence of a poison in the blood, whose effects were produced in very much the same way as

<sup>1</sup> See Valleix Guide du Médecin Praticien. Tome deuxième, p. 707.

<sup>2</sup> See Monthly Journal of Medical Science for November 1849.



those of opium and other narcotics ; death in such cases, as those now referred to, taking place by way of coma. All who have seen any number of cases of cholera are familiar with the circumstances now alluded to ; and the painful experience of the almost certainly fatal issue of the cases which presented such symptoms cannot soon pass from the mind. Hopes of ultimate recovery given rise to by an unexpected rally from a state of prostrate collapse ; by a wasting diarrhoea checked, and urgent vomiting controlled, by a returned pulse and genial warmth taking the place of a deadly coldness of the surface ; were too often disappointed. Many such promising cases being cut off in the way already adverted to, it soon became manifest that the only certainly favourable symptom was the restoration of the urinary secretion. The urine, then, first passed in cholera is found to present the following characters :—To be dark in colour, muddy in appearance, to be deficient in specific gravity, to be generally acid in re-action—when treated with nitric acid, to manifest the presence of bile or biliary colouring matter—when tested by heat and nitric acid, to yield a precipitate of albumen ; to be remarkably deficient in urea ; and lastly, when viewed under the microscope, to contain a large amount of epithelium, derived from different parts of the urinary system, and generally one or other of the common crystalline deposits—most frequently uric acid. The morbid character most intimately connected with the mode of death adverted to, is the extreme deficiency of urea, the retention of that substance being, as we know, a frequent source of mischief in other diseases ; but the two characters which chiefly concern us in this investigation are, the presence of albumen and the deposit of the epithelium. In several examples the albumen existed in such amount as to allow the urine to be called highly coagulable ; but more generally the expressions, coagulable, or slightly or faintly coagulable, more correctly described it. The albumen continued present in general for some days, usually decreasing in amount, but occasionally increasing for a day or two. This coagulability of the urine was associated invariably with the presence of a large amount of epithelium ; as in the case of the urine in simple scarlatina the epithelium was derived from the bladder, as well as from the kidney ; it was, however, more common in the cholera urine to find the entire epithelial lining of the minute tubes. Attentive observation enabled me to note these further points ; the amount of epithelium, and the degree of coagulability of the urine, always stood in exact ratio the one to the other. They generally appeared together, and again in company disappeared. I have found the epithelium present alone before the albumen appeared, but have never observed the opposite case. Again, the period of the disease at which the albumen and epithelium in the urine appear, is an interesting consideration in regard to their cause and pathological import. I have just referred to the favourable nature, as a symptom, of the return of the urinary secretion. It is indeed just as the period of the resolution of the disease is arrived at, just as con-



valescence begins, as bile returns to the stools, and as the general appearance of the patient commences to improve, that the first urine is passed; and these are the characters it presents. Now I have called the albuminous urine of simple scarlatina a *desquamative* albuminuria, and for the same reasons I call the passage of albumen in the urine of this disease a *desquamative* albuminuria. The examination of both most emphatically indicates the progress of a desquamative change in the lining and secreting mucous membrane of the kidneys; and both diseases afford the evidence of desquamation taking place in other parts of the system. The desquamation of the cuticle in scarlatina is not more constant than that of the mucous membrane of the intestinal canal is in cholera. These peculiar features of the urine in cholera I have always found best marked in the severest cases of the disease. There remains one other point of still higher consideration, because beyond its mere interest there is a weight of value, namely, that by a just appreciation of these evidences furnished by the urinary secretion, means may and have been adopted, whereby the condition of the patient may be improved, happily his restoration from otherwise certain death secured.

*Third.*—Albumen in the urine of erysipelas. I need not occupy the attention of the Society long with this example. I have found that after severe attacks of idiopathic erysipelatous inflammation, and most frequently when a large surface of the skin has been affected, that the urine has, during the progress of convalescence, become albuminous. I do not regard temporary albuminuria as so invariable or frequent a symptom of erysipelas, as I conceive it to be of scarlatina, at least I have not found it so. But since my attention was directed to this subject, I have found albumen in the urine during the early progress of convalescence from a large number of severe cases of the disease, more especially when these two symptoms had been present,—severe gastric or intestinal irritation and derangement, and considerable desquamation of the cuticle. The quantity of the albumen present was never great; the period of its occurrence was at the resolution of the disease, as convalescence commenced, and during the progress of desquamation. It was, as in the other examples of albuminuria I have already referred to, invariably associated with epithelium, affording evidence of desquamation; but more closely resembling the urine of scarlatina, in being less charged with this ingredient than that of cholera. The question arises, that seeing this temporary albuminuria is a symptom of certain of the exanthematous diseases, is the fact of the frequent occurrence of albumen in the urine of erysipelas, when its symptoms most nearly resemble those of that class of diseases to be considered as at all proving that identity?

In the interesting paper<sup>1</sup> read to the Society at its last meeting, by Dr Alexander Wood, the relations of erysipelas with scarlatina were

<sup>1</sup> Since published in Medical Times and Gazette. July 1852.

sought to be established by a reference to other but very important facts. The temporary albuminuria which occurs in the course of both adds, I think, another link to the chain of connection and relation.

In now taking leave of the subject of desquamative albuminuria, I have only to add, that the three examples now considered are not the only ones I could have adduced. There are others, of which variola and certain febrile affections of the skin are instances; but the three I have selected differ in no very marked degree from these and others, while they have served sufficiently to illustrate the temporary albuminuria dependent on desquamation. I have, it will further be observed, not claimed for the urine in these three diseases an entire similarity in their characters; it is sufficient for my purpose, if you agree with me in thinking that they so nearly correspond in certain particulars, that, setting aside their differing characters, I am entitled to conclude that the *temporary albuminuria* common to the three arises from the same cause in each.

And now, *Secondly*, I have to consider the case of *Inflammatory Albuminuria*, which I propose to do very shortly, by reference to one example—the *dropsical disease following scarlatina*. Every one who has paid attention to the condition of the urine in this most interesting affection, must have noticed the great dissimilarity subsisting between its external and other characters, and those of the urine in simple scarlatina; while in the latter, the amount of urine passed, except during the continuance of febrile symptoms, is undiminished, one of the most certain forerunners, as it is always the most invariable accompaniment of dropsy, is the excessive reduction of the quantity of urine. This urine, when further examined, is found to contain a large amount of albumen; while, under the microscope, frequently blood, not unfrequently exudation corpuscles or compound granular cells, always much epithelium, and the fibrinous casts of the renal tubes are recognised. The symptoms which accompany these changes in the urine are generally well-marked, the most prominent, save the dropsy, being a very uneasy, often severe, lumbar pain, and marked febrile excitement. But, independent of these general symptoms, it will, I think, be admitted, that the characters presented by this urine, while they differ from those of the urine in simple scarlatina, indicate also the existence of a much more serious change in the secreting mucous membrane of the kidney, than a merely desquamative one. In order, however, to arrive at a correct opinion in regard to the pathological importance of the change undergone in the kidney during the dropsical disease, it is necessary to bear in mind both the symptoms presented by the patient and the hints afforded by the characters of the altered urine. These taken together give evidence of general febrile excitement, and of renal congestion, inflammation, and exudation. I have examined the urine in many such cases, and have found the albuminous condition much more lasting than in the simple cases,—indeed observation and experience show now pretty plainly that the long-continued albuminuria of drop-

sical scarlatina may, and often does, lead imperceptibly—insidiously it may be—to organic renal disease. In many instances I have found the albumen, though large in amount, and associated with all the general and local inflammatory symptoms alluded to, speedily and entirely disappear. I have not seen many cases of the dropsy following scarlatina, which I had watched from the commencement of the primary disease, but I have seen a few, and in all such the dropsical and aggravated symptoms appeared at the time the temporary albuminuria was going on, and were evidently the result of exposure to cold. This variety of albuminuria, then, which I have called inflammatory, may or may not be *temporary*: it is to be feared that not unfrequently neglected, or even unskilfully treated, the affection it accompanies lays the foundation of permanent renal disease. In most cases, however, it is fortunately otherwise, while in nearly all it may be looked upon as, under judicious management, a curable disorder.

*Thirdly.—Critical Albuminuria.*—1. *Pneumonia.*—Becquerel,<sup>1</sup> Simon,<sup>2</sup> Andral,<sup>3</sup> Finger,<sup>4</sup> and others, have all noticed the not unfrequent occurrence of albumen in the urine of pneumonia. Several observers in this country, though perhaps on a less scale, have done the same; in particular, Dr William Aitken<sup>5</sup> has recorded several cases of pneumonia in which albuminuria occurred. The peculiar appearance presented by the urine about the critical period in acute pneumonia is well known. The urine, perhaps clear and transparent, perhaps even pale in colour, though in general not remarkable for any of these characters, becomes at that time almost suddenly dark and muddy, loaded with amorphous urates, which speedily subside in the form of a dense deposit. This characteristic is known to all attentive observers, but I do not think that it is so generally known, that that urine, cleared as it is by the first application of heat from the solution of the urate of ammonia, is, by a continuance of the heat, and by the addition of nitric acid, caused to manifest the presence of albumen. Such, however, I have found to be the case in a large number of instances. It is undoubtedly, as Schönlein and others have pointed out, by the increased energy of the kidney that the mass, in many cases very large amount, of exudation poured into the substance of the lung is got rid of. That exudation is found in the urine in a very different form. The deposit being for the most part composed of amorphous urate of ammonia, of uric acid, and of a large number of very small molecules or granules, which are unaffected by heat, and unaltered by acetic acid, and which are, I think, to be recognised as the debris of the exudation,—finally, of albumen, deposited by heat, or on the addition of nitric acid. These are the

<sup>1</sup> Semeiotique des Urines, p. 327.

<sup>2</sup> Chemistry. Sydenham edition. Vol. ii. p. 214.

<sup>3</sup> Quoted by Becquerel, Semeiotique des Urines, p. 332.

<sup>4</sup> Präger Vierteljahrsschrift, 1847, No. iv.; also Monthly Retrospect of Medical Sciences, edited by Drs Fleming and Gairdner. Vol. for 1848, p. 66.

<sup>5</sup> Edinburgh Medical and Surgical Journal, No. 178.

ingredients of the deposit found in the urine of pneumonia, this the manner in which the inflammatory exudation to a certain extent chemically transformed, but to some degree having only undergone a breaking down, is as effete matter discharged from the system. In respect to the period of occurrence of the albumen, I have already mentioned, that about the crisis of the disease is the time I have detected it.

By critical period in pneumonia, is meant the time at which resolution begins, when the exudation, which had rendered a portion of the lung useless—impermeable to air—is being got rid of. The occurrence of the albumen in the urine I have, on a few occasions, noticed a day or two before the more general deposit appeared. This, from observation, I was led to regard as a very favourable symptom,—it certainly was a very interesting one, because, just as certainly as the returning crepitation, and the less dull sound on percussion over a condensed lung, indicate the breaking up of the exudation, and the return of the air to the previously closed vesicles, did the albumen in the urine advertise the approach of the more dense deposit, consisting of the amorphous urates, etc. The continuance of the albumen is very variable. I have never known it to disappear under five or six days. In chronic pneumonia, more especially when the disease has advanced slowly, and when, as is not unusual, a considerable portion of the lung is affected, and when, as is certain, cure is tedious, resolution slow, I have found the duration of the albumen longer than in acute pneumonia. In such instances I have known it to continue for weeks; it did so in one most interesting case, to which I shall presently refer. As the duration of the albumen is variable, so is its amount. It was always present unmistakeably, allowing no doubt of its existence; often it was present in considerable amount, not unfrequently in very large. Such are the facts I have observed in regard to the existence of albuminuria in pneumonia, or rather in the convalescence from it. In regard to the frequency of its occurrence, I may mention, that in almost all the cases of pneumonia<sup>1</sup> admitted into the Royal Infirmary under the care of the senior physician during a period of nine months, and which I carefully examined, the appearances were such as I have detailed; and that the casual examination of many other cases in wards under the care of other physicians served to strengthen my belief in the almost uniform occurrence of the facts now noted.

I shall here give the particulars of one case,—that before alluded to.

In August 1848, a hale man, aged upwards of 60, was admitted into the Infirmary, with acute pneumonia affecting the inferior lobes of both lungs. The disease ran its course with marked severity, but at the end of fourteen days from his entrance to hospital the patient was convalescent. As I was in the habit of doing,

<sup>1</sup> Of at least eleven cases I have preserved notes, and these in connection with some other features of interest in the urine of pneumonia I shall hereafter lay before the Society.

in the case of every patient at that time, I examined this man's urine,<sup>1</sup> and found it highly coagulable; still it was passed in normal amount, and maintained a good specific gravity, while under the microscope it was found to contain a large deposit of amorphous urates, and a considerable amount of the granular matter I have already described. In this case the opinion first formed (after the discovery of the albumen) was, that he laboured under Bright's disease, and that the acute affection which caused his entrance into hospital was an example of one of those intercurrent inflammations which, whether affecting the pericardium, pleuræ, or lungs, we know to be common in that disease. The general symptoms of the patient, however, in particular his appearance and marked convalescence, together with his freedom from dropsy, and the characters presented by his urine, appeared to me to militate against the idea of his suffering from renal disease. Accordingly, I kept him under careful observation for some time longer, both while he remained in the hospital and after his dismissal from it; and examining his urine from day to day, perceived its characters to undergo those modifications I have described; and finally, found it to present those of a perfectly healthy secretion. Had this examination not been continued, or this cause of temporary albuminuria been unknown or unregarded, the probability is, that incorrectly appreciating the evidence afforded by examination of the urine, and particularly the detection of the albumen, instead of considering the coagulability as an evidence of the progress of a healthy action, we should unreasonably have concluded it to be that of the existence of a permanent organic change. To this albuminuria, then, I have given the title of *Critical Albuminuria*,—because my data being correct, and my conclusions justifiable, it is to be regarded as an evidence of a critical action, and commencement of a change undergone by a diseased part before its return to a healthy state. But I can further illustrate this subject by a reference to changes which occur in some cases of typhus fever. I have found albuminuria by no means an uncommon attendant on the convalescence from typhus; not, however, nearly so invariable in its occurrence as in scarlatina, or even so common as in pneumonia; so frequent, however, as to lead me to examine all cases in which it occurred, and that with very great care. The result has been, that no one of any such cases has, either at the time or during a considerable period of observation afterwards, afforded the evidence of any organic change in the kidneys to account for the albumen in the urine.

The albuminuria in the case of typhus appears to me of special interest, as occurring much more frequently, if not entirely, in certain cases of typhus. It is in those cases in which we know, or have reason to suspect, that the deposits, generally called typhus deposits, have taken place in internal organs, that we find albumen in the

<sup>1</sup> The first examination had not been made till the 14th day of the disease.



urine. Two or three observations of a somewhat different nature have led me to this conclusion ; for example, I have found the urine albuminous in cases of abdominal typhus,—that is, in those cases in which we generally find severe diarrhoea as a symptom during life, and deposit in the intestinal glands as the most prominent lesion after death. In several cases of this kind, which proved fatal, I have found albumen in the urine for days before death ; and in others, which happily recovered, I have as frequently noticed its occurrence. In both these instances the albumen appeared, for the most part, at an advanced period of the disease, at least after the particular symptoms had continued for some time ; while in the former the albuminuria continued up to death ; in the latter, in some it disappeared as convalescence was fairly established ; and in others it lasted for a longer period. The amount of albumen in these cases, and the other characters with which the coagulability was associated, were exactly as I have described them in the example of pneumonia ; and finding the albuminuria to bear a relation to the deposits in internal organs in typhus, I have been led to regard the kidneys as the emunctories by which the morbid matter so deposited is to a certain extent at least removed from the system,—and so doing, to regard the temporary albuminuria of typhus as a critical albuminuria. It is, I think, no objection to this view that deposits, such as those referred to, remain in organs for a lengthened period ; for, firstly, I do not think we can pretend to limit the period of their removal or disappearance ; and I am inclined to believe that, when they do so disappear, the urine will very probably contain the ingredients I have noticed ; and secondly, the calcareous masses found in the spleen, and other organs, accepted as the earthy remains of the deposits spoken of, certainly attest the removal by some channel or other of the animal matter, of which, in their original condition, these deposits were partly composed. This is an interesting subject, and invites further inquiry.

And now, in conclusion, I have thus brought under the attention of the Society some of the various causes, as I have observed them, of temporary albuminuria, more particularly as occurring in the course of certain febrile or other acute diseases. Let me again state, that I am far from supposing that in this consideration I have included all its various causes, or the diseases in which it may occur. There exist several others, as puerperal fever, phthisis, etc. : these, however, I have not hitherto been able to observe in numbers sufficient to justify any deductions, though from what I have seen I feel inclined to attribute the albuminuria in these, and some others, to a blood change. Temporary albuminuria occurring in the course of certain diseases has in this paper been considered under three heads—Desquamative, Inflammatory, and Critical—and to one or other of these three I have referred the coagulable urine of scarlatina, cholera, and erysipelas, of dropsical scarlatina, and of pneumonia and certain cases of typhus fever ; it remains for the Society to consider whether



or not these divisions are authorised.' The study of the morbid qualities of the urine is one of confessedly great interest and importance, and so also is the pathology of the kidney: both of these have individually received great attention,—not, however, sufficiently in respect of the relation to, and dependence of, the one upon the other. It is in this direction that our labours must now advance; and doing so, we cannot fail to arrive at facts equally instructive as important.

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ARTICLE II.—*Remarks on the Yellow Fever which appeared of late years on the Coast of Brazil.* By WILLIAM M'KINLAY, M.D., late Surgeon of H.M.S. "Cormorant," employed on that Coast.  
—(Continued from p. 274.)

EPISTAXIS was the only form of hemorrhage observed in the "Cormorant," and none of the cases in which it was noticed recovered.

There was no remission observed in the mild cases, nor in those that proved rapidly fatal; even in the other cases they were by no means distinct or regular. When observed, the stage of pyrexia lasted generally about thirty-six hours, and the remission about twelve. In others, again, there was a daily remission in the morning, with an increase of all the febrile symptoms in the evening.

Black vomit was observed in seven of the fatal cases; one of these at one time gave hopes of recovery, having been two days free from febrile symptoms; the same man had epistaxis also, but mortification of the whole arm carried him off on the seventeenth day of the disease.

One boy, of 17 years of age, made a perfect recovery, and remained in the ship until paid off in 1852, after having black vomit in my own presence. Two others—stokers—who had very severe attacks of fever, among the first attacked, were said by the attendants to have vomited some "black stuff." At first this black vomit resembled albumen, or thin arrow-root, tinged black with very fine charcoal, the particles of which, when narrowly inspected, had a scaly shining appearance; this matter was brought up with an effort, as in ordinary vomiting; by degrees it came to resemble "coffee grounds," was gulped up without an effort, and sometimes spouted from the mouth to a distance of several feet. It was frequently observed also, both on shore and afloat, during this epidemic, that in many of the fatal cases which did not show black vomit before death, matter precisely like it was found in the stomach after death.

The first symptoms generally complained of were headache and vertigo. A distinct rigor was not observed in any case; but alternate chills and flushes of heat were common symptoms at the commencement.

<sup>1</sup> For an account of an interesting discussion which followed the reading of this paper before the Medico-Chirurgical Society, in which Dr Christison, Dr Andrew Wood, and Dr W. Gairdner took part, see the *Monthly Journal* for July 1852, p. 92; also the "*Medical Times and Gazette*," June 19, 1852, p. 623.

One class of cases presented themselves with severe headache, much giddiness, a pale anxious countenance, great prostration of strength, inability to stand or to hold the head up, the chin resting on the sternum, a red eye, a frequent and labouring pulse, hurried respiration, much mental alarm, and the temperature of the surface under the natural standard; lumbar pains were sometimes present also, and generally a wish not to be disturbed, with a disinclination to answer questions. Head symptoms, or a tendency to coma, prevailed in these throughout, unless the efforts against this train of symptoms were successful; and when they proved fatal, the person died comatose. This was not only the most fatal form of the disease, but also the form most rapidly fatal. The flies remained on the face undisturbed as the disease advanced, and towards the close occasional violent screeching with jactitation were present; and in all the forms of the disease in the "*Cormorant*," the mental faculties remained entire until late in the disease.

Another class of cases had high febrile action from the commencement, intense headache, vertigo, a hot dry skin, full firm pulse, severe lumbar pains, urine diminished and high coloured; the remissions were more distinct in this form of the disease than in any of the others; and much intestinal irritability (if not inflammation of the intestinal mucous membrane) was apt to appear, as evidenced by much vomiting, purging, red tongue, which gradually assumed a dry, red, and glazed appearance, by more or less pain at the epigastrium, and sometimes of the abdomen generally, on pressure. This train of symptoms, if not early counteracted, soon caused much exhaustion, generally continued until within a short period of death, which took place by asthenia. Suppression of the intestinal secretions was a prominent symptom in this class of cases, in the absence of the symptoms of intestinal irritability indicated above. The strongest purgatives, often a repetition of them, were required to move the bowels; and constipation was often a troublesome symptom during convalescence, when the bowels would not act without medicine; and if not kept moderately open, tenderness of epigastrium on pressure, and often of the abdomen generally, with other disagreeable symptoms, were almost certain to supervene, and to continue until the bowels were again freely moved. It was in this class of cases, which was also the most numerous, that hepatitis so often followed the febrile attack.

A third class of cases appeared to show a combination of the symptoms of the two previous classes, less prostration and oppression than in the first class, and less arterial action than in the second; there was a tendency to coma, the fever advanced through its stages at a slower rate, it was long before a state of apyrexia appeared, and convalescence advanced very slowly.

Those cases which occurred after the 20th. April, at Monte Video, commenced with catarrhal symptoms, coryza, cough, etc. They resembled common catarrh so much—many cases of which were pre-

sent in the ship at the time, the great diurnal difference of temperature no doubt favouring their existence, and the remission in the occurrence of fever cases during the previous three weeks made me so confident that the disease had disappeared—that at first I looked upon these as cases of catarrh. The rapid progress of the disease, however, soon undeceived me; and much arterial action, with intense heat and great dryness of skin, followed these symptoms so rapidly, and to such a degree, that I deemed it advisable to have recourse to venesection to subdue them. This was followed with the best results in all, except the two fatal cases; in them the advantage was only temporary, and was followed by rapid sinking. The blood abstracted in these cases did not coagulate, but remained of a dark colour, and of a tremulous jelly-looking appearance; after death they showed the same ecchymotic appearance about the upper part of the trunk, which had been observed in the former cases; and I afterwards ascertained that the same train of catarrhal symptoms had appeared towards the conclusion of the epidemic at Rio, that the practitioners there had thought it requisite to bleed also, and with much the same result as in my own cases. The symptoms varied a good deal in different situations; for instance, in H.M.S. “Tweed” early delirium was rather common; in some cases I believe it was the first symptom noticed in that ship; while in other ships, as well as on shore, suppression of urine was a common and of course a very fatal symptom.

The fever never assumed an intermittent character in the “Cormorant;” indeed there was not a case of ague in the ship during all the commission.

In regard to the symptoms, Dr Fairbanks says, “One peculiar symptom was very remarkable, and observed in almost every case, that was a constant dilatation of the pupil, and this was observed in many persons at the time not affected with fever. The fever in foreigners was almost universally continued; exacerbations and remissions were very seldom observed. In some it would be merely ephemeral; in others, in two or three days, health would be re-established.” “The pulse almost always very little affected.” “Yellowness of the skin was not observed in perhaps more than one-third of the cases, and not always so, even after death.”

“The force of the disease appeared to be expended on the stomach and intestinal canal.” “People from the interior of the country, who came to the city during the epidemic, suffered severely, and many died.” “The great mortality was confined to the shipping and unacclimated foreigners; of those attacked about one in three died.” “Of black vomit only three cases recovered in my practice.” “The disease was most severe in the months of December and January, gradually declining, although with occasional exacerbations, until July, when it ceased altogether.”

I ought to have mentioned, when describing the symptoms, that

in those cases in which the intestinal irritation, already referred to, occurred, a very deceptive interval usually intervened between the decline of the febrile symptoms and the appearance of these abdominal disturbances. It was of variable duration, from two days upwards, and during this interval there was nothing to indicate the approach of danger; on the contrary, there was every appearance of a steady progress towards recovery.

Cases of headache, and slight pains across the loins, were common during the prevalence of this epidemic, and for some time afterwards, which never attained sufficient severity to demand a cessation from duty. These often applied for aperient medicine; often one dose of a saline aperient was sufficient to remove these symptoms permanently; often also this effect was only temporary, and a repetition of the medicine was necessary; and in several cases a very little exposure to the direct rays of the sun, even for months afterwards, caused a return of headache, etc. In short, there was every degree of indisposition, from the slightest headache to the fatal black vomit.

Without venturing far on theoretical grounds, we surely have, in the history of yellow fever, full evidence of its being a blood disease,—that the blood, in fact, is poisoned; that this poison, when concentrated or in large quantity, acts narcotically. In severe cases the blood does not coagulate properly, when drawn from a vein; it is so fluid that it exudes sometimes from the mucous membranes in large quantities; and a sanguineous exudation has been observed even on the external surface of the body. The cases of the disease in which somnolency, or a tendency to coma, is observed, are the most deadly, and the most rapidly fatal. In the same cases there is often suppression of urine—a symptom of comparatively recent observation, and perhaps not sufficiently attended to in many instances in observing and describing the disease, as it is seldom noticed by the patient, and, unless particularly inquired for by the medical attendant, may not be observed,—so that we are not in all cases to infer its non-existence when not described. This tendency to coma, whether the direct result of the absorption of the poison into the blood, or the indirect result, the poison first causing suppression of urine, the urea being retained in the blood and causing the comatose state, is a matter which demands more investigation. And it may be doubted whether the intestinal irritability, so often alluded to in these remarks, may not be caused by the poison being eliminated from the system by the intestinal mucous membrane.

When the great irruption of disease into the “Cormorant,” on the 22d March, is considered, the malignant type of that disease, and the consequent mortality, it may appear strange, that out of 145 people in close proximity, under almost precisely similar circumstances, only forty-two cases of fever should occur. This proportionally very small number of attacks is, in my opinion, to be attributed to our going so promptly and so rapidly to the southward. The latitude of Monte Video and the Cape of Good Hope is nearly the same; the climates

of both places are similar to a degree ; the same diseases prevail in them, and it would be perhaps difficult to point to two places on the globe where febrile diseases prevail to a smaller extent (see Major Tulloch's "Military Medical Statistics"). Had we remained within the tropic, or, what would have been still worse, had we remained long in Rio harbour, I feel confident it would have fared little better with us, perhaps worse, than with the "Eclair" in 1845.

And yet, notwithstanding that the poison causing the disease, whatever it may have been, appeared to be so concentrated in Rio harbour, in the middle of March, some ships escaped the disease. When H.M.S. "Cormorant" arrived in the harbour on the 16th March, we found that H.M. brigantine "Spider" had been there for about a week before us. She sailed on the same day that we did. We in fact towed her out of harbour. She was at anchor about half way, and in a direct line between us and the city. So much was she supposed to be in a more dangerous position than we were, that Captain Schomberg, being senior officer, had serious thoughts of ordering her to move to a position more distant from the city. Communication between her and the shore was not at all interrupted. She was also in the harbour at various other times during the prevalence of the epidemic, and never had a case of fever.

The United States' ships of war "Brandywine" and "St Louis" arrived from the Rio de la Plata at Rio de Janeiro in company, early in March. They remained about three days, sailed together for Monte Video, had been at anchor in the same part of Rio harbour ; the "Brandywine" lost three officers and six men, the "St Louis" did not get the disease at all. The United States' ships of war generally acted very prudently in avoiding Rio harbour as much as possible during the prevalence of this epidemic, and suffered less from the disease than the ships of other nations. The French ships of war were employed chiefly in the River Plate, and suffered little also. The steamers "Archimedes" and "Prony," with troops on board, touched at Rio on their way to Monte Video ; both had more or less fever, but in the latter ship it seems to have been confined to the seamen, the soldiers escaping.

In regard to other national ships, there is nothing to call for any remark, as far as my knowledge extends. H.M.S. "Crescent"—a dismantled frigate at anchor in Rio harbour, was used as a depot for captured negroes, and, when not required for that purpose, used as a store ship, a prison ship, and occasionally as a hospital ship for the more severe cases of disease in the squadron. This ship had no means of escaping from the disease by going to sea ; but, under the able medical superintendence of her experienced surgeon, Dr Ellis, who laboured through this epidemic as well as that of 1851, without assistance, the mortality in her was much smaller than might have been anticipated under the circumstances. I am not aware of any man-of-war, except those already mentioned, that remained any length of time in Rio harbour during the prevalence of fever without getting



a few cases of it; nor am I aware of any that suffered to the same extent as the ships I have referred to.

The mortality at Rio has been variously estimated: the statement most generally credited says, that 15,000 died between the 1st December 1849 and 1st September 1850; and if the city, suburbs, shipping, etc., are included, I believe the estimate is as nearly correct as possible. In the "*Jornal de Commercio*" of 5th September 1850, however, Dr Chernoviz, a native of Poland, in practice at Rio, says that only 3,827 died of fever, including both slaves and free. This statement is so much at variance with all others, that I suppose some mistake must have been made.

The natives of northern countries, more especially if recently arrived from those countries, were the greatest sufferers from the fever at Rio. Norwegians and Swedes suffered very severely; so did the British, Portuguese, Sardinians, and Belgians. The acclimated of all nations escaped longer, and suffered much less than the unacclimated of the same nations. Whole families of newly arrived Portuguese perished. The Italian Opera Company lost seventeen; and Italian pedlars, of whom there were many at Rio, almost entirely disappeared. "The Norwegians, Swedes, and Finlanders, constituted the melancholy trio which furnished the greatest number of victims. The crews of many Swedish and Finland vessels died almost systematically one after the other, and almost always after suffering from three to five days." So says Dr Lallemand, and I have every reason to believe his statement to be quite correct. The passengers who arrived at Rio by the Havre de Grace packets during the fever suffered very severely; often more than the half of them died in less than three weeks from the time of arrival. The native Brazilians, no matter what shade of colour, suffered least of all; and the blacks had the disease very generally, few of them having escaped, as few had it in a severe form, and very few, indeed, of them died. From fifteen to twenty-five was the most fatal age; and females had no exemption from the disease. There is reason to believe, although it would be difficult to furnish a sufficient number of cases to prove it, that persons formerly acclimated in Brazil lost the protecting power of that acclimatization by a residence since of more or less duration in a temperate climate.

It is well known in those of our colonies, often visited by yellow fever, that natives of this country who have become acclimated in them, if they revisit this country and remain for some time, they are very liable to have a severe attack the first sickly season after their return to the colony.

The subject of the importation and contagion of yellow fever has been so often and so keenly disputed, that it will be sufficient here, I hope, to give my own opinions, and any observations on the particular epidemic in question that occur to me, as impartially as a subject so much contested will admit of.

I have already referred to the meteorological changes which have



been so very generally observed on this coast of late years, by non-professional people more than by medical men; and also to the greater frequency of dysentery and hepatitis in her Majesty's ships employed on the Brazilian coast during the last ten years, as noticed by myself before the appearance of yellow fever. The polka or insolation fever, which appeared on the coast about six years ago, which commenced every year at the same period regularly, at which the fatal yellow fever made its appearance, has also been referred to, with the prediction of Dr Lallemand as to the possibility of its becoming developed into a yellow fever under certain circumstances; and also the statement of Dr Ellis, that this polka had yearly acquired greater potency, until the end of 1849 showed a fatal yellow fever epidemic. Of this epidemic the polka might be said to be prodromic, or perhaps even a milder grade of the same disease; and I question much if the polka would ever have extended its name into the domain of the medical nosologist, if Brazil had been known to be frequently or even occasionally visited by remittent or yellow fever.

We have also the statement of Dr Pennell, that ships from Europe got the disease before entering any Brazilian port. "Several masters of vessels, without being questioned, declared that they entered the harbour with fever on board, although direct from Europe; that as soon as they approached the coast, and came within the influence of the breezes from the shore, their men fell sick with fever. Most of the cases were slight, but some were attended by black vomit, and proved fatal after their arrival in port." I believe I am correct, however, in stating, that the ship in which these cases of black vomit occurred communicated with, although she did not anchor at, Pernambuco, by landing a boat with her master there for a very short time, but that neither the master nor any of the boat's crew had fever subsequently.

The same circumstance was noticed also at Bahia regarding vessels arriving there. Dr Fairbanks says, "The captain of the brig 'Jesse,' on her passage from France to Brazil, was attacked with similar symptoms sixty leagues distant from Pernambuco." But Dr Paterson soon found out that "these cases were merely from attacks of the mildest nature, and not yellow fever." We ought to be aware, however, in weighing the evidence, that Dr J. Paterson is a strenuous supporter of Sir W. Pym's views of yellow fever.

The various reports about the prevalence of yellow fever in Brazil in former times have been noticed, and the unsatisfactory result of our inquiries upon this subject; but we can have no doubt that it has been a frequent as well as a fatal disease, epidemic as well as sporadic, in Guiana, on the north coast of South America. We have an excellent account of a severe visitation from this disease in British Guiana of late years by Drs Blair and Davy. We know that the disease has prevailed fatally, and not for the first time, at Cayenne, in French Guiana, in the end of 1850 and beginning of 1851; and can it be very extraordinary that the boundary line of

Brazil should not always be an effective bar to the extension of the disease? particularly when the contiguous districts of both Guiana and Brazil are so precisely alike in climate, elevation, soil, etc.

It is true, that severe epidemics of this disease have not been usual or much noticed, if ever present, in Brazil; but that they were not so appeared to surprise every one who attended to the subject, who often endeavoured very unsuccessfully to find reasons in the topography, meteorology, etc. of the country to account for the non-appearance of tropical fever in it. This being the case, it need not now be very surprising that this anomaly should no longer exist, however much surprised and astonished many were at the time when they found the disease actually present among them.

It is undoubted that the epidemic of yellow fever, of which we are now taking notice, appeared first at Bahia, and an effort was made there to prove that the disease had been imported by the French barque "Alcyon;" so thoroughly without foundation, however, was this attempt, that it was entirely given up by the contagionists even. They then looked out for another vessel, and found that the brig "Brazil," under American colours, had arrived at Bahia on 30th September 1849. At that time ships from Europe had to perform quarantine at Bahia on account of cholera prevailing in Europe. The "Brazil" got pratique at once, as she had a clean bill of health from New Orleans, the port of departure, and the last port touched at according to the account she gave of herself. On the 2d of October an anonymous letter appeared in a newspaper, the "Correio Mercantil," blaming the health-officers for giving pratique to a vessel from New Orleans, where not only "the terrible cholera, but the usual scourge of yellow fever was raging," and asking, "Is there some magical difference in the houses to which the different vessels come consigned?" and accuses the health-officers of "carelessness, want of reflection, or bribe." The animus of the writer of this letter is easily perceived, as well as its object, and both are sufficient to injure its credibility materially.

A voyage from New Orleans to Bahia occupies from thirty to seventy or seventy-five days, on account of the opposing trade-wind and equatorial current. The "Brazil" was seventy-five days from New Orleans. The anonymous letter above referred to is the only authority we have for the existence of yellow fever at New Orleans; but supposing, for the present, that it did rage there furiously, that the "Brazil" got the disease there in the most concentrated form, such a long voyage entirely within the tropics, indeed close to the equator, was surely sufficient for the disease to run its course, if strongly contagious, through the crew of the vessel, to expend its fury, to burn itself out for want of fresh fuel, and for the vessel again to become healthy before her arrival at Bahia. There is little doubt that she was healthy at the time of arrival, but she lost two of her crew on the passage; and there was an attempt made afterwards to prove, that the "Brazil" had touched at Havanna, that the two

men who died had black vomit, etc.; but after considerable inquiry at Bahia into the subject, I have no hesitation in saying, that in proof of these points, we have only assertions subsequently advanced by the contagionists to support their own views: it has even lately been found out that she took passengers on board at Havanna and landed them at Bahia;—where is the proof?—and that the “Brazil,” previous to her arrival at Bahia, had landed a cargo of slaves at Havanna. This attempt to associate the importation of the disease with the notorious traffic in slaves has so laudable an object, in one view at least, that we ought not perhaps to quarrel with it; but I believe there is no foundation for the statement. The “Brazil” was sold soon after her arrival at Bahia to a Spanish company; the transfer of the vessel, change of flags, etc., to take place on the coast of Africa, for which place she soon sailed, heavily armed, with a crew of sixty men, and *then* returned to Cuba, where she landed 600 slaves, and, it is said, went again to the African coast for more. This much is certain, that her American crew left her at Bahia, were dispersed, and, with the exception of her master, nothing more is known of them. The vessel had no fever at Bahia, nor is any of her crew known to have had fever afterwards; but her American master, although he never had the disease himself, is supposed to have caused all the mischief which subsequently occurred at Bahia, and along the coast of Brazil, by communicating a disease which he never had, perhaps had never seen, to a Brazilian boy whom he had never seen perhaps, but who lived over a store frequented by this master. This story of the master was subsequently improved upon when it was found that some cases of fever had occurred prior to that of the Brazilian boy above referred to, and between whom and the master of the “Brazil” no communication could be traced. It was found that the “Brazil” took passengers on board at Havanna, that they landed in that part of Bahia called Santa Barbara, and there communicated the disease. This circumstance, however, was not found out until about nine months after the event; and after all it is only a slight variation in one of the bars of the same jingling tune, as the source is still traced to the “Brazil,” and, as usual, we have only assertions without proof. The “Brazil” is not even said to have been generally unhealthy on the passage to Bahia. Dr Fairbanks, in noticing this vessel, does not say anything about her touching at Havanna. “There was no sickness on board when she arrived, nor were any of her crew afterwards attacked except the mate, who died on the 8th or 9th November following, from apoplexy. A few days after her arrival in Bahia it was reported in the newspapers that two deaths from yellow fever had occurred during the passage; but this is positively denied by the consignees of the vessel, and other individuals well acquainted with the circumstances of the vessel, and who could have had no motive for concealing the fact had it occurred.” This is the origin ascribed to the disease by those who believe in its importation into Bahia; and let us recapitulate

the evidence on which we are required by them to give credence to their views. We have no evidence that yellow fever existed at either New Orleans or Havanna at the time of the "Brazil's" departure. We have no evidence that she touched at Havanna at all; we have no evidence that she had the disease on the passage, or at any period either before or after her arrival at Bahia, or that any of her crew had it at any time. But then we are told that "the prevalence of yellow fever in any unusual or marked form, either at New Orleans or Havanna, was by no means necessary to establish the point" that the disease was thence exported. If we were to disprove this absurdity some other airy phantom would be found to prop a tottering theory. Arguing against such theorists is like fighting a shadow; and if the "Brazil" could get the disease at either of the places mentioned without its being prevalent there, why not as well get it at Bahia?

So much for the importation of the disease into Brazil. Its diffusion along the coast is said to have taken place by means of contagion; and this we shall find more difficult to disprove for the following among other reasons,—that in a country with so much coasting trade, and so many ports as Brazil, the appearance of any disease at a sea-port must necessarily be preceded by the arrival of some ship by which the disease may have been imported.

The following tabular form will show the time when fever appeared at some places on the coast, the latitude of the places, etc. :—

Places in their order of Latitude.	Latitude.	Time when the Fever appeared.	Places in the "order of time" when Fever appeared.
Para,.....	1° 30' S.	Early in Feb. 1850	Bahia.
Maranham,.....	2° 30' "	In May 1851	Maceio.
Parahiba do Norte,..	7° 6' "	January 1850	Rio de Janeiro.
Pernambuco, .....	8° 3' "	21st December 1849	Pernambuco.
Maceio,.....	9° 40' "	Early in Dec. 1849	Parahiba do Norte.
Bahia,.....	13° "	2d November 1849	Para.
Guarapari, .....	20° 43' "	End of May 1850	Paranagua.
Campos, .....	21° 22' "	Early in June 1850	Santos.
Rio de Janeiro,.....	22° 53' "	9th December 1849	Mangaratiba.
Mangaratiba, .....	23° 7' "	Middle of May 1850	Guarapari.
Santos, .....	24° "	Early in April 1850	Campos.
Paranagua,.....	25° 30' "	Middle of Mar. 1850	Maranham.

This does not pretend to give all the towns on the coast that had the fever, only such places as I was able to obtain more or less information about; and I believe that the disease did not extend further to the southward on the coast than St Catherine, in latitude 27° 35' S., where there were only a few sporadic cases. One case has been referred to as having occurred in April 1850, at Portalegre in Rio Grande do Sul (30° S.); this was a very doubtful

case,—one report says that scarlatina prevailed there at that time, another that it was a bilious fever, while a third makes it out to be yellow fever. Several ships arrived at Monte Video with the disease on board; but it did not extend there beyond those ships. Campos I believe to have been the only place on the coast where the disease survived the cold season of 1850.

Maranham was long triumphantly referred to by many, as proving the efficacy of good quarantine laws, when properly carried out, in preventing the irruption of this disease in 1849 and 1850, in defiance of the existence of all the local causes supposed to attract or contribute to the appearance of the disease. After having been so successful in those years, they could not surely have been so very careless as to relax their quarantine laws, or the system of enforcing them, in 1851; yet we hear that the disease made its appearance there, and proved very fatal, indeed it was only declining there in July 1851.

It has been stated that “the order of time” and latitude in which different places on the Brazilian coast were attacked tend to prove that the disease is contagious, inasmuch as those places which had most frequent intercourse with Bahia got the disease first; for instance, Maceio is the only port of any consequence between Bahia and Pernambuco, and chiefly resorted to by the coasting trade. This was also the only place on the coast between the towns mentioned that had the fever. I have endeavoured to make the table given above as correct as possible; and I do not know that I could add to the information it contains. One remark may be allowed,—that all epidemic fevers usually first seize upon, and prove most fatal, in places densely populated, such as large crowded towns. All the towns of Brazil that had fever are littoral; and it may be asked, How far this fever is a littoral disease?

The French (slaver) barque “Alcyon,” already referred to, is said to have taken the disease to Pernambuco from Bahia. The contagionists appear to have been determined to make some use of this vessel. Four men were sent from her to the hospital of Dr Fairbanks, at the latter place, with fever, on the 4th December. One died on the 7th, another on the 9th, one returned on board on the 6th, and the other remained in hospital, where he finally recovered. The vessel left Bahia on the 8th or 9th, with her remaining crew healthy, arrived at Pernambuco on the 17th, and landed her sick at the French hospital; one man died there on the 19th December, when the rest were re-embarked by order of the authorities, and all communication with the vessel interdicted. The consul reports two cases on shore on the 21st December that had not the slightest communication with the “Alcyon” or her people; and if that ship had communicated the disease, one would naturally look for the first cases at the French hospital; they occurred, however, at a considerable distance. Another account says that the cases of



Dr Paton and Mr Pitt were the very first cases on shore; they occurred at the British hospital, in the quarter Boa Vista, the French hospital being in the quarter San Antonio, and neither they nor any one else at the hospital had the "slightest communication" with the shipping. The case of M. Lassere, to whom the "Alcyon" was consigned, was cited as an instance of direct contagious propagation. Mr Cowper, the British consul, says:—"When all communication with the 'Alcyon' was forbidden, M. Lassere offered this measure all the opposition in his power. To show his contempt for infection, he went on board the 'Alcyon' daily for several days, until he was laid up with fever, and died on the third day. His death created the greatest panic on shore. Up to this period the fatal cases had been confined to seamen and new comers; but M. Lassere was a man of robust constitution, had been resident in the country twenty-five years, and his wealth enabled him to obtain the first medical aid; notwithstanding all this, he died, and from that moment no one considered himself safe. He died at his country house in the village, or rather district, of Capunga, where my cottage adjoined his place. Capunga had been perfectly healthy up to this period, but within a week twenty cases of fever and five deaths had occurred." Again, Mr Cowper says:—"There is no lazaretto here, and no medical or any other assistance is allowed to be sent to the infected vessels; therefore, should the epidemic spread on board, the mortality would no doubt be dreadful." Under these circumstances how was M. Lassere allowed to visit the ship so often and so regularly? This requires more explanation; and supposing the poison of the disease to be diffused in the atmosphere, M. Lassere was exposed to this source of the disease, as well as to that in the "Alcyon."

Another case, much relied on as an instance of direct contagious propagation, is thus told:—"In the early part of the epidemic, a waggoner was sent into the city, from Engenhio Caruana, an estate five leagues in the country, belonging to Dr Domingos de Souza. Having finished his business in the city, where he remained many hours, the waggoner sickened on his way home; and shortly after his arrival there showed unequivocal symptoms of yellow fever, of which he soon died. Those who attended him were next attacked, and the disease speedily spread over the whole estate. So great was its mortality that Dr Domingos was obliged to beg aid from the neighbouring estates, which was refused upon the ground that the appearance of the disease on his own estate afforded so palpable an evidence of contagion. Mr Cowper is certain there was no disease on the estate before the return of the sick waggoner from the city."

For these two instances, and for several remarks in preceding pages, I am indebted to an able paper on the yellow fever of Brazil, read by Dr M'William, R.N., before the Epidemiological Society of London. I regret being unable to arrive at the same conclusions as



the doctor, but this does not diminish my esteem for his ability, industry, and acumen, which I entertain in common with the medical officers of the Navy generally.

At Rio de Janeiro, the fever was for a long time alleged to have been imported by the American barque "Navarre." She arrived there on the 3d December from Bahia, which place she left on the 24th November, with a healthy crew, and bringing no intelligence of the disease. The vessel was sold soon after her arrival, and her crew went into lodgings on shore. The vessel arrived in ballast. One of the sailors who had belonged to this ship, of the name of Andersen, had been some time lodging in a house, in the Rua da Misericordia, belonging to a man named Frank, where he had been ill some days before he was taken to the Misericordia hospital, on the 28th December 1849; and he died there on the 30th. But it was found that another sailor, of the name of Enquist, who had arrived, about a fortnight before, direct from Russia, by the brig "Volga," and who had also been ill some days, and had lodged in an apartment behind Frank's house, where he was a frequent visitor, was admitted into the Misericordia on the same day as Andersen, and died the following night; both these were concentrated cases of yellow fever. These were supposed to have been the first cases in the city; but another case was found out subsequently to have occurred in the Naval Hospital, on the 9th December, which has been already noticed.

Dr Lallemand appears to adopt the view that this disease extends itself by contagion:—"The disease fixed itself in the Rua da Misericordia, proceeding from the house above-mentioned, running almost without interruption from house to house, though with milder symptoms, and even passed into the narrow or cross streets, until that entire quarter had sickened; while during that and the following days not a single suspicious case occurred in any other part of the city. In the harbour the disease did not take any sudden leaps, but went slowly from ship to ship." "We see the first cases developing themselves, linked together as unquestionably as the closely united links of an iron chain."

Dr Pennell says:—"In many instances the newly-affected had either been on shore, or were known to have had intercourse with the sick; but in many others, and not a few, no communication, direct, or indirect, was known to have occurred, nor indeed, from circumstances of time and place, could have taken place." "In no other way than by supposing the disease to be of endemic origin can it be explained how the natives and the acclimated suffered so little. In no other way than by supposing this disease to be of endemic origin can it be explained how ships came into port with this identical fever on board."

Dr Lee says:—"It now extended to the suburbs, and gradually to places one, two, and three leagues from the city. Many who lived out of the city, and who took every precaution, and some even

went so far as to refuse to visit or receive visitors, and yet they were attacked with fever, and many died."

Dr Ross, of H.M.'s brig "Petrel," felt satisfied that the subjects of the cases in that vessel had contracted the disease by being present for a very short time in a house in Bahia, where the disease existed at the time. But these cases did not communicate the fever to others in the vessel, either passengers or crew.

Dr J. Paterson, of Bahia, finds no more difficulty in tracing the diffusion of the disease in Bahia and along the coast to contagion, than he does in tracing the importation of the disease to the "Brazil."

The following observations occur to me on this subject with reference to the "Cormorant." Two cases of fever happened in boys in the ship, in November 1849, soon after coaling alongside the wharf at Sauda, then the usual manner and place of coaling for our men-of-war steamers. These cases were returned under the head "Synochus," the type of the fever was not, however, decided; and I question much, had these cases happened at the time of the yellow fever or subsequently, if they would not have been returned with a different designation. One of them had to be sent home ultimately, fits resembling epilepsy having supervened in his case; and the other, whose case was very similar to one of those many attacks of mild fever, which occurred in the ship subsequently when yellow fever was prevalent, returned to his duty after thirty-three days. So convinced was I that these cases had their origin in the filth, etc., at Sauda, that I made a representation of the circumstance; and I am not aware if this had any influence in causing the change in the method and place of coaling, which took place so soon afterwards.

We left Rio harbour on the 28th February 1850. During the previous three days the subject of the next case had been twice at the Sauda coal-wharf, for a short period each time, having been employed on the duty of towing off to the ship the large boats full of coals by means of the ship's boats. On the morning of the 4th March this amiable and promising young officer left the ship to cruise in the boats as already mentioned, and died on the 9th of concentrated yellow fever, without having been seen by any medical man. Every attention was paid to his comfort, however, by the officer in charge of the party, and indeed by all the party. Granting that he got the poison of the disease at Sauda before leaving the harbour, he did not communicate it to any of those in the boats with him, fourteen in number, who were so assiduous in their attentions to him during his illness, that I cannot suppose more than two at the utmost escaped actual contact with him. The officer in charge of the party, who was quite exhausted, and almost in a state of fever when he returned to the ship, from the anxiety and want of rest which he had endured on account of his companion's illness, escaped the disease, not only then, but throughout the epidemics both of 1850

and 1851; and we had returned to the harbour three days before the next case occurred on the 19th March.

It ought to be mentioned, that none of the "Cormorant's" men had been on shore on leave at Rio subsequent to November 1849, in consequence of some of them having then been ill treated by being put in jail, and kept there for months without trial, and in consequence of the determined hostility of the slave-dealers on account of the "Cormorant" having been so successful in giving a great check to the slave trade. The officers went on shore occasionally, but much less frequently than at other times, and of course boats and their crews went occasionally to the wharfs. After the 16th March, communication with the shore was avoided as much as possible, and the ship anchored as far from the shore as possible; but in avoiding the shore we afterwards found that we approached a charybdis, in the shape of an English brig, "Argo," a merchant vessel, at anchor near us, that had the disease, and lost two of her crew during our proximity to her; and it was supposed by some that we received the cause of all the mischief which subsequently occurred in the ship from that vessel.

I have already referred to the intercourse between the "Cormorant" and "Tweed." I was on board the latter ship longer than any one else belonging to the "Cormorant." I had none of the protection which some suppose to be derived from a former attack, and escaped the disease.

Three people belonging to the "Cormorant," who had been some time on board the "Crescent"—two of them in confinement in the prison of that ship, the other a bombardier in charge of the prisoners—returned to the "Cormorant" on the 20th March. The bombardier, and one of the other men, had attacks of fever in the "Crescent;" the latter had a second attack in the "Cormorant," already referred to, and was sent home for disease of the heart; the third escaped the disease, not only in the "Crescent," but also in the "Cormorant," both in 1850 and 1851. He had never been at sea, nor within the tropics before. There was a working party from the ship at Cobras on the 21st March, where the naval stores for our squadron are kept, and where there were some cases of fever at the time. One of the party was attacked by the fever early on the morning of the 23d, and died on the evening of the 27th March; another of the same party—a man with a constitution not originally strong, and much injured by syphilis and by mercury—complained in the boat coming off to the ship of severe purging, vomiting, and much prostration of strength. A little medicine adapted to his case was given to him that evening; next morning he was well, and resumed his duties. On the 1st May he was attacked by the fever: the first symptoms resembled those of inflammatory fever more than those of yellow fever. On the afternoon of the 2d, symptoms of pneumonia came on; he was bled, syncope threatened on the loss of fifteen ounces, and he felt much relieved. The same symptoms

soon returned with epistaxis; he was again bled to twelve ounces, with relief, and no tendency to syncope. Soon after, on attempting to sit up in bed, symptoms of excessive loss of blood made their appearance, delirium and deafness on sitting up, coma on laying down. He died comatose early on the 5th May,—the last and most rapidly fatal case in the year. During the time referred to in these remarks, when the "Cormorant" was in Rio harbour, the officers sent their linen, etc., on shore to be washed, and received the same clean after having been washed. Some of them sent them to the city, some to the island of Cobras in the bay, and others to the eastern side of the bay (Nitheroy). Fever prevailed at one place at one time, at another at other times. I inquired pretty minutely into this matter as a possible source of contagion, and with as unbiased a mind as I could bring to the task, without eliciting any observation worthy of notice. To give the particulars would occupy too much space to little purpose. Some of them also sent their clothes to be washed at small villages on the coast when the ship was out of harbour on long cruises, as well as at Monte Video when there, without ever having been suspected of communicating the disease to any of those places.

Out of fourteen people—most of them young men, and all less than six months from England, who were in attendance on the sick of the "Cormorant" at various times during the prevalence of this malady—only two suffered, both of whom were, when attacked, attending on the commander in a large airy cabin, the commander being ill at the time. Both were comparatively mild cases, and one of them had a second attack after an interval of seven weeks.

The assistant-surgeon, sick-berth attendant, and myself, were of course much with the sick, and none of us have ever had this fever, either before or since. Many others also were frequently in contact with their sick messmates when performing various acts of kindness for them; and, so far as I am aware, in no one instance was a subsequent attack attributed to this source. This subject is generally pretty keenly discussed in ships. The "Cormorant" was no exception to this remark; and the only case said to proceed from contagion was that of a youth, twenty years of age, of a relaxed phlegmatic habit and temperament, subject to ulcers for which he had been on the sick report since the 1st of February; he had two open indolent ulcers when attacked by the fever on the 31st March. He was confined to bed on the same deck—which was well ventilated—with many of the sick when attacked. He had no stamina to withstand the disease, quickly succumbed to it, and died on the 4th April. Supposing the poison of the disease to have existed in the atmosphere, this youth was exposed to that source as well as to any other; and I really do not see any material difference in the manner of attack between his case and those of the two who were attacked on the previous day.

Those cases which occurred at Monte Video subsequently to the

interval from the 31st March to the 20th April call for a few remarks. It will be remembered that the last new case of fever added to the sick report, prior to this interval, was that of the youth above alluded to, who was attacked on the 31st of March. The last death from fever happened on the 6th April; the very last death, resulting from sphacelation of the upper extremity after fever, occurred on the 12th April; and on the 16th of the same month, when we got pratique, there were four cases, some time convalescent from fever, on the sick report. The first case after the interval happened in one of the boys already alluded to, who had fever in November 1849, soon after the ship was at Sauda. This boy was attacked on the 20th April, and by the 2d May five more cases had occurred; out of these six, two died, two had hepatitis after our return to within the tropic, and were ultimately invalided from the "Crescent;" the other two recovered. The subjects of the two hepatic attacks had been, along with the sick-berth attendant, on shore on leave at Monte Video. They staid several days on shore in a state of intoxication; and when they came off to the ship on the 23d April they were punished by being made to stand on the after part of the upper deck during the greater part of the day for several days in succession. All the three were still undergoing this punishment when the two former were attacked with fever,—the one on 30th April, the other on 1st May. Their companion, the sick-berth attendant, escaped the disease throughout the whole of the epidemic. I have already noticed some difference in the symptoms of these cases which occurred at Monte Video; but how are we to account for this fresh outbreak of fever? Did those attacked after the 20th April contract the disease at Rio, the poison remaining latent in their systems up to this time? or did the disease extend to these six from the last cases of the previous batch? or had a new source of disease been established in the ship? I am unable to answer these questions, but will mention the following particulars to enable others to draw their own conclusions. The man who was attacked on the 1st May—a strong active man, thirty-seven years of age, who usually worked as a sailmaker—had sewed up all the bodies of those who died before this time, previous to their burial. This was also the man who subsequently had such a severe attack of hepatitis. So urgent were the symptoms that seventy ounces of blood were taken from the arm in two days, and he took forty grains of calomel in the same period of time; and I was much surprised that he bore all this depletion—three times to the extent of twenty ounces, after a smart attack of fever—without any tendency to syncope. He was subsequently invalided, sent to England, and was again at Rio in another ship in less than four months, and in a pretty good state of health. I have already referred to a case which also occurred about the same time, when describing some communication with Cobras in March: this was a fatal case.

We are told that yellow fever is a specific disease,—a disease *sui*



*generis*,—and that contagion implies an *animal* poison ; if so, the disease, I presume, must acknowledge a specific cause only, and the supporters of contagion, to be consistent, ought to prove that it never proceeds from any other cause, such as decayed vegetable matter, and that it never arises spontaneously.

(*To be continued.*)

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ARTICLE III.—*Researches into the Mode of Development of the Tissues in the Mammalian Embryo.* By JAMES DRUMMOND, M.D., Member of the Physiological Society of Edinburgh.

IN the following paper will be detailed a number of observations, which were made with a view to ascertain, 1st, The morphological changes which the tissues undergo in the process of their formation in the embryo of the mammalia ; and 2d, The chemical changes which the histogenetic or protein compounds undergo during the same process.

The attention of observers has in general been directed solely to the study of the changes in form which the tissues pass through during the process of their development, the chemical changes being almost entirely neglected. It seems to me, however, that the latter branch of the inquiry is fully as important and as complicated as the former. In the formation of each tissue we see it presenting a series of morphological changes, and corresponding with each of these we find that it presents a different chemical constitution. To take the formation of white fibrous tissue as an example, we find that it presents the following morphological stages,—1st, A stage in which it consists of granular bodies, presenting the characters of nuclei, embedded in a glary fluid or blastema ; 2d, A stage in which we find portions of this fluid assuming the solid form, and becoming deposited around the nucleus so as to constitute a cell ; and 3d, the gradual transformation of this latter into perfect white fibrous tissue. Such is the description generally given of the formation of this tissue, and, from anything which is stated to the contrary, we are left to conclude, that the tissue, in all of these stages of its growth, presents the same chemical constitution as perfect white fibrous tissue : that it is the same chemical compound undergoing a series of changes in shape alone. In each of these stages, however, it presents a different chemical constitution. Thus, in the first stage it consists of soluble albumen. In the second stage it presents all the chemical characters of the fibrin, of which the primitive fibrillæ of muscle are composed. In the third, or transitional stage, between the spindle-shaped cell and perfect white fibrous tissue, it consists of compounds which present partly the reactions of the fibrinous compounds and partly those of the gelatiniform compounds ; and it is only after a considerable time that it acquires the constitution of the gelatiniform compounds.



The same holds in relation to the other tissues in the body: side by side with each morphological change we find a corresponding change in the chemical constitution of the structure. Moreover, whenever we have two or more tissues presenting the same morphological characters at any stage of their development, we find that at this stage they have the same chemical constitution, however much they may differ in this respect afterwards. Thus, white fibrous tissue, yellow elastic tissue, and non-voluntary muscle, all resemble one another at certain stages of their development, and in these stages they have all the same chemical constitution.

This remarkable fact suggests the question, whether, indeed, it be not the change in the chemical constitution which, in a great measure at least, determines the change in form; whether the morphological change be not the mere external expression of the internal or chemical change which the particles composing the structure have undergone.

## SECTION I.

### I.—*Morphology of White Fibrous Tissue in its different Stages of Development.*

1. In a portion of blastema, from the deep layer of the integument of an embryo calf, about two inches long, I found the following elements: 1st, embryonic corpuscles in large numbers. These bodies present a rounded shape, granular surface, and measure from  $\frac{1}{3000}$ th to the  $\frac{1}{2500}$ th of an inch in diameter. Acetic acid either produces no effect, or causes them to shrink slightly, bringing their outline at the same time more distinctly into view. These bodies will be more particularly described subsequently. 2d, The same bodies with a quantity of minute granules deposited around them; 3d, Others with the granular matter deposited in such a manner as to present a spindle-like form. 4th, Bodies similar in shape to those last mentioned, but in which the granular matter appeared to have melted together so as to constitute a solid fusiform body with the embryonic corpuscle in its centre. Occasion-

Fig. 1.<sup>1</sup>



Fig. 1.—Portion of blastema from the integument of a foetal calf, two inches long.

<sup>1</sup> All the figures are represented as seen under a power of 250 diameters linear.

ally, at the two extremities of these, free granules were seen arranged in linear series. No distinct fibres were present.

2. In a foetal calf, four inches long, the blastema, taken from the same part, presented the same elements as above: the number of solid spindle-shaped corpuscles being, however, much greater than before. Many of these were much elongated, and at their pointed extremities were arranged minute granules in linear series, as in the former instance.

Fig. 2.



Fig. 3.



Fig. 2.—Portion of blastema from the integuments of a foetal calf, four inches long.  
Fig. 3.—Another portion more developed.

3. In a foetal calf, five inches long, the same structures were present.

4. In the foetal calf, seven inches long, there were present, in addition to the structures above mentioned, distinct bundles of white fibrous tissue.

Fig. 4.



Fig. 4.—Portion of blastema from the integuments of a calf, seven inches long.

5. In a calf farther advanced than the above, there were still present a few embryonic corpuscles, and also spindle-shaped corpuscles. The greater portion, however, consisted of distinct fibres, such as are found in perfect cellular tissue.

It is generally at a pretty far advanced stage of embryonic life that the fat begins to be deposited, and the localities, where we find the adipose tissue existing, such as around the kidney, in animals after birth, are in young embryos occupied by a glairy light-amber coloured fluid. In this substance the development of the white fibrous tissue can be traced very easily, and here I found it formed in the same way as above described.

6. In the soft gelatinous-like substance surrounding the umbili-

cal vessels, as well as in the foetal membranes, and in several other parts, I have found the development taking place in the way above mentioned, but more frequently it seems here to be formed in a

Fig. 5.

Fig. 6.

Fig. 5.—Portion of blastema from the integuments of a calf still older.  
Fig. 6.—The same, after the addition of acetic acid.

different manner. Thus, in a portion of the inner layer of the chorion of the foetal calf, four inches long, there were present a large number of embryonic corpuscles embedded in a homogeneous semifluid matrix. No other structures were present. In the more external layers of the same part, the embryonic corpuscles were found as numerous as before, and presenting the same characters. The matrix, however, showed here and there distinct indications of fibres in the shape of delicate straight lines running through it in different directions, and in some parts perfect white fibrous tissue was present. In this instance there were few intermediate struc-

Fig. 7.

Fig. 7.—Portion of chorion of foetal calf, four inches long.

tures, such as the spindle-shaped corpuscles, present: the white fibrous tissue appearing to be formed by the direct fibrillation of the homogeneous matrix. This appears to be the way in which white fibrous tissue is most frequently formed in pathological exudations.

In regard to the views which have been held respecting the formation of white fibrous tissue, Schwann believed that there were first formed around the nucleus rounded cells. These cells elongated in two different directions, and finally split up into a bundle of fibres. According to Henle the blastema is first converted into

flattened bands, which lie between parallel rows of nuclei. Then each of these bands, splits up at once into a number of fine parallel fibrils. Each of these views seems to be to a certain extent correct; for in one case we may have the white fibrous tissue formed by the direct splitting up of the blastema into fibres, as Henle says, or we may have certain intermediate structures formed.

In both modes of development of white fibrous tissue in the embryo, we have the embryonic corpuscle always present; and hence we infer, that it is essential to the formation of this tissue. In the formation of white fibrous tissue in pathological exudations also, we have bodies formed agreeing in all their characters with the embryonic corpuscle. With respect to the question as to the ultimate destination of the nucleus, Schwann believed that it disappeared shortly after the formation of the tissue. Henle supposes that these bodies are converted into yellow elastic fibres, hence termed by him nuclear fibres. How far the observations of Henle on this point are to be regarded as correct we shall see hereafter. There can be no doubt, however, that Schwann's view, in regard to the disappearance of the nuclei from the fully-formed tissue, is not correct. These bodies are to be found in white fibrous tissue throughout the entire period of its existence, both in the embryo and in the perfect animal. During embryonic life, we find numbers of them existing free, amid the fibres of the tissue; in the animal after birth they may also be occasionally seen free; but in general they are imbedded in the fibres, so that it is necessary to dissolve these, or render them transparent by some re-agent, such as acetic acid, before the nuclei can be brought into view. On addition of acetic acid, however, they are often seen to be as numerous as they are in the tissue in the embryo. In the white fibrous tissue, after addition of acetic acid, we find that the nuclei are often round, others are oval, while others again present a somewhat elongated form with blunted extremities. In their other morphological characters, as well as in their behaviour towards reagents, they correspond to the bodies which first appear in the formation of the tissue in the embryo.

## II.—*Chemical Relations of White Fibrous Tissue at different stages of its Formation.*

In the development of this tissue, we have a very beautiful illustration of the convertibility of albumen into other protein, as well as into gelatine yielding compounds. In tracing the changes that take place upon the albumen during this process, we are also enabled to form a more correct estimate of the physiological relations and relative importance of the different protein compounds than we could otherwise do. In the first stage of the development of white fibrous tissue we have present,—1st, Embryonic corpuscles; 2d, A quantity of granular matter, either floating free or aggre-

gated in the vicinity of the embryonic corpuscle; and 3d, The fluid plasma, the only protein compound in which, as we shall afterwards see, is albumen, or the analogous compound vitelline. The composition of the embryonic corpuscle will be considered afterwards; at present the composition of the granules which are deposited in the vicinity of the embryonic corpuscle may be spoken of—Are they gelatine-yielding or protein compounds? The following are their re-actions with different chemical substances:—

1. Weak acetic, tartaric, and citric acids, cause them to swell up and become gelatinous. After a very short time they are completely dissolved by these re-agents.

2. Dilute hydrochloric acid produces the same effects.

3. They are not dissolved by nitrate of potash.

4. Tincture of iodine imparts to them an orange-yellow colour.

5. Chromic acid gives them a greenish-yellow colour.

6. Millon's solution of hyponitrite of mercury in nitric acid causes them to assume a blood-red colour.

All these re-agents produce the same effects on plain and striated muscular fibre, as well as upon the compounds of fibrin, and hence I believe that these granules are composed of the same chemical compound as that which enters into the composition of muscle,—that they are, in short, identical with the fibrine of muscle. At this stage in the formation of the white fibrous tissue, no gelatine-yielding compounds exist in it; for, although it was boiled for a long time, and the filtered solution tested for gelatine, no indications of this compound were obtained.

In the second stage of the formation of this tissue, where we have present, as figured 3 and 4, numerous solid spindle-shaped corpuscles, these bodies present the same reactions as the *fibrinous* compounds, and no gelatine can be extracted from the tissue by boiling. With respect to these spindle-shaped corpuscles, we shall have occasion to speak of their chemical composition more particularly hereafter; in the meantime it may be stated, that they correspond exactly, not only in their morphological characters but also in their behaviour with different chemical re-agents, to the spindle-shaped corpuscles, first particularly described by Kölliker, as constituting the greater portion of the non-voluntary contractile tissues. The fact that we thus find two structures agreeing both in their morphological and chemical characters, and yet differing much in their physiological import,—the one being endowed with contractile power, the other, so far at least as we know at present, being destitute of this property,—ought to show the impossibility of forming, from the morphological characters or chemical composition of a structure, any definite conclusion as to its physiological properties. Hence we do not think that Kölliker is warranted in his statement, that these spindle-shaped corpuscles are always to be regarded as contractile structures, inasmuch as they may be

regarded as merely transitional stages in the formation of white fibrous tissue.

In the *third* stage in the formation of this tissue, where we have distinct fibres present, very similar to those in perfectly-formed tissue, these fibres are not, when first formed, composed of gelatine-yielding compounds; they are composed of the same compound as the granules and the spindle-shaped corpuscles,—viz., fibrine similar to what occurs in muscular fibre. It is only after they have been formed for some time that gelatine can be extracted from them by boiling. Between the stages in which we find these fibres composed of fibrine and gelatine, there are certain intermediate compounds formed, which present partly the re-actions of the compounds of fibrine, and partly those of the gelatine-yielding compounds. Thus they do not present the distinct red colour when treated with Milon's solutions; nor do they present a yellowish-green colour when treated with chromic acid. With nitric acid they do not give the characteristic yellow colour which the protein compounds give when treated with this re-agent. In these respects the fibres agree with the perfect fibres composed of gelatine-yielding compounds; but from the latter they differ, in not yielding gelatine on boiling.

With respect to the second mode of formation of white fibrous tissue, in which the blastema splits up at once into fibrils. These, though presenting almost the same characters as those in perfectly formed tissue, differ from them entirely in their chemical composition, being formed, not of gelatine-yielding compounds, but of the same compound as the fibre of muscle, or the spindle-shaped corpuscles already mentioned. In their gradual transformation into fibres yielding gelatine by boiling, intermediate compounds are formed, similar in reactions to those above mentioned. The transformation from fibrinous compounds into compounds yielding gelatine takes place somewhat slowly. Even after the perfect formation of the tissue—I mean after we can distinguish no difference by the microscope between it and perfect white fibrous tissue—and after gelatine can be extracted from it on boiling, there is still present in it a considerable number of fibres which are not dissolved by long boiling, and which do not yield gelatine.

At the suggestion of Dr Gairdner, I made the following observations on the lymph which is found in exudations, with a view to ascertain the changes which it undergoes during the process of organisation into white fibrous tissue. The first case was that of a woman, who died of tubercular peritonitis, September 8th, 1851. There were several moderately strong adhesions between the pleura pulmonalis and pleura costalis on the right side. On the left side, there were firm adhesions at the apex of the lung, with soft recent lymph over the lower portions. The whole intestines were glued together by recent lymph. The adhesions on the right side, when examined microscopically, were found to be composed of perfect



white fibrous tissue, as represented in figs. 8 and 9. On addition of acetic acid, numerous nuclei of different shapes, most of them

Fig. 8.

Fig. 9.

Fig. 10.



Fig. 8.—Portion of firm pleural adhesion.

Fig. 9.—Another portion of the same, further developed.

Fig. 10.—The last acted on by acetic acid.

being round or oval, were brought into view. These adhesions were found on analysis to be composed of—

Compounds yielding gelatine on boiling,..... 64

Compounds not resolvable into gelatine by boiling,..... 36

The firm adhesions at the apex of the lung on the left side presented the same characters as those on the right side. The soft lymph covering the pleura over the lower portion of the lung of the same side presented, when examined under the microscope, numerous bands of delicate fibres running parallel to one another, and composed for the most part of minute granular particles arranged in linear series. Imbedded amid the fibres were numerous

Fig. 11.

Fig. 12.



Fig. 11.—Portion of recent lymph from pleura.

Fig. 12.—Another portion of the same, further developed.

round and oval corpuscles, presenting a granular surface, and measuring about  $\frac{1}{2800}$ th of an inch in diameter<sup>1</sup>—the so-called plastic

<sup>1</sup> These bodies used formerly to be confounded with the pus corpuscle. The distinction between the two was first pointed out by Dr Bennett in 1842. He states that they differ from the pus corpuscle, not only in their general appearance, but also especially in their relation to water and acetic acid; the latter producing little or no effect upon them. The same bodies were afterwards described by Lebert as pyoid corpuscles.

or pyoid corpuscles. These bodies presented the same characters, and behaved towards chemical re-agents in the same manner as the bodies which have been already described under the name of embryonic corpuscles. With respect to the fibres, these presented the same re-actions as the fibrinous compounds, and yielded no gelatine on boiling. They agreed in character and in composition with the recently formed tissue, as it is found in the embryo.

The soft lymph, which was found gluing the intestines together, presented the same characters as that above mentioned.

Another case was that of a woman who died of bronchitis with pleuritic exudation, November 29th. Extensive firm adhesions were found between the pleura pulmonalis and costalis on the right side. On the left side there were also numerous adhesions; many of these were firm and tough, others composed of lymph of considerable firmness, whilst between these and the firm adhesions of perfectly-formed white fibrous tissue, there was a gradual transition.

The adhesions composed of the lymph, when examined with the microscope, presented numerous fibres, mostly composed of granules arranged in linear series, and wanting that homogeneity of structure which the fibrils of white fibrous tissue present. Entangled in these were considerable numbers of bodies similar to those already described. The fibres in these portions presented the re-actions of the compounds of fibrine. No gelatine could be extracted from them by boiling.

In places where the adhesions were much firmer, and to the naked eye presented the characters of white fibrous tissue, these were found, on examination, to consist of fibres, partly presenting the granular appearance above mentioned, and in many parts presenting the characters of perfectly-formed white fibrous tissue. The latter were not coloured red by Millon's test, nor did chromic or nitric acids give the distinct indications of protein compounds. In these respects they agreed with perfect white fibrous tissue, but they differed from it in not being converted into gelatine by boiling.

The firm adhesions presented, when examined with the microscope, all the characters of perfect white fibrous tissue; yet, on analysing these portions, comparatively little gelatine was obtained. The following is the analysis :—

Compounds yielding gelatine, .....	26
Compounds not resolvable into gelatine by boiling, .....	74

*(To be continued.)*

ARTICLE IV.—*Contributions to Obstetric Pathology and Practice.*  
By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—(*Continued from p. 138.*)

NO. VI.—ON THE POSITION OF THE PATIENT FOR PARACENTESIS IN OVARIAN DROPSY; AND ON THE PLACE OF PUNCTURE.

WHEN paracentesis is resorted to for the relief of ovarian dropsy, the method in which the operation is performed among us, is generally, if not invariably, the following:—The patient is seated more or less upright upon the edge of the bed, or upon a chair,—a broad bandage is placed around the lower part of the trunk, and the two ends of it, crossed behind, are each entrusted to an assistant, with the view of pulling and tightening it as an abdominal compress, in proportion as the fluid is evacuated,—and a small opening is cut in the anterior part of the bandage, as an aperture by which a trocar of sufficient size is introduced through the abdominal parietes into the ovarian cyst.

The bandage or compress encircling the trunk, is generally alleged to be used with two objects—*first*, To aid by abdominal pressure in the evacuation of the ovarian fluid; and *secondly*, To secure the patient against the chances of faintness and syncope upon the sudden withdrawal of such a large quantity of fluid from a person seated in the upright position. Different kinds of compressing bandages, as linen, flannel, caoutchouc cloth, etc., etc., have been recommended to be employed; and the ends of them are sometimes slit or perforated, with the view both of making the compression exerted by them more complete, and of allowing the bandage to be run and applied more easily and accurately in proportion as the abdomen diminishes in size, during the evacuation of the contained fluid through the trocar. I have seen the operation of paracentesis in ovarian dropsy performed often, and by many different hands; but I have rarely seen the bandage run correctly, even when in the first instance it was most carefully applied, and afterwards as carefully pulled by the two assistants in charge of the two ends of it. As the fluid is gradually evacuated, the bandage, in fact, is ever liable to lose its proper adaptation to the diminishing shape of the abdomen; or it becomes oblique in its position, and pressing upon the canula of the trocar, threatens to displace it; or, as sometimes happens, it rolls up, and strongly compresses one circle or portion of the abdomen, leaving the other parts of the abdominal tumour comparatively uncompressed and unsupported. Besides, any one who has acted as an assistant in a case of tedious tapping, knows that the mere pulling at the end of the bandage is a task sufficiently irksome and fatiguing.

Latterly, when performing the operation of tapping in ovarian

dropsy, I have placed the patient in the horizontal position, and dispensed entirely with the use of the compressing bandage during the operation, with, I think, no small advantage and relief to the patient, to myself, and to my assistants. If the patient be placed in the supine position, there need be no dread of the tendency to fainting and syncope, for the prevention of which the bandage is specially recommended; and at the same time, the contents of the dropsical cyst or cysts, are, it appears to me, even more easily, and certainly more completely evacuated, than when the operation is performed with the patient placed in the upright position.

In tapping an ovarian cyst in the horizontal position, the patient requires to lie as near the front of her bed or couch as possible, with the face of course turned towards it, and indeed, with the distended ovarian sac projecting if possible over the edge of the bed. If it is previously and accurately known that it is the right or left ovary which is affected, and that there is a prospect of the tumour being very completely evacuated of its contents by the operation, the patient should lie upon the right side if it be the right ovary, and upon the left side if it be the left ovary that is diseased. The trocar is then introduced into the distended cyst at the usual part in the abdominal parietes, and with the usual precautions. Towards the termination of the operation, the most complete evacuation of the cyst may be secured when necessary by turning the patient somewhat upon her breast, so as to make the puncture as dependant as possible,—and, when necessary, by compressing the abdominal parietes with the hands—the latter, a proceeding which requires to be followed in most cases of paracentesis in the upright position. One great danger attendant upon tapping an ovarian cyst, consists in the liability of air to go backwards into the emptied cavity towards the latter part of the operation, when the stream through the trocar becomes imperfect, or actually intermits in consequence of the irregularity of the abdominal compression. I believe that this accident, and its consequences (inflammation of the walls of the cyst from decomposition of its remaining contents), will be found to be much less liable to occur when the patient is tapped in the horizontal position, and the parietes of the abdomen are allowed to compress the cyst merely by their own elasticity, and by the external pressure of the atmosphere, than when the operation is performed with the patient sitting upright, and with the compression in a special degree entrusted to the proper adaptation and action of a bandage, the mechanism of which is not easily regulated. Let me further add that the preparations for the operation are far less formidable to the patient when the tapping is performed in the horizontal position and without bandages; and, besides, the necessity of assistants to manage the bandage is dispensed with. The compression itself of the bandage amounts to a feeling of distress and suffering with some patients; and I have been strongly assured by those who have been tapped at different times in both ways,

that the absence of the bandage, combined with the horizontal position, were great advantages to them, as far as their feelings and comfort were concerned. It is almost unnecessary to add, that the edge of the bed requires to be protected by several plies of sheeting or towels; and, if deemed necessary, a bandage may be placed round the abdomen, when the patient lies back in bed subsequent to the operation, particularly if the abdominal parietes are greatly relaxed, and the remaining mass of ovarian tumour feels very loose and mobile in the cavity of the abdomen. Sometimes, however, I have dispensed even with this secondary bandage.

In performing paracentesis for the evacuation of an ovarian cyst, the trocar is usually introduced in one of two situations, viz.,—either centrally, in the course of the *linea alba*; or laterally, in the course of the *linea semilunaris*. It perhaps is of little moment which of these two places is adopted, provided only the trocar is introduced in a situation in which the fluctuation is very distinct, and the parietes of the cyst thin and equal, and in which, therefore, the instrument easily reaches the cavity of the cyst.

In selecting the place of the puncture, it must be held in remembrance, that various causes may render the introduction of the trocar at particular sites difficult and dangerous; and that the presence of one or other of these causes should induce us to select another situation. For example:—*First*, The chance of wounding the urinary bladder must be avoided. The evacuation of the organ immediately before the operation, is our best security against this. *Secondly*, The uterus is sometimes elevated and drawn upwards in front of an ovarian tumour, and has been fatally wounded by the trocar in the operation of paracentesis. This ascent and displacement of the uterus, can be ascertained before the operation; and all chance of injuring it would be avoided, if, as I have already stated, a point in the cyst sufficiently fluctuating and thin in its parietes, be selected as the site of the puncture. *Thirdly*, Ovarian cysts have been occasionally found so turned upon their axes, that the elongated Fallopian tube has stretched across the front of the diseased ovary, and interfered with the introduction of the trocar; and a dense fibrous state of the cyst at particular parts has led to the same mischance—the cyst thus becoming merely displaced and not perforated by the pressure of the point of the instrument. A case of obstruction to tapping from this cause is detailed by Dr Bright in the Guy's Hospital Reports. The puncture, in consequence, must not be made over a point which feels unequal and condensed in its structure. *Fourthly*, In the later stages of ovarian dropsy, the tumour often compresses so much the contents and parietes of the abdomen, that the circulation of the blood through the abdominal portion of the vena cava is much interfered with. I have twice, in dissections of ovarian dropsy, seen the cavity of the vena cava obstructed from this cause. In consequence, a *rica-*

rious venous circulation is set up in many cases through the superficial veins of the abdominal parietes; which hence become greatly enlarged. These veins are often seen of the size of goose-quills or larger, and running immediately beneath the skin. In paracentesis, the wounding of one of these large veins with the trocar must be carefully avoided. *Lastly*, The epigastric artery has been opened by the instrument in ovarian paracentesis. It is on this account that some authors have advised us to select the *linea alba* in preference to the *linea semilunaris*, as the site of puncture. Mr South relates a case, however, in which this artery was fatally wounded when the tapping was made in the first of these sites—the *linea alba*. Common care, and a little examination beforehand for the seat of a pulsating artery in the thin and distended abdominal parietes, should enable us to avoid this source of danger. And if we avoid this difficulty, and at the same time select as our proper site for the operation, the part where the fluctuation is most distinct, and the parietes of the sac most thin and equable, it matters not whether that be in the course of the *linea alba* or of the *linea semilunaris*. The latter is perhaps the best, because the most dependant site, if we have our patient lying, during the operation, in the horizontal position.

It has been suggested by Callisen, Macarn, Delpech, Recamier, Arnott, and others, that dropsical ovaries should be tapped through the roof of the vagina in preference to the abdominal walls. If the so-called ovarian cyst is unilocular, its contents may certainly be evacuated by this means, as well as by tapping through the parietes of the abdomen; and I have more than once evacuated the contents of a dropsy of the Fallopian tube, by introducing the small trocar, which forms the usual exploring needle, in this position. In one of these cases the elongated sac formed by the distended Fallopian tube inflamed after its evacuation, and, in consequence, seemed to become entirely obliterated. The patient, a lady from New York, had previously been almost incapacitated from taking exercise, and had been in bad health for several years. Since her return home, she has been pregnant, and borne a dead child. But it is excessively rare that a true ovarian dropsy, so distended as to require tapping, is unilocular. In forty-nine cases out of fifty, or perhaps in a larger proportion, the enlarged ovarian dropsy requiring the operation of paracentesis, consists of the multilocular form of degeneration of the organ. And in this compound or multilocular cystic dropsy of the ovary, paracentesis by the vagina can very seldom readily or safely evacuate the contents of the diseased mass. For, in the operation of paracentesis in the common multilocular form of ovarian dropsy, we evacuate merely the contents of the *largest* cell or cells in the mass; and we reach this largest cell or cells easily through the abdomen, but cannot usually reach them readily, through the vagina. This important circumstance depends upon a simple pathological law, which has



not been adverted to, as far as I know, in any of the numerous essays or observations which have been published on ovarian disease. Cystic tumours of the ovary, like other morbid tumours and collections, increase always most readily and rapidly towards that direction in which there exists the least physical resistance to their growth; and, on the other hand, most slowly and imperfectly towards that side or sides on which they meet with most opposition to their mechanical development and increase of size. Ovarian tumours do not usually grow readily, or largely, towards their inferior or pelvic sides, because the resistance of the floor of the abdomen and pelvic parietes offers in that direction sufficient impediment to their development. But they increase and develop readily in an upward direction towards the abdomen, because in that direction they meet with comparatively little resistance or opposition to their growth. And while the cells in the pelvic portion of the remaining multilocular tumour, are (as far as I have examined them in a considerable number of preparations and dissections) usually very small in size, however great in number,—we have on the contrary, in consequence of the above pathological law, the largest cyst or cysts in the mass generally, if not always, placed, *first*, at the upper or abdominal extremity of the tumour,—and, *secondly*, on the anterior part of the abdominal tumour, rather than on its lateral or posterior parts; the cyst or cysts in front growing more readily, because they are less resisted in their growth by the abdominal parietes in front, than the cyst or cysts placed towards the sides or back of the tumour, inasmuch as these latter are repressed by the denser fabric of the lateral and posterior walls of the abdominal cavity of the patient. It is, I repeat, in consequence of this pathological arrangement, that, fortunately, by the operation of abdominal paracentesis, we are usually able to evacuate the largest cyst or cysts in the mass; and in consonance also with the same law, the contents of such more prominent cyst or cysts are usually far more fluid, and hence more easily capable of being evacuated through the trocar, than are the contents of the more condensed and undeveloped cysts of the tumour. In fact, the larger and more anterior cyst or cysts have often their contents sufficiently fluid for evacuation by paracentesis, at the very time that the more undeveloped and more compressed cysts still contain a thick gelatiniform matter quite incapable of being evacuated through any trocar.

It is almost unnecessary to add, that the horizontal position of the patient answers as well for paracentesis in ascites as for paracentesis in ovarian dropsy. It sometimes happens that the two are combined in a greater or less degree. A few days ago I saw, with Mr Goodsir, a case of this combination, viz., a very large multilocular ovarian tumour floating in, and surrounded by, a quantity of ascitic fluid. On placing the patient in a horizontal position, the ovarian cyst was first evacuated of its contents, which were of a very dark brown colour, and mixed with old-effused blood. After the

ovarian cyst was completely emptied, a second puncture of the abdominal parietes was necessary, while the patient was still lying in the horizontal position, to remove the collection of ascitic fluid.

NO. VII.—ASCENT OF THE UNIMPREGNATED UTERUS (*Elevatio Uteri.*)

No subject in uterine pathology is better known to the practitioner than the displacement of the uterus downwards, or the so-called *Prolapsus Uteri*. But the occurrence of the ascent, or displacement of the uterus upwards, has been left comparatively unnoticed. In the preceding observations on paracentesis, I have alluded to its elevation and position upwards in front of an enlarged ovarian tumour, as occasionally rendering it liable to be injured by the trocar of the surgeon. Voison has published a case in which the elevated uterus was fatally wounded from this cause, in the operation of tapping an ovarian dropsy.<sup>1</sup> I have seen several instances of multilocular dropsy of the ovary, in which the uterus was drawn up and elevated more or less in front of the ovarian tumour, but only very rarely so as to be beyond the reach of the finger. The same upward displacement, or *Elevatio Uteri*, occasionally happens in connection with the presence of fibrous uterine tumours. In cases of fibrous tumours, I have known this displacement so great that the os and cervix uteri could not be readily or at all reached by a vaginal examination with the first finger or fingers of the right hand, till the mass of the tumour was pressed downward, and, at the same time, somewhat rotated forwards upon its transverse axis by the left hand placed externally upon the abdomen. Within the last two weeks, I have met with an instance of this elevation of the uterus to a higher degree than I remember to have previously observed. The displacement in this instance has occurred in connection with the presence of several large fibrous tumours in the uterine parietes.

CASE I.—The patient, aged 40, had been married twenty years, had born one child about a year after marriage, but had never been again pregnant. The abdominal cavity is now distended by a mass of dense firm fibrous uterine tumours, which stretch upwards to nearly the scrobiculus cordis, and are altogether as large as the uterus in the eighth month of pregnancy. The mass consists of one great tumour stretching upwards above the umbilicus, and super-added to this are several comparatively smaller masses above and laterally, forming tuberoso elevations and projections upon the sides of the larger central tumour. One of these smaller tumours, situated towards the left side, of a flattish form, and about two and a half inches broad, is pediculated and mobile, like a peritoneal poly-

<sup>1</sup> Recueil Periodique de Médecine, vol. vii. p. 362.

pus. On examination per vaginam, a rounded elongated portion of the largest tumour is felt pressing low down into the pelvis, and filling up the space in front of the rectum. The vagina, from the lowest point of the tumour upward, is narrowed and flattened between the tumour and the pubis; but the finger, when passed along this contracted canal, cannot reach the os or cervix uteri. In fact, a sound passed into this canal, runs a considerable distance upward from the vulva before it touches the upper extremity of the vagina, and consequently before it reaches the cervical portion of the elevated uterus; and the end of the sound arrested at the junction of the vagina and uterus, can be felt through the abdominal parietes *as high as two and a half inches above the upper edge of the symphysis pubis*. The body of the uterus can be felt as a flattened projecting mass above this part.

This patient, who has come for medical advice from Australia, was there considered to be labouring under ovarian dropsy; and it was supposed to be a case admitting of removal by operation. But that it is a fibrous tumour, and not a multilocular dropsy of the ovary, is certain from its slow growth, from the density of the tissue of the tumour, from the arrangement alluded to of superadded external tumours, and from one of these superadded tumours having become pediculated like an external polypus,—a morbid arrangement which we never see in ovarian disease. Besides, there is this common, and, as I believe, pathognomonic sign present, that in various parts of the larger tumour, and particularly on its right side, a loud souffle is heard synchronous with the pulse, when the stethoscope is used,—a phenomenon very common in fibrous tumours imbedded in the substance of the parietes of the uterus, but which I have never met with in any instance of dropsical disease of the ovary. The affirmative evidence of the enlargement being a fibrous uterine tumour which can be usually derived from the simultaneous movement and union of the mass of the tumour, with the body and cervix uteri (under a combined abdominal and vaginal examination), is here of course wanting; in consequence of it being impossible to reach the elevated uterus. Nor, for the same reason, can we take advantage of those other means of diagnosis between fibrous tumours of the uterus and cystic degeneration of the ovary, which depend upon the respective measurements of the length of the cavity of the uterus in these two diseases, as ascertained by the employment of the uterine sound.

At present I have under my care, from Berwickshire, a case of very large fibrous tumour of the uterus, in which the os uteri is elevated above the pubis; though not to the great height mentioned in the preceding instance.

CASE II.—The patient, æt. 38, has been married seventeen years, but has had no family. Menstruation has been regular; and only

lately somewhat menorrhagic. Her general health is good. About ten years ago she first noticed an abdominal enlargement, which has gradually and slowly increased. The tumour is now of as great size as the uterus at the ninth month of pregnancy. It touches the lower edge of the ribs upon the right side. Its external form, however, is somewhat irregular, particularly from a large projection upon it towards the right iliac region. The tumour is not so firm in consistence as fibrous tumours generally are; and towards its centre, and near the umbilicus, it feels comparatively so soft as to give a deceitful idea of fluctuation, like that sometimes imparted by subcutaneous adipose tumours. Three months ago a trocar was passed in this situation, without drawing off any fluid whatever. There is a deep musical souffle, synchronous with the pulse, to be heard on the sides of the tumour, particularly on the left side. On making a vaginal examination, the os uteri cannot be reached, but a decreasing, conical-shaped cavity may be felt, passing up in front of the tumour, and somewhat towards the left side. When the patient is placed upon her face, and the finger deeply passed along and behind the symphysis pubis, the os uteri can with considerable difficulty be touched, lying above the upper edge of the pubis. The uterine bougie, when introduced into the os, passes readily for several inches, showing the uterus to be elongated and hypertrophied upon the side of the tumour. In this instance, there is no projection downwards of the tumour, as in the preceding case, into the pelvic excavation. The pelvic cavity is, in fact, quite free, and the roof of the vagina is altogether higher than usual.

It does not seem difficult to understand the mechanism by which the uterus becomes morbidly elevated in such cases of ovarian and uterine disease, as I have alluded to in the preceding remarks. If an ovarian or fibrous tumour, attached organically to the back wall of the uterus, grow downward upon the roof of the vagina, or, in other words, into the reflection of peritoneum between the rectum and uterus, and develope itself steadily in this its *lower* segment, the extension of the tumour in this downward direction, upon the resisting roof of the vagina, forces the tumour to lift the uterus (which is attached to the anterior surface or body of the tumour), higher and higher with it during the longitudinal development of the mass to which it is united. The tumour, in its downward longitudinal development, necessarily carries upward more and more the uterus affixed to its anterior part; in the same way as the uterus in its own enlargement during pregnancy carries and elevates upwards the Fallopian tubes and ovaries attached to its two sides. Or the enlarging uterine or ovarian tumour may, as in the second case detailed above, obtain a similar elevating influence upon the uterus, by resting its lower and growing segment upon the pubis or sides of the brim of the pelvis, instead of upon the roof of the vagina; thus ultimately displacing the uterus upwards

by somewhat the same kind of mechanism, as the os and cervix uteri are often, in common pregnancy, raised upwards and backwards above their usual level for some time after the uterus expands into the cavity of the abdomen, and rests, during its enlargement, upon the anterior circle of the pelvis.

Morbid ascent or elevation of the uterus sometimes occurs in connection with other morbid states besides the two mentioned,—ovarian dropsy and fibrous uterine tumours. Occasionally, indeed, the cervix uteri, and, consequently, the whole organ, is found placed at an unusual height from the vaginal orifice as a natural conformation; and in advanced life, when the uterus becomes atrophied, it sometimes is situated higher than natural, with the vagina drawn upwards in a funnel shape. I have seen it displaced upwards from diseased action in connection with, and as a result of, pelvic cellulitis or pelvic abscesses; as well as from the effects of simple pelvic peritoneal inflammation and adhesions (*Perimetritis*).

NO. VIII.—ALBUMINURIA IN PUERPERAL AND INFANTILE  
CONVULSIONS, AND IN PUERPERAL AMAUROSIS, ETC.

Above fifty years ago, Hamilton<sup>1</sup> and Demanet<sup>2</sup> first stated the important fact, that puerperal convulsions were liable to be preceded by symptoms of anasarca in the pregnant mother. The truth of this remark has been subsequently confirmed in incidental observations made by Duges, Burns, Montgomery, Ingleby, Johns, and others. The special pathological nature, however, of the œdema or anasarca preceding and predisposing to puerperal eclampsia remained uninvestigated, nor was any direct morbid relation attempted to be traced between the dropsy and convulsions. Previously to my first course of lectures on midwifery in the University of Edinburgh in 1840-41, I had, in more than one case, ascertained the œdema or anasarca seen in patients affected with puerperal convulsions, to be one of the numerous and important forms of dropsical disease which Dr Bright had shown to be connected with the existence of albumen in the urine. Up to 1843 I had detected, in repeated instances, this connection of puerperal convulsions with albuminuria, but hitherto I had found it only by examination of the urine during

<sup>1</sup> Duncan's Annals of Medicine for 1800, vol. v. p. 313. "Where œdematous swellings of the lower extremities take place to a considerable extent in the latter months of pregnancy in women of an unimpaired constitution, copious blood-letting alone prevents the occurrence of convulsions either before or during labour." See also his Practical Observations, p. 354. "The fits are preceded most frequently by lancinating pain of the head, sometimes by crampish pain of the stomach, and sometimes by œdematous swelling of the face and upper parts of the person."

<sup>2</sup> Recueil Periodique de la Societ  de M decine, tom. ix. (1801-2) p. 110. He regards "l'anasarque comme une de leurs causes essentielles."

life. In the spring of 1843 I saw a fatal case of puerperal convulsions, in which, in addition to the detection of albuminuria during life, I had an opportunity of observing the usual Granular Disease of the kidney on post-mortem inspection.

CASE I.—A woman, pregnant for the third time, and whose health had latterly been impaired, was seized with severe puerperal convulsions, in consequence, as her friends supposed, of strong mental excitement. I saw her, along with her medical attendant, about twelve hours after the convulsions began. She was at that time quite comatose in the intervals between the fits. In despite of venesection and various other measures of treatment, which were tried with the hope of relieving and rousing the patient, the convulsions continued, the coma deepened, the extremities became cold, the circulation began to fail, and the patient was evidently hopelessly moribund. The child's heart, however, still continued to beat, as was ascertained from time to time by the stethoscope; and the principal indication left was the preservation, if possible, of its life. The os uteri had, from the imperfect labour which had supervened from the attack of the convulsions, opened to about the diameter of a shilling, but its structures were rigid. In order to extract the child, I followed the plan of turning recommended by Dr Hamilton, and with far more facility than I anticipated *a priori*,—viz., I tilted and pushed upwards and aside the presenting head with two fingers of the right hand introduced per vaginam, while, at the same time, by manipulating upon the child with the left hand through the abdominal and uterine parietes, a lower limb was at last brought near the opening of the os uteri, and seized. The relaxed state of the uterus and parts, resulting from the deep coma of the patient, no doubt greatly facilitated this version. On attempting to drag the body of the child through the os uteri, by pulling at the extended limb, I found the rigid structures of the undilated cervix to resist altogether the passage of the trunk. Under these circumstances, I made two slight incisions into the cervix, one on each side; and on the re-application of extractive force, the breech now passed the yielding os uteri, and the birth of the child was readily effected. The child was born alive, and it survived and thrived well. The placenta escaped without any hemorrhage. The mother died in the course of two or three hours after the birth of the child. A post-mortem examination of her body was made by Professor Bennett. The lateral incisions into the lips of the undilated os, and the laceration or fissuring accompanying these incisions, were found not to extend beyond the duplicature of tissue forming the vaginal portion or projection of the cervix uteri; and had not been followed by any hemorrhage. There was no blood, fluid or coagulated, in the cavity of the uterus or vagina, or around the incisions. The kidneys presented a well-marked specimen of Granular Degeneration, probably of some standing.



In allusion to the preceding instance, I stated in the "Monthly Journal" for 1843, that this case "offered me the first opportunity of confirming, by inspection after death, an opinion that I had been led to adopt from the examination of the symptoms during life, and had publicly taught for the two last sessions,—viz., that patients attacked with puerperal convulsions had almost invariably albuminous urine, and some accompanying, or rather preceding, dropsical complication, and hence probably granular renal disease."<sup>1</sup>

My friend Dr Lever, of London, who happened also to be directing his attention at the same time to the connection of puerperal convulsions with albuminuria, published in the last Number of "Guy's Hospital Reports" for 1843, several cases of puerperal eclampsia, in five of which he had found the urine to be albuminous; and to these cases he appended some excellent remarks on the probable relations between these two morbid states. Of late years the same subject has been investigated, and the connection between albuminuria and puerperal eclampsia more or less elaborately traced and discussed by various continental authors, particularly by Cahen and Bouchut, Rayer, Devilliers and Regnaud, Depaul, Caseaux, etc.

Since the above date (1843) I have seen a variety of cases of puerperal convulsions in consultation and hospital practice, and have always (with very rare exceptions indeed) detected the existence of albuminuria in the urine of the mother. In one or two instances I have found the kidneys presenting traces of recent acute inflammation; as pus, etc. See examples of "Puerperal Convulsions connected with Inflammation of the Kidney," in Monthly Journal for 1847, p. 212. Sometimes, as in the case detailed above, convulsions, or symptoms threatening them, recur in successive labours in the same mothers in connection with established granular disease. Usually, however, the state of albuminuria which leads to puerperal convulsions is a transitory morbid condition, from which the patient recovers within the course of a few days after delivery; and the affection does not depend upon, or result in, any actual change of structure in the kidney. And it may be that the premonitory œdema, headaches, etc., and the actual convulsions themselves, do not stand in the relation of effect to albuminuria or renal disease as a cause, but that all of these circumstances—the dropsy, the convulsions, and the albuminuria—are simultaneous or successive effects of some one common central cause, viz., a pathological state of the blood, to the occurrence of which pregnancy in some way peculiarly predisposes, probably from various acts of secretion, nutrition, and depuration being vastly increased and altered by the conditions of utero-gestation. Occasionally, however, the state of albuminuria, when once induced, will continue for several weeks after delivery. Some time ago I attended, with Dr

<sup>1</sup> Monthly Journal of Medical Science for 1843, p 1015.

Fairbairn, a case that was peculiar in this respect; as well as from the lateness of the occurrence of fatal convulsions.

**CASE II.**—The lady had been confined in the country without any symptoms of eclampsia. She came to Edinburgh about seven weeks after her accouchement. When Dr Fairbairn and I then saw her together, her particular leuco-phlegmatic colour, some lesions of the senses, her occasional fits of stupor and want of memory, and the other undefined symptoms of which she was complaining, led me to suggest the propriety of testing the urine, in order to ascertain if it contained albumen. It was found highly albuminous. In the course of eight or ten days subsequently, our patient was suddenly seized with convulsions, followed by coma, under a repetition of which she soon expired. No case has, as far as I know, been put on record, in which eclampsia supervened so very late after delivery.

In the instance just now referred to, the albuminuria was, there was reason to believe, present before the termination of the patient's pregnancy; but did not lead to any attack of convulsions during, or immediately after, labour. And I have now seen a number of such instances, in which the urine was albuminous during the last days or weeks of pregnancy, without convulsions, or any other special morbid phenomena, supervening in connection with labour.

In several of these instances, temporary, and in one or two more permanent, amaurosis supervened at the time of delivery; accompanied in most, but not in all, with intense headache.<sup>1</sup> During the course of the present year, I saw one such case of puerperal amaurosis in connection with albuminuria, in a patient at the sixth month of pregnancy; and the albuminuria in this, as in some other cases, tended to bring on premature labour.

**CASE III.**—It was the patient's second pregnancy. Her face had looked swelled for a day or two previously. During the night she complained of intense headache. In the morning she complained of such a degree of blindness, that she could not distinctly see objects and persons. The urine was highly albuminous. She was freely bled. True labour pains supervened early in the forenoon.

<sup>1</sup> Drs Bright and Barlow observed amaurosis in four instances of albuminuria, but these cases were not connected with pregnancy. M. Landouzy states, that he has seen thirteen cases of weakness of vision commence, cease, and re-appear with albuminuria, and without any appreciable change in the eye or its appendages. And he considers some degree of amaurosis as a common complication with albuminuria.—(*See Archives Générales de Médecine for Nov. 1849, p. 370.*) Hamilton and other authors incidentally mention amaurosis as a symptom connected with cases of convulsions. Dr Ingleby has published a case in which a patient was affected with common puerperal convulsions in her first pregnancy; and in a subsequent accouchement was attacked with complete amaurosis, which continued during the whole period of her labour. Vision was gradually restored.—(*See his Facts and Cases in Obstetric Medicine, p. 53.*)

She was placed under the influence of chloroform for some hours, and delivered of a premature child, which was alive, but did not survive. The amaurosis in a great measure disappeared after the bleeding, and the patient's recovery after delivery was speedy and perfect,—the albuminuria passing off within a week subsequent to her confinement.

Lately I have visited, with Mr Sidey, an interesting case of more permanent amaurosis connected with the puerperal state and chronic albuminuria :—

CASE IV.—The patient, now aged 36, is the mother of six children. In 1847, two days after the birth of her fifth child, she became totally blind in the course of a single night, the amaurosis being found complete when daylight came in. The blindness, however, gradually and entirely passed off in a few days. During the second week following the birth of her last child in July 1850, she again became suddenly and completely blind, with some accompanying symptoms of stupor, and a very slow pulse. The amaurosis has not, however, altogether disappeared on this occasion, as after the former attack. The patient's vision is still (September 1851) so imperfect that she cannot read; her memory is extremely defective; she often forgets the proper word to use in the middle of a sentence; the iris now contracts, but for some time was dilated and immobile. The last child died of convulsions about a week after birth. Mr Sidey discovered the urine to be highly albuminous upon her first attack of amaurosis, and has found this state continuing in repeated examinations of it from that time to the present.

Four years ago, I met with the following instance of the complication in question :—

CASE V.—A patient who was to be under my care at her confinement, sent for me several weeks before her expected time, to tell me that her vision had become so imperfect, that she found she could not distinctly see the trees placed before her window. There was no other special symptom present; but this degree of amaurosis led me to examine the urine, which I found to be highly albuminous. During the few succeeding weeks, the amaurosis increased, and in addition, symptoms of hemiplegia slowly and gradually came on. Convulsions did not supervene, as I feared they would, during the labour, which was somewhat premature. The child survived. After delivery, the mother recovered greatly, but not entirely, from the nervous lesions, and is still suffering under a slight degree of hemiplegia.

Other lesions of the nervous system may present themselves under the same circumstances. Some of these I have noticed in an earlier Number of this Journal. (See observations "On Lesions of the Nervous System, etc., in the Puerperal State, connected with Albuminuria," in Monthly Journal for 1847, p. 288.)

In fact, writers upon midwifery have long stated to us as premonitory symptoms of puerperal convulsions various lesions of the nervous system,—as headache, giddiness, derangements of sight and hearing, etc.,—and have told us, that when more or fewer of these symptoms make their appearance, an attack of convulsions is to be feared in connection with labour, but does not always supervene. These so-called *premonitory* symptoms of convulsions, however, are only in fact so many symptoms of acute albuminuria. They are indicative of the future probability of puerperal eclampsia, inasmuch as they are indicative of the actual presence of albuminuria. And, consequently, whenever they do present themselves, their existence should lead us to examine accurately into the state of the urine,—assured that, if they are found to be connected with albuminuria, we may be certain of the liability of our patient to the supervention of convulsions,—a liability that, no doubt, may be often lessened or averted by proper antiphlogistic treatment before labour, and by using such means as excite and act freely upon the intestinal, renal, and cutaneous secretions.

A few weeks ago I saw an instance in which convulsions in a child after birth were connected with the presence of albuminuria in its urine,—or connected (as it should be perhaps more correctly stated) with that condition of blood-poisoning or uræmia, which is the result of albuminuria,—whether that condition consists in a morbid accumulation of urea; or is produced, as Frerichs supposes, by the presence of carbonate of ammonia in the blood; or is, as is more probable, the effect of some other morbid agent in the circulating system, capable (like strychnia) of increasing the centric irritability or polarity of the spinal system to such an excessive degree that, under this super-excitability, comparatively slight eccentric causes of irritation in the stomach, intestines, uterus, bladder, etc. etc., readily induce convulsive attacks of a general form, like those of puerperal eclampsia.

CASE VI.—A lady, pregnant for the first time, was suddenly, when near the full period of utero-gestation, attacked, when rising in the morning, with severe headache, faintness, and threatening of convulsions. My friend Dr Weir, under whose professional charge she was, saw her immediately, and bled her largely at the arm, etc. On making a second visit about three hours subsequently, she took, when he was present, a most severe fit of convulsions, which left her in a state of deep coma. Two hours afterwards I saw the patient with him. She was still comatose, and remained so for some hours subsequently. The child's heart, when examined by Dr Weir and myself, with the stethoscope, and while the mother was still comatose, had only 88 beats in the minute; but in the evening it had risen to its usual rate of 130. Next afternoon labour supervened; the patient was put under the influence of chloroform

for some hours; and a living child was born without any recurrence of the puerperal convulsions. The mother made a slow but perfect recovery. On the third day after birth, the child began to suffer under a succession of convulsive attacks, which gradually increased in severity during the next twenty-four hours, when it was placed for a considerable time under the influence of chloroform inhalation, and the fits ceased. After the convulsive attacks supervened in the child, Dr Weir and I had two opportunities of examining its urine; and on both occasions we found the renal secretion of the infant, like that of the mother, highly albuminous. Some time subsequently the infant died of inflammation of the cellular tissue of the loins and pelvis. We were not permitted an inspection of the body.

I am not aware that any one has hitherto observed albuminuria co-existing with infantile convulsions; but future observation may perhaps show it to be a common pathological condition in some forms of that disease; and probably in the Trismus Nascentium. In such cases, indeed, the urine has hitherto seldom or never been examined, in consequence of the trouble and difficulty connected with obtaining specimens of it in the affections of infancy. Albuminuria may yet be found to play also an important part in other diseases of infancy. The induration of the cellular tissue, or skin-bound disease (Sclérème—" *L'Endurcissement ou l'Œdème du Tissu Cellulaire*" of French authors) is an extremely rare affection in Edinburgh. I have only seen two cases of it; and, as was stated in this Journal several years ago (see Monthly Journal for 1843, p. 699), in both of these instances the urine was coagulable. Hence, at that time, I ventured to suggest, that the skin-bound disease itself, or at least some forms of sclerema, may be a variety or effect of Bright's disease in early infancy; the effusion into the cellular tissue which constitutes the marked feature of the affection, being so far analogous to the anasarca occurring with albuminous nephritis. For the solution of this point, affirmatively or negatively, we can only look to some of those continental pathologists who have ample opportunities of studying the disease in question.

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ARTICLE V.—*Case of Fracture of Base of Skull, with Loss of Cerebral Matter, terminating in Recovery.* By WM. GRAHAM, M.D., Surgeon of H.M.S. "Vengeance."

GEORGE PAWLEY, æt. 18, 1st class boy, on the 6th of June last, whilst going aloft, fell from the main futtock rigging to the deck (height of sixty feet), striking the edge of the sail. I saw him immediately after the accident, and found him totally insensible; his breathing was stertorous and very much embarrassed; pulse 80, and tolerably strong, but soon after began to intermit; there was considerable hemorrhage (venous) from the left ear and nostrils, and por-

tions of *undoubted* cerebral matter in the left ear; pupils were variable, but answered to the stimulus of light. In about four hours sensibility had partially returned; pulse 100, small, but not intermitting; blood and cerebral matter continued to ooze from the left ear; and, on introducing a probe, exposed bone and irregularities on it detected. His pulse continued to rise during the day, breathing became embarrassed, and he passed his urine involuntarily. Head shaved, and cold lotion constantly applied.

*June 7th.*—Remained quiet during the night; pulse 150, very small; skin hot and dry; more blood and cerebral matter came away; bowels not open. An enema was administered, which brought away a quantity of feculent matter.

*8th.*—Passed a restless night, tossing his hands and arms about; pupils variable, one dilated and the other contracted, and he squints; skin hot and dry; tongue coated; pulse 120, sharp and wiry. Hirudines xx. temporibus—doses at intervals during the day.

*9th.*—Has passed a tolerable night, and is now perfectly conscious, but articulates imperfectly; now keeps his eyelids open and looks around; skin cool and moist; pulse 68, soft; no more cerebral matter has come away; continues to pass his water involuntarily; breathes comfortably. Cold to be continued to the head; to have arrow root and chicken broth.

From this time he daily and gradually improved; he regained his hearing and speech completely, and his intellect remained unimpaired; in fact, his messmates said "he was brighter than before the accident." He was granted a pension certificate, and went on board the "Growler" steamer, to return to England on the 6th of July.

This remarkable case shows to what extent injuries of the brain may go in some cases, without that active and destructive inflammation so generally dreaded following. Here there can be no doubt that there was considerable laceration and loss of substance, as witnessed by myself and colleagues, Messrs Ward and Stephens, who concur with me in opinion, that fracture and separation between the squamous and petrous portions of temporal bone existed external to the position of the tympanum, so that it remained intact; for had it been ruptured, hearing could not have been so perfectly restored. The hemorrhage, I presume, was from the lateral sinus. The quantity of cerebral matter that oozed from his left ear was about two tablespoonfuls. The reason why I applied leeches on the 8th, in preference to having recourse to venesection, was, that I considered the symptoms due to the re-action necessary in cases of this description, and not to actual inflammation; moreover, sailors bear the loss of blood very badly.

This is, on the whole, the most remarkable case I have met with in twenty years' experience, and goes to prove the truth of the saying,—“Whilst there is breath there is hope.”



ARTICLE VI.—*Death from the Administration of Chloroform.* By  
WILLIAM M. BROWN, M.D., Melrose.

ON the 10th of August 1852, I was asked to see Mr Martin, cattle-dealer, Earlston. Upon visiting him, I found a number of ulcers on the left leg, the surfaces of which, and the surrounding skin, presented a very unhealthy appearance. He had been under treatment by regular medical men, and also by a clergyman who practises the quackery, homœopathy; but with no benefit. As he was anxious for something to be done, I proposed to destroy the surface of the sores, and part of the surrounding skin, with pot. fusa; but as the sores were extensive, and the application of the caustic would be very painful, it was thought advisable to put him under the influence of chloroform, to which I saw no objection. I accordingly went over the following afternoon, and, before proceeding to apply the caustic, I gave him the chloroform. He was not easily affected by it, and struggled a good deal. After beginning to apply the caustic, I found he was not sufficiently insensible to pain, and gave him a little more chloroform, which had the desired effect. I then proceeded with the application of the caustic, and was just finishing when I observed a sort of catch in his breathing. I immediately stopt, and on looking at him I saw the mouth and eyes open, the breathing irregular, face pale, the eyes slightly turned upwards, and the pupils dilated. I ordered the window to be thrown open, dashed cold water in his face, tried artificial respiration, but with no effect. In a few minutes the man died. I cannot say how much chloroform was used, as a good deal was lost in the giving of it, but certainly not more than I have repeatedly given. There could be nothing faulty with the chloroform, as it was the same I have been using for some time; and, as a guarantee of its purity, I may mention that it was Messrs Duncan and Flockhart's. No examination of the body could be procured.

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## Part Second.

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### REVIEWS.

*The Stomach and its Difficulties.* By Sir JAMES EYRE, M.D.,  
Edinburgh. London: Churchill. 1852. Pp. 152. 8vo.

THIS is a short, quaint, gossiping, truthful exposure of the frailties of the stomach, of the penalties which it consequently incurs, and of

the purgatory it must undergo for relief. The author is evidently an amiable elderly physician, of whose opportunities of personal experience, in the matter he writes of, no doubt can be entertained. He tells his story with a chatty ease, yet earnestness, which cannot fail to win from his reader conviction both of his sincerity and his soundness. And there is no denying that any one who walks in good time by his not very ascetic counsel, will have chosen "the best of all ways to lengthen his days," so far as the stomach can assign them a limit.

"The Stomach and its Difficulties." Is this not a misnomer? We had always supposed that the main difficulty with the stomach was to find something to put into it; and that, this being got over, it was on the contrary very remarkable for facilities,—for its facility of doing wrong—its facility of going wrong—and above all, its marvellous facility of coming right again, if let alone. And we are still inclined to think, that, if the stomach has a difficulty at all to contend with, this must be simply the loose habits of a near neighbour, the palate, which will insist on introducing improper company into their common premises.

Like other popular writers on the stomach, Sir James Eyre starts with a popular description of that vessel. "It is," says he, "very like in shape to a *Caledonian* bagpipe." We were not aware of this. The chief peculiarity of the *Caledonian* instrument, besides its overpowering energy, is a set of three long pipes, sticking up over the shoulder when in action. These no doubt might prove a convenient provision, in the stomach, as in the bagpipe, against a surcharge of wind or other "difficulties." But we have not yet seen such a stomach.

Having described the stomach, he goes on to catalogue the sorts of things to be put into it, and he arrives at the tiresome old conclusion, that "man is an omnivorous animal, as is shown by his teeth,  
\* \* \* \* \* which were no doubt intended by our Creator to be our main guide on this point."

Very questionable physiology. It is not surprising, however, that a London physician should be betrayed into the mistake of supposing that the entire genus *Homo* is an omnivorous animal. We happen to know of a Londoner having supped on stirabout made of a pound of salt and a pint of ale—his last meal upon earth. Our author knows another, who, in his sound senses, dined on a hat, and digested it. And he speaks with enthusiasm of the stomach "passing on heterogeneous abominations in an improved condition," and disposing of "the omnium gatherum of all clean and unclean bodily things." In this latitude, the stomach can do nothing of the kind. Such prowess must be proper to the sub-species *H. Londinensis*. It has been ascertained, at all events, that man elsewhere cannot live with the mole on worms, or with the cow on grass, or, like the vulture, on fly-blown donkey, or, like the ostrich, on tenpenny-nails and open hundred-bladed penknives, or upon invisible entomostraca,

which Dr Knox says are the only food of the herring. Man, then, is not omnivorous. Neither are his teeth a sure guide upon this point. If they were, he ought not to feed until he gets them, or after he has lost them. We have an uncommon curiosity to see a case of teeth suggestive of "heterogeneous abominations" and "all unclean bodily things,"—or a maxillary apparatus which would point at a hat as a suitable dainty.

At no distant date, says Sir James, we in England lived upon "suction," that is, liquids, and potent ones too. But now we have taken to "stuffing,"—to wit, very substantial things, and plenty of them. The plentiful platter, however, is, in his opinion, far more perilous than the laughter-loving cup. It is more insidious; for it is so well managed, that even "religious people,"—that is, divines—will not see the sinfulness of it. And it is the instrument of self-destruction; for they who do not hold hard "with the snaffle-rein of common sense," when about to clear the platter, dig their own grave with their teeth.

And, nevertheless, continues he, neglect and inattention are sinful too; inasmuch as it is evidently sinful for a man to fast, if through mental distress taking away appetite—because he is one "who will not forgive his Maker for afflicting him;"—and scarcely less sinful if the cause be undue mental application, or a hobby for long fasts, because he withholds from the stomach what is its own by the law of nature and nations.

Therefore let every man begin the day betimes with a good breakfast—not before the appetite for the day begins—but yet as early as may be. The plethoric, possessed of an aptitude for making blood, must pinch himself upon tea or coffee, with bread and butter, and an egg (!), or two (!!), but nothing more. To every one else the author offers handsome terms:—tea, and coffee, and bread, and butter, and fish, and meat, and eggs, and abundance of marmalade and other preserves. Which aggregate, he calls a Scotch breakfast.

This is perilous advice. Everybody, of course, knows that a Scotchman invariably sits down to a breakfast of all these trifles and more—such as cold game, fried ham, devilled bones, oatmeal cake, barleymeal bannocks, honey, *et cetera*. But a Scotchman is an Outside Barbarian in gastronomy. When a Celestial like our author, or any of his countrymen, sits down to a table so furnished, he ought to remember this, and that he has not three spare pipes to his bag.

After such a breakfast, it is no wonder that "luncheons are not recommended." And nevertheless "this is the fittest time of the day for eating fruit, \* \* \* especially raspberries, strawberries, and gooseberries,"—and also for sea-bathing,—during which, the addition of a mouthful or two of the water, would, in our opinion, be not amiss in the circumstances.

And now for the great repast of the day. "Man's character is much influenced by the aliment which sustains him." Therefore,

when he dines let him eat up to his character. If he would be of bold and fierce deportment, let him subsist chiefly on flesh. If inferior in activity and game, let him be fishy in his tastes; especially if he has not good teeth. Soups are not forbidden, but one ounce of meat is worth twenty of any soup. Choose beef in preference to everything else, above all, the fillet and inside of the sirloin—venison, mutton, and game, second in order—chicken and other poultry next—pork, not without a long fast in prospect—and in all circumstances, eschew veal, as being whitened by unchristian atrocities. Dine by no means on cheese alone; because Dr Stark of Edinburgh (not Vienna, Sir James), lost his life through such dining. Devour bread like a Frenchman; and do not nibble it like an Englishman. Crave not unwholesome puddings, and gross vegetables, or poisonous pastry; but regard as the greatest of dietetic blessings, a cook who can *not* make pies and tarts. Happy is the man who can digest salmon; for he may encounter all other dishes without fear. Thrice happy the dyspeptic who attains to the digestion of a salad: His cure may be said to be finished. Eat ice, if it must be eaten; but tremble. Fear not potatoes, if roasted—oysters, if stewed—crabs, if you be free of anti-cancerous idiosyncrasy. And, to conclude, observe variety; but not in one day. Bolt not with Jonathan. Stuff not with John Bull. But, with our author, tell in time your “kind-hearted hostess, who ever and anon inquires what you will take next, that you will take breath.”

As for liquids, there is water everywhere at hand, or barley-water acidulated with lemon-juice, but not too much of that (!), or a small tumbler of warm (cold?) weak brandy and water, or a glass of Sherry and cold water, or, instead of this, Port or Madeira for certain idiosyncracies. Touch not inter-prandial Champagne, if a single glass cause a blush on the countenance. Indulge in ale and porter only with cheese; and in that never. When post-prandial potation is imperative, renounce Port, stick to Sherry and full-bodied Claret, with Scotch or Irish (?) whisky occasionally, in water. And above every thing, remember with Dr Babington (or Dr Gregory), that “a pint of wine daily is hard drinking.”

In one circumstance only does the author admit an exception. “It is marvellous,” says he, “how much fluid of a vinous or even spirituous nature \* \* \* is really needed, in making inordinate bodily exertion, as in climbing mountains.” This is a prodigious mistake on his part, as even Highlanders will confess to him, should he fall in with a Highlander honest in a question of whisky. But what can an author, who has lived all his days among flats, be expected to know about mountains?

Add to all these rules a contented spirit, with, if possible, a clever jester for company—and dyspepsia will be numbered by-and-bye with leprosy, the black death, and other extinct diseases. But if not, there is always one other resource left. Resort to “The Stomach and its Difficulties,” for sound medicinal treatment and regi-

men; which can hardly fail, with a stomach not texturally and organically broken up beyond repair.

Beware, however, of undue familiarity with the author's pet, oxide of silver. Beware, at least, of imagining with him, that your physiognomy is in no danger from it, or in less danger than from the nitrate. We know only one man who has acquired the hue of Frankenstein's monster from nitrate of silver, but two similarly tinted by the oxide.

*Climate of Italy in Relation to Pulmonary Consumption; with Remarks on the Influence of Foreign Climates upon Invalids.* By T. H. BURGESS, M.D., etc. London: 1852. Small 8vo. Pp. 206.

THE object Dr Burgess has proposed to himself in this volume, is to point out, that all those places supposed to be favourable for consumptive people in Italy are in point of fact injurious; that the idea of their being beneficial is a popular delusion, and that it is much better to visit some of the sheltered places in our own country, than with a view of seeking health, really find a grave in foreign climes. He says:—

“The rapid and extensive variations of temperature observed in the Italian climate—the absolute necessity to consumptive invalids of changing their place of residence as the seasons change—the fatigue, discomfort, and risk, attendant upon every such change—and the mania for sight-seeing in cold churches and galleries, which no invalid can overcome, have frequently, during my sojourn in Italy, suggested to me the following reflections:—

“1. Has not Nature adapted the constitution of man to his hereditary climate?

“2. Is it consistent with Nature's laws and operations, that a person born in England, and attacked by consumption, can be cured by a foreign climate, in every characteristic opposite to his own?

“3. Why should a warm climate be preferred to a cold one, if the temperature be equable? the mortality from consumption being less in the latter than in the former.

“4. A revolution must take place in the system of every consumptive invalid who goes to Italy, before he can become acclimated; and how many must sink under the probationary process, from fatigue and exhaustion?

“5. If a phthisical patient derives benefit from a foreign climate, he should never leave it; for it is obvious, if he returns to his native climate, his constitution will be again changed or remodelled, and he is then rendered obnoxious to the same physical causes which originally produced his complaint.

“6. The rapid variations and extensive range of temperature peculiar to warm climates greatly counterbalance their alleged good effects.

“7. It is more in accordance with Nature's laws to believe that when *change* is necessary in cases of consumption, a modification of the climate in which the patient and his ancestors were born and reared, or, in other words, *change of air in the same climate*, by removing from one locality to another, more appropriate to the patient's condition, will effect greater good than any violent transition to warm countries.”—Pp. 22-24.

In our own opinion, nothing is more difficult than for a medical man practising in this country to arrive at just notions concerning

the sanative influence of a foreign climate in cases of pulmonary consumption. He may read books on the subject generally; he may study monographs on the especial advantages of particular places, and he may further converse with sensible men who have practised there, without being in any degree more enlightened. As a general rule, every local practitioner speaks highly of the superior merits of his own place of residence. He is ready to give you a list of the most extraordinary recoveries. He instances the cases of Lord this and Lady that, who, on their arrival, were in the worst possible condition, and who, during their sojourn in his locality, even surprised *him* by their rapid recovery. In short, when listening to these accounts, we feel astonished that any case of phthisis should die, did not all such practitioners, in reply to a straightforward question, acknowledge that deaths notwithstanding were very common, and that, after all, these remarkable cases were the exception and not the rule. The real questions to be answered, in reference to the sanative influence of climate, are,—1st, What is the proportion of cases in which an arrest of the disease takes place, as determined by a strict diagnosis, the stage of the disorder, and the age and general strength of the patient? 2d, Are such arrests more frequent in foreign countries than they are at home? So far as we are aware, no series of facts exists capable of satisfying us on these points. On the other hand, is it not certain that if a phthisical person recovers his bodily strength in Madeira or Italy, the benefit is at once ascribed to the influence of climate; whereas, if the same thing happens at home, the case is considered one of bronchitis, or at all events its phthisical character is denied? Yet it has of late become sufficiently evident, that, with proper care and treatment, phthisis may be arrested in this country much more frequently than was formerly supposed; and we have no reason to believe that such arrestment is more common in Madeira, Egypt, or Italy, than it is in Edinburgh or London. It may then fairly be asked,—Whether the practice, which has so long prevailed, of sending consumptive patients abroad is beneficial or not? Dr Burgess unhesitatingly pronounces in the negative, and argues as follows:—

“If we contemplate the climate theory through the appropriate medium of the natural history of creation, we shall find that the argument is also in our favour. We may seek in vain along the entire range of organised existence for an example of diseased animals being benefited by removal from a warm to a cold, or from a cold to a warm country. There appears nothing in the book of Nature so violently inconsistent. The fishes which inhabit the waters of the British islands will not thrive in the Arctic seas, nor those of the latter in the ocean of the tropics. The birds of the primeval forests of America generally die in this country, unless reared like hot-house plants; and so with the wild animals which live and flourish in the jungles of Asia or the scorching deserts of Africa.

“Man, although endowed in a remarkable degree, and more so than any other animal, with the faculty of enduring such unnatural transitions, nevertheless becomes sensible of their injurious results. For familiar illustrations



of this influence, we have only to look to the broken-down constitutions of our Indian officers, or to the emaciated frame of the shivering Hindoo who sweeps the crossings of the streets of London. The child of the European, although born in India, must be sent home in early life to the climate of his ancestors, or to one closely resembling it, in order to escape incurable disease, if not premature death. Again, the offspring of Asiatics born in this country pine and dwindle into one or other of the twin cachexiæ—scrofula and consumption, and if the individual survives, lives in a state of passive existence, stunted in growth, and incapable of enduring fatigue. If such extreme changes of climate prove obnoxious to the health of individuals having naturally a sound constitution, how are we to expect persons in a state of organic disease to be thereby benefited? In fact, view the subject in whatever way we may, we must eventually arrive at the natural and rational conclusion, that Nature has adapted the constitution of man to the climate of his ancestors. The accident of birth does not constitute the title to any given climate. The natural climate of man is that in which not only he himself was born, but likewise his blood relations for several generations. This is his natural climate, as well in health as when his constitution is broken down by positive disease, or unhinged by long-continued neglect of the common rules of hygiene.

*“Change of air in his own climate, or removal to one nearly approaching to it, is the natural indication, and will effect whatever good climate can effect in consumption.”—Pp. 19-21.*

Our own experience is on the whole hostile to the propriety of sending phthisical patients abroad in search of health. We have now met with many consumptive individuals who, so long as they remained at home, continued in a satisfactory condition, enjoyed life, and carried on their usual occupations in comfort; but who, seized with an unconquerable desire of completely getting well, through the agency of a warm climate, have gone to Italy, and died most miserably. Such cases have been so frequent with us, as to have given rise to a feeling of great scepticism as to the utility of expatriating such persons,—a feeling which would have become absolute, were it not counterbalanced by a conviction engendered by foreign travel, and dependent on what may be called personal sensation, rather than actual experience of any beneficial result obtained by others. We allude to that exhilarating feeling which the traveller experiences in the south of France, or the borders of the Mediterranean, caused by the clear atmosphere, balmy air, and luxuriant landscape. He who has felt that delightful sensation, and paid attention to its influence on his own bodily powers, will not easily abandon the idea that such influence, if rightly directed to the relief of certain morbid conditions, must have some effect. We believe that such a feeling insensibly constitutes the real basis of all our belief concerning the good effects of climate; and as we still think, notwithstanding all Dr Burgess has said, that, in certain cases, it is really beneficial, it may be worth while to inquire why it often fails, and why it sometimes succeeds.

Supposing, then, that residence or travel in certain foreign countries may be beneficial in particular cases, and the chief argument in its favour are the sensations to which we have alluded, it cannot be denied that many fallacies are liable to enter into our reasonings.

For instance, it does not follow that the same elastic feeling experienced by a healthy, vigorous individual on the mountain side, on the sea shore, or in the beautiful valley, should be felt by a debilitated, worn-out person in a similar situation. Nor is it reasonable to suppose that the qualities of mind, power of exertion, and consciousness of bodily strength—all of which are elements in the production of the feeling alluded to—should be alike in the two cases. Hence, while some persons may be benefited, and the nutritive powers stimulated under such circumstances, others will feel languor, depression of spirits, or increased fatigue, and find themselves much worse. The difficulty, therefore, is to discriminate between these two classes of persons,—a difficulty which defies all general rules, dependent as it is not only on the stage of the disease and bodily strength of the individual at the time, but also on his peculiar constitution, habits, general excitability, powers of imagination, and cultivation of mind. Hence, before sending patients abroad, all these points must be anxiously considered; and even then the whole will resolve itself into the fact, which can only be determined by experiment, whether, upon actual trial, they feel better or worse.

We believe, however, that in most cases the change is at first beneficial, and that it would be to a considerable extent permanent, were it not for another fallacy which extensively prevails. We allude to the idea, that the climate itself has a sanative tendency, and that the breathing this or that air is like taking so much medicine, and ought to do good *per se*. Now it should be considered, that the best climate is only useful as a means of taking exercise, and promoting the nutritive functions, without exposure to those drawbacks which are more or less common at home. It is by regarding exercise as necessary to securing active digestion that its importance as a therapeutic agent becomes obvious in phthisis, and any locality which will enable the sensitive invalid to go out daily on foot, horseback, or in a carriage, without the chance of meeting cold winds or showers of rain, must possess an advantage over one where these occurrences are common. Now all accounts agree in representing Madeira, and some other places, as more favoured in this respect than even the best localities in England,—and if so, they may, in the sense referred to, be more beneficial as places of residence.

In searching for such benefits in a foreign climate, the patient has often to sacrifice the occupations he may be accustomed to at home, and the society of his friends. But if this can be done without inconvenience, and without causing mental depression or a sense of *ennui*, it may even be advantageous. Mental impressions must not be overlooked. Then he will experience a great difference between the comforts of an English residence and those in a foreign house, which, to the healthy traveller, are often annoying, and to the invalid are injurious. In Rome Dr Burgess says the

streets are built to exclude, as much as possible, the rays of the sun, and in winter are as damp and cold as rain and frost can make them. And then he adds, "What a difference between the warm carpet, the snug elbow-chair, and the blazing coal fire of an English winter evening, and the stone stair-cases, marble floors, and starving casements of an Italian house!"

It is well pointed out by Dr Burgess, that those who go to the large Italian cities are exposed to other dangers connected with the desire of seeing celebrated places, works of art, churches, vaults, etc., which induce great bodily fatigue, and often chill the body by long exposure to damp air, or from standing on cold marble floors. He says:—

"It has often occurred to me, while observing the habits of consumptive patients when in Italy, that a description of the *climate*, of old ruins, cold churches, empty palaces, long picture galleries, and other places favourable for the collection of stagnant air, but where invalids notoriously pass a great portion of their time, would be much more useful and appropriate than any elaborate account of the external or natural climate of the country which the most minute and careful observation could afford. It matters little how pure the atmosphere may be in reality, if the air the patient breathes for so many hours each day is impregnated with noxious exhalations, as it must be in the majority of instances, while he is admiring the bronzes, pictures, and statues, of the cathedral, or trying to decipher half-worn inscriptions on the mouldering walls of some ruin or dungeon.

"The attractions of the basilic of Saint Mark, a church which has not its parallel in the world, are certainly of no ordinary kind. The mosaics, sculptures, basso-relievos, and arabesques, with which it is profusely ornamented, together with the gilded arched roofs, the pavement of jasper and porphyry, the five hundred columns of black, white, and variegated marble, of bronze, alabaster, vert-antique, and serpentine, are irresistible to the foreign invalid, who soon finds his way thither, and passes hours fatiguing his frame, gazing at the marvels of the building, standing on its cold and sunken floor; for the piles underneath have given way in many places, and hence he breathes an air damp and impure.

"The Ducal Palace, close by, has also various attractions, and I doubt whether the masterpieces of the greatest painters Venice has produced, with which the ceilings and walls of the different apartments are adorned, are so eagerly sought after as the Piombi and the Pozzi, the latter being the dungeon cells in the vaults of the palace, over which the boats on the canal pass, and with whose history so many tales of horror are connected. These horrible dens are still dismal and damp, although the walls are boarded to prevent the humidity from penetrating."—Pp. 106, 107.

"In the renowned capital of Tuscany, wandering amongst its splendid, but cold and damp churches, its palaces and picture-galleries, many an English invalid annually hastens his end; and it not unfrequently happens here, as in other cities of the south, that the places most frequented, and possessing the greatest attractions, are of circumscribed dimensions and badly ventilated. For instance, visit the far-famed *Tribuna*, of an afternoon, in autumn, and there you will find, in a small octagon chamber, like a moderate-sized boudoir, containing the most valuable gems of antiquity, and some of the finest paintings in existence, a crowd of eager spectators, even including invalids, jostling each other from want of room, gazing for hours together upon the immortal works of art around, whilst breathing all the time a heated, confined, and impure atmosphere. An observer will not remain long before

his attention is arrested by the ominous short, dry, jerking cough, and, on looking round, he is sure to see the same stereotyped picture of the 'English disease,' so painfully familiar to travellers throughout Italy, supported on the arm of an attendant, staring at the marble statue 'that enchants the world,' which often seems more alive than the gazing invalid."—Pp. 134, 135.

Again he points out that in Rome—

"The rank and luxuriant grass, weeds, and wild-flowers,—the Flora of the Coliseum,—which grow in profusion all over the amphitheatre, and the moist and stagnant air of the place, combine in forming a noxious atmosphere, the evil effects of which are soon experienced by strangers, whether invalid or robust, who pass any time there. I have repeatedly observed invalids wandering about this vast ruin for hours, and with the aid of a guide climbing over the different stages of the mouldering walls to catch the effect produced by the variety of views which are renewed at each arcade. At night, and by moonlight, is the favourite time for visiting the Coliseum, in order to see the effect of light and shade, with the endless details of ruins thus shown. No consumptive patient who is able to drive to the spot, and to crawl over the walls, ever omits such moonlight visits! One might suppose that an individual in bad health would choose a more cheerful scene—at least, one less significant of his own condition; but it may be, perhaps, that ruins console each other."—Pp. 175, 176.

Another evil of large continental cities consists in the attractions of fashion, so that the young can seldom resist the late evening parties, the dance or public amusements, when flushed with excitement or exertion, they return to their homes late at night, exposed to the chill air, the injurious effect of which is augmented by the previous heat and foul air of crowded assemblies. All such irregularities and every kind of over-fatigue are more than enough to counterbalance the supposed good effects of climate. Hence places of quietude, offering no temptations to gaiety, and possessing only natural advantages of scenery and the gentle stimulus of a clear atmosphere, mild temperature, and cheerful society, are the best.

Another fallacy is, the idea that warmth is the agent which, in such cases, does good; and people talk of a warm climate as synonymous with a healthy climate in such cases. But unaccustomed warmth is most relaxing, and tends, instead of checking, to occasion increased development of the tubercular exudation. Nothing is more common in this country than to observe how phthisical patients get worse on the approach of sultry weather in summer, and how comparatively better they are in winter, so long as they avoid exposure to cold winds. In fact, it is not a warm climate which is sought for by the invalid, but a temperate climate during the winter, in a more southern country than England. As summer approaches many parts of the British Isles are infinitely preferable.

It follows, from all the information we have been able to collect, that that climate is best which will enable the phthisical patient to pass a few hours every day in the open air, without exposure to cold or vicissitudes of temperature on the one hand, or excessive heat on the other. Wherever such a favoured locality may be found during the winter months, its advantages should be considered

as dependent on exercise, and on the stimulus given to the nutritive functions, rather than to its influence on the lungs directly. It is a matter also of great importance to remember, that the comforts of home, a well-arranged diet, general hygienic rules, and a proper treatment, are as necessary in Madeira, Italy, Spain, or Egypt, as they are in Edinburgh. Lastly, we will venture to say, that the good effects of a foreign climate have been greatly exaggerated; and to all of our readers who feel interested in the matter, we cannot do better in proof of it than recommend the perusal of Dr Burgess' well written and agreeable volume.

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*Biography of Dr Sheridan Muspratt, F.R.S.E., M.R.I.A., F.C.S., Membre de la Societ  d'Encouragement, Professor of the College of Chemistry, Liverpool, etc. etc.* Written for the "Lancet," by a London Barrister-at-Law.

*A Third Edition of the Influence of Chemistry on the Animal, Vegetable, and Mineral Kingdom.* By Dr SHERIDAN MUSPRATT. Price Half-a-Crown. 8vo. London: Churchill. 1852.

THE Barrister-at-Law who writes this biography modestly withholds his name; but we are informed by the equally anonymous writer of the preface, who states himself to be a London practical chemist, that the said biographer is an "eminent member of the bar, who, with literary requirements, has been enabled to combine an intimate acquaintance with the moral and intellectual character of the subject of his labours." We of course have indulged in the usual wonderings and guesses as to who this literary barrister might be. Taking the word bar here to mean, by metonymy, the legal profession, we at first thought Chief Justice Campbell might have been refreshing himself with a little light biographical work, after having disposed of the Chancellors of England. Then we thought of the versatile genius of Lord Brougham; and subsequently of Thomas Babington Macaulay, and that this literary engagement satisfactorily accounted for the non-appearance both of the two long-promised volumes of his "History," and of himself before his constituents in Edinburgh. But we soon discovered internal evidence in the work itself that all these conjectures were wrong, and that the biographer of Dr Sheridan Muspratt is no other than that literary character—the renowned Mr Briefless of "Punch." We stake our reputation as critics upon this opinion, at which we have arrived by discovering an unmistakeable resemblance in mental characteristics between the biographer now before us and the celebrated contributor to our facetious contemporary.

We have a word or two, however, to say to Mr Briefless. We have no objection to his employing his pen as much as he pleases;



but we must protest against his doing so at the expense of the feelings of another; and therefore we tell him that the hoax of which he has been guilty, in writing such a biography as this of any man living in the position of a public teacher, is abominable. It is one of those practical jokes more replete with malice than humour, and with which we have no sympathy.

Mr Briefless begins his "Life of Dr Sheridan Muspratt" with the following judicious remarks on the utility of biographies in general.

"To look into the inner lives of celebrated men, and see the working of those powers which elect them from the multitude, has always been held a study refreshing and instructive to the mind."

There is a sneer in this exordium to the biography of a living man, which shows at once the animus of the writer; and we find that the same spirit pervades the whole pamphlet. We are told, for instance, that—

"At an age when others were only stripping for their work, he was already gaining ground; not tempted to exertion by a hope of wealth, nor urged by the spur of adversity, but obeying the impulse of a love for science, which worked by the force of its intensity;"—

that in 1843, when he went, like many others, to study chemistry under Liebig,—

"It was not long before the prince of chemists recognised in his pupil that energy and ardent love for chemistry which afterwards created between them the closer relationship of friendship." \* \* \* \* "That, so pleased was the last [Liebig] to possess such a pupil, that he considered this a reward sufficient of itself for the advantages of his instruction, and refused to receive the customary fees;"—

and we are called upon, after perusing a recital of what was done by him at Giessen, to

"Pause a moment to admire the energy which had exhausted the instruction of three universities, Glasgow, London, and Giessen, and achieved an European fame before twenty-four years of age;"—

and to confess—

"That Dr Muspratt has no reason to complain of any slowness on the part of his contemporaries to recognise the efforts he has made for the elucidation of chemical truths, the ability he has displayed, and the successes he has achieved."

Had the name of Scheele, Priestley, Cavendish, Davy, Berzelius, Faraday, or the "Prince of Chemists," been substituted here for Dr Sheridan Muspratt, such expressions as these would be set down as inflated bombast, and would only entail ridicule on the *Biographie*; but, when coupled with the name of a gentleman who is entitled to respect, at least, for working at chemistry when others with his means are too apt to spend their time in less commendable pursuits, we can look upon them only as intended to throw ridicule on the *Biographé*, and, through him, on his favourite science. It was heartless of Mr Briefless to do so, and doubly heartless to append to



the biography a third edition of a paper by Dr Muspratt, as if the whole proceeding had his full concurrence.

We have said enough to expose the animus of this nameless writer; but there is one other evidence of it which we must denounce as something unparalleled in the annals of anonymous literature. Mr Briefless, in his exordium, assumes a very peculiar style of writing; and then, at a subsequent page, gives a letter professedly from Dr Muspratt, and ostensibly as a specimen of his powers as a writer, but written so precisely after the model of what has preceded it, as to leave little doubt on our mind that one pen indited both. At least, if the letter be genuine, we should feel compelled to acknowledge that Mr Briefless has a wonderful power of imitating the style of another.

Here is what Mr Briefless gives as Dr Muspratt's:—

“Sir,—As art is illimitable, improvement acknowledges no point at which it may say to itself ‘I must stop.’ In that which seems perfect to-day, the examination of to-morrow discovers some demand for alteration or addition; so that the work which genius had laid aside as finished, it now takes up again as a subject for the renewal of labour. But human enterprise, in order to fulfil thoroughly its mission, requires too frequently something more to stimulate it than the mere anticipation of successful achievement. Effort is not unfrequently attended with contingencies which render it prostratingly painful, and entail a duration overpowering to even more than ordinary patience, and consequently stands in need of cheering that it may not be utterly given over,” etc.

Here is what Mr Briefless gives as his own:—

“A wonderful theory it is to think that while there is hardly a mind educated in any branch of science which cannot understand its discoveries when once they have been made, it is only to an individual here and there that it is given to throw light on the untrodden paths of science. It shows how universal is the superiority of a great over a little mind; and suggests how possible it is—arguing with mere reference to this disparity, for other testimony is more than ample—that an almost infinite series of orders of intellect may rise above one another in a super-mundane economy, perhaps with even greater progress from inferiority towards that which is perfect or infinite. We believe that to some such reflection and to the conviction of weakness it produces, do we owe the humility which distinguishes those who *can* from those who only presume that they can.”

We repeat that we cannot reprehend too strongly such practical jokes as this.

If we cannot commend the spirit of this book, however, we must do justice to the correctness and excellence of its typography. We have detected only one error of the press. It refers to a portrait of Dr Muspratt, representing him with an amount of moustache—whisker, beard, and imperial—that would have affrighted the most hairy “pard” that Shakespeare ever saw in a dream. Concerning this picture we are told, that “The portrait which accompanies this biography is taken from an excellent photograph by Beard.” We apprehend that this is a typographical error, and that it ought to be, “The beard which accompanies this biography is taken from an excellent photographic portrait.”

## COLLOQUIA DE OMNIBUS REBUS.

COLL. V.—DE REMEDIIS NOVIS, SPECIFICIS, DIABETICIS, ETC.

*Obstetricus* [to *Chirurgus*]. Might a friend venture to inquire what has disturbed your equanimity this evening?

*Chirurgus*. Even yours would have been unsettled by the gentleman's story, who drove from my door as you arrived.

*Chemicus*. A tall, handsome, young fellow. I wondered to see him leaving your hospitable gate at such an hour.

*Chirurgus*. He is not in condition to enjoy hospitality, and came here for a very different purpose. He is one of the

VICTIMS OF MERCURY. Passing through Edinburgh with a mercurial sore-throat, a pocketful of mercurial prescriptions, and a mercurial belt, he felt uneasy travelling with three such unsafe companions, and came to see what I thought of him and them.

*Chemicus*. He would be surprised to learn that the root of his misfortune lay in his belt and recipes, and not in his throat.

*Chirurgus*. Very possibly. But I have not yet told you all. Led by incidental circumstances, he had been for some time indulging freely in wine and wassail, and living a life of hard exercise and constant exposure. On expressing my wonder at this, he told me, to my consternation, that the London surgeon, who advised him to poison himself with mercury, had not put him on his guard, or under any rule or restriction, as to diet or regimen. You may judge what reason I had for appearing discomposed.

*Chemicus*. The traveller has cause to thank his stars and his constitution of "oak and triple brass," that he had not bid adieu to his nose and palate at least. What a fearful amount of misery must arise from the waggon-loads of mercurial pills and potions which are administered in London to all sorts of weak and scrofulous victims of venereal disease! It is a subject of painful reflection to every mind not proof against every humane consideration.

*Chirurgus*. The *Athenæum* tells us the other day, that medical men "have a vested interest in fever and cholera: their estate consists in the foul places, the bad drains, the putrid heaps of the city grave-yards." If this opinion, which is doubtless founded on acquaintance with the sentiments of the author's medical friends—should fairly represent the tone of metropolitan medical ethics, it would be unreasonable to expect the abandonment of the mercurial treatment of syphilis. But we must hope that things are not quite so bad as might appear from the *Athenæum*. In every medical community there must be numbers of professional men who are not so blinded by the pursuit of gain as to have their eyes shut to the truth, because it may affect their pockets. There are even

some bright exceptions to the dogmatic mercurialism of London surgery.

*Medicus.* Do you mean to tell us, that, after what has been done and written about syphilis and mercury during the last forty years, a non-mercurialist is still the exception in London practice?

*Chirurgus.* Certainly. Have we not perpetual proof of this in the contents of the London journals, and in such living illustrations as my belted traveller—whose case, I can assure you, is by no means a solitary one in my observation.

*Medicus.* This is deplorable. When I first went to London, in 1820, satisfied by frequent experience in our Infirmary here, of the soundness of the non-mercurial doctrine, first propounded by the medical officers of the army, and then systematised and powerfully advocated by Dr John Thomson, I was shocked to find, as pupil of one of the great metropolitan hospitals, its “foul-ward” patients salivating, many of them for the second, third, or fourth time, and its surgeons ignorant or regardless of the glorious victory over mercury gained by our army surgeons, and conclusively followed up in the North. Returning thither in 1838, I expected to encounter truth at last in the ascendant; but in vain. After the lapse of eighteen years there were the same wards, the same fetid atmosphere, the same mercurial victims—other surgeons, but the same ideas. Is it possible that fourteen years more have wrought no decay in that old donjon-keep of prejudice?

*Physiologus.* I can add my testimony that matters were in the same state in 1833; having found in its attics the same sort of patients, and spit-boxes, and atmosphere, and notions that year, while a pupil, as you did in 1820.

*Obstetricus.* When *Chemicus* and I accompanied the late Mr Bransby Cooper at his visit in Guy’s Hospital in 1836, we ascertained that every surgical patient in the hospital was taking mercury in one shape or another; and there is no reason to suppose that matters are any better yet, so far as syphilis is concerned.

*Chirurgus.* The more need, then, for us to show the contrast; which the Managers of the Infirmary have just put it in our power to do. The great additions now made in the new buildings will afford ample accommodation for venereal patients, who for many years have been excluded from the hospital. We shall thus enable the student, as well as others, to learn from personal observation the truth of the principles, which have been so long taught and practised in Edinburgh:—that “Hunterian chancres” and other primary affections may be cured by simple local treatment, without any mercury; and that in most secondary cases, mercury, instead of being an antidote for venereal infection, is another poison, and nothing else.

*Editor.* But would you consider so slight a matter as a Hunterian chancre a fit subject for hospital treatment?—it is such a trifle now under the non-mercurial method.

*Chirurgus.* The more occasion to prove to our unbelieving neighbours that it is so.

*Editor.* And where will you obtain in Edinburgh secondary cases of such severity as to instruct pupils or convince sceptical Southrons?

*Chirurgus.* Edinburgh can still supply a few of indigenous growth, thanks to one or two surviving home believers in the specific virtues of mercury against syphilis; and any want of native produce will be amply made up by arrivals from other parts still groaning under the mercurial curse.

*Editor.* To what do you ascribe so great a disregard of advancement in therapeutics as this dogged perseverance of our London brethren in the mercurial delusion?

*Chemicus.* To metropolitan indifference for improvement originating from without;—Roman contempt for everything barbarian.

*Physiologus.* Don't you think it may be rather referred to the prevalence there of a blind, degrading faith in Specifics, of which this mercury in the cure of syphilis has long been the chief?

*Medicus.* To both the one and the other concurrently, but at bottom to an imperfect, unsound, therapeutical education.

*Chemicus.* Why look farther than to metropolitan apathy towards "outside" improvement. For example, there has not been a single improvement of any importance made here in the treatment of diseases during the last five and twenty years that has been admitted into London practice, except tardily and imperfectly, if admitted at all.

*Medicus.* That is a bold proposition, yet true, and which, I doubt not, you can substantiate, if it be called in question. It may well rouse our metropolitan friends to serious reflection. But meanwhile, look a little beyond this state of things, and I think you will find its origin to be mainly a radical defect of tuition in therapeutics.

*Chemicus.* It was a marvellous step backwards, when in 1850 the whole Boards of medical education in London, by incomprehensible common consent, reduced their requirements in materia medica to a course of lectures of three months.

*Medicus.* A heavy blow and discouragement truly to therapeutics. And more than this:—it is a proof to me that the nature and scope of therapeutics have not yet been duly appreciated in the London schools, or by the Boards of education there.

Is it possible to estimate too highly the importance of this branch of medical science? What is the ultimate object of medicine but the cure of diseases? What then ought to be the ultimate object of all medical education, if it be not the knowledge of the means of cure? To what purpose should we teach anatomy, physiology, chemistry,—why pathology and diagnosis,—if we did not possess remedies, medical and surgical, which we could put into the hands of students when so instructed? But fortunately we do possess them,—indeed in

too lavish profusion. And the best of them are hard to obtain, difficult to know, variable in quality, puzzling to select, nice to prepare, but above all most wonderful in action,—energetic, multifarious, complex, versatile, and singularly influenced by co-operating circumstances.

The ancients knew all this : Therapeutics, indeed, with semeiology, constituted almost their whole circle of medical science. The early modern physicians knew it also : Witness Matthioli's great folio *Commentationes*, which went through eleven editions during half the sixteenth century. Alston, the first British professor of *materia medica*, knew it. He started in this University in 1738 with a course of lectures of six months in duration, and I have never heard that either professor or student has since found the period too long. In all great medical schools of the present day, except one, the same opinion has prevailed. In Britain, under the united name of *Materia Medica*, on the Continent under the separate heads of Pharmacy and Therapeutics, the means of curing diseases are taught in just equilibrium with the other branches of medicine. In London alone has it entered into the understanding of man to conceive that pharmacy, therapeutics, diet, and regimen may be mastered by a student in sixty lectures. When, indeed, University College, and afterwards King's College, were founded on the model of that of Edinburgh, an attempt was made to place the *materia medica* on a satisfactory footing, and other London schools followed the example. But after a twenty years' trial, the attempt, it seems, has signally failed ; and in 1850 both the London College of Surgeons and the Apothecaries' Company reduced their requirements in *materia medica* to the old miserable standard.

*Chirurgus*. Possibly they thought that all which is at present positively ascertained on the subject may be taught in three months.

*Chemicus*. If professors of medicine and surgery were to teach only what is positively known in their several departments, few of them would require more time. It is the very uncertainty of *materia medica*, and especially of therapeutics,—the number of doubtful points to be discussed, the quantity of falsehood to be cleared up, the amount of fashionable humbug to be exposed, that entail the necessity of deliberate tuition.

*Medicus*. Exactly so. But unfortunately, in the London system there has long been no time left for anything but hasty tuition in this and some other equally important branches. The dominant influence of the College of Surgeons as an educational body,—the partial, narrow views of their Council, who now, as in time past, will look to nothing but anatomy and surgery as deserving of earnest attention,—have been the main cause of this. With the Council of the College, Anatomy and Surgery have been everything ; at least every thing else is little more than nothing. Even Physiology and Pathology are by their regulations mere offsets or appendages to

anatomy, and to be taught as branches of it,—a very natural error for a body composed entirely of hospital surgeons and lecturers on anatomy and surgery, and in which no other branch of medical science or art is represented. And as for the Apothecaries' Company, it is easy to see why they do not encourage the science which they ought peculiarly to foster: they cannot even yet overcome the old hallucinations that apprenticeship is education, and that a student, who is constantly handling drugs, must necessarily come to know all about them.

The consequences of all this might have been foreseen. What their directors undervalue, students do not prize. What the magnates of the profession do not cherish, the masses neglect. Therapeutics has ceased to be an object of inquiry, or is cultivated without method or principles. No one seems to care to improve our knowledge of old remedies. There is an incessant thirst for new ones. But these are sought for by the rule of chance; and not so much because they are needed for the purpose to which they are applied, and for which there is no want of acknowledged means; but apparently to satiate a morbid public craving for novelty, or to serve as a periodical invitation and advertisement. A wide-spreading empiricism broods over medicine, penetrating even into high places; and quackery of all kinds grows rank under its shade, pervading even the regular profession.

*Obstetricus.* You take a gloomy view of things. But the very magnitude of the evil will by and by work out its own reformation.

*Medicus.* It is not easy to avoid despondency, when one beholds, in relation to so essential a branch of medical science and practice, the ignorance of the profession, the advance of quackery, the sneers of the public, and the apathy of our medical rulers.

*Chemicus.* "Apropos des Charlatans," I see

A NEW HOMŒOPATHIC PETITION against the University of Edinburgh has been presented to the Town Council, its Patrons. What do they want now?

*Editor.* The same favour as formerly;—that the Patrons shall compel the University to graduate homœopaths. But the Patrons have wisely shelved the petition by transmitting it *simpliciter* for the perusal of the Senate. It is a pity however they did not see they were merely made a catspaw of,—being set to talk about homœopathy at the Council Board, and thus to issue unwittingly a homœopathic advertisement. The originators could have no other aim with such instruments as their petitioners.

*Chemicus.* Who are they this time?

*Editor.* Nine hundred and fifteen decent tradesmen, operatives, and servants, with a remarkable predominance of the feminine gender, and especially a large assortment of housekeepers, cooks, and chambermaids. On this occasion there is neither lord, nor admiral, nor general, nor churchman.



*Obstetricus.* Is not the Archbishop of Dublin among them?

*Editor.* No. But they quote him in the body of their petition as one of their backers.

*Chirurgus.* Then let us leave the matter with the Archbishop and the chambermaids. It is in very safe keeping in their hands.

*Chemicus.* Reverting to the pestilence of new remedies with which medicine has for some time past been assailed, is there no short-hand way of bringing them to trial and condemnation? No lifetime is long enough to test them in the ordinary way.

*Medicus.* Test them, in the first place, by the principles of therapeutics, and most of them will be at once disposed of. We have only to look to the classification of known remedies, according to external characters and composition, in order to see that very many modern novelties in the materia medica are mere delusions. For remedies so classified possess generic actions, proper to each group, with which the actions of unknown individuals of the same group must in general coincide.

*Chemicus.* But anomalies in action exist among known individuals of the same natural group. Why not among the unknown?

*Medicus.* In a more advanced state of therapeutic science these anomalies will disappear one after another. They will be found to be parts of subordinate and intercurrent laws, which may direct the choice of remedies as much as the fundamental laws of the action of natural groups. For even already the existence of these fundamental laws is so well established, and many of the exceptions are so well accounted for, that a strong presumption of the value of a supposed new remedy may be formed by one who has made this interesting subject his study.

*Editor.* And what is to be done when this test fails, or is inapplicable in the present state of our knowledge?

*Medicus.* Make trial of such remedies by all means, and dispassionately; but with jealousy, if their alleged virtues violate the general rule of agreement in family properties; and above all, if they are put forth as specifics,—a term which appears to be used in the present day whenever no reason can be assigned why remedies act, or why they were resorted to.

*Chemicus.* I admit that when a remedy is spoken of as a specific, the word simply means, that we know nothing of its action. But do you think that, as a general rule, we are likely to be directed to new remedies by the consideration of their family position?

*Medicus.* Not for the present perhaps. But such will be the common rule no doubt, when the medical profession shall, for some five-and-twenty years, make it their duty, in all civilised countries, to throw their whole force on the study of therapeutics, as has been done with such signal success for pathology during the twenty-five years that are just past.

*Physiologus.* And meanwhile we are even already not without valuable instances of therapeutic theory successfully guiding practice

in the choice of new remedies. Take chloroform for an example. The properties of chloroform were not discovered by accident. Sulphuric ether having been ascertained to be an anæsthetic, all toxicological experience and theory led to the conclusion, that other ethers and etheroids would possess similar properties; and, accordingly, several such substances were found out, and chloroform at the head of them all for energy, safety, and facility of administration.

*Medicus.* Another excellent illustration is the gradual progress by which we have arrived at the most modern

**TREATMENT OF DIABETES.**—Having attained something very like a true pathology of the disease,—having discovered that it is not a disorder of the kidneys, but a depraved digestion,—and having ascertained the chemical composition of all the principal articles of man's food,—by theory it was at once inferred, that a number of old remedies in the shape of physic, and many new ones still proposed from time to time, may be allowed to sink into oblivion. By theory, too, we know that a peculiar regulation of the diet constitutes the only sound treatment; and we know also what articles compose that diet,—thus already making a great stride towards the cure. For, by the substitution of gluten-bread and cakes made of bran, butter, and eggs, for ordinary bread and other farinaceous food,—and by allowing such vegetables as spinage, cauliflower, brocoli, and cabbage, which contain little or nothing capable of conversion into sugar, we have rendered a permanent nitrogenous diet practicable, which it was not before,—and so we effect sometimes a cure, and often a most material amendment, which may be maintained indefinitely by due dietetic observance.

*Obstetricus.* Have you seen any one recover entirely in that way?

*Medicus.* A gentleman of 65 recovered entirely three years ago, and continues well, unless he exceeds at table; and another of 25, and a third, a boy of 13, are greatly improved,—the latter, indeed, might be thought in all respects well, except that the urine continues saccharine.

*Obstetricus.* Although we do not now know any medicine to improve this state of things by directly controlling the morbid peculiarity of digestion which constitutes the disease, who knows that theory may not soon direct us to one?

*Medicus.* It is much more likely to do so than empirical trial, that is, accident,—which has been hitherto followed as the main guide. Indeed, I know not but that it may have actually pointed out a remedy already. At least I have just received some very apposite information, which may interest you, relative to an entirely new remedy, derived strictly from theoretical considerations,—namely, the

**TREATMENT OF DIABETES BY RENNET,** which seems to promise well. Dr Gray of Glasgow was lately induced to make trial of

this substance by the following theoretical views. Diabetes consists in the process of digestion stopping at the conversion of other organic principles into sugar, which cannot be oxidated in the lungs, and is therefore thrown off as excrementitious by the kidneys. But rennet out of the body converts sugar into lactic acid, and it may therefore do so within the body likewise. Should such conversion take place, however, the disease will be brought to an end, if Liebig be right in his opinion, that lactic acid is one of the principles of the organic world which can support respiration, by becoming oxidated in the lungs. Resting on these views, Dr Gray tried rennet in the case of a patient so much reduced by diabetes, of at least twelve months' standing, as to be unable to work. Dietetic treatment had been only of partial benefit. Medicines of various kinds had been of little use. The urine was copious, 1045 in density, and strongly saccharine. On the 30th of last July, a teaspoonful of rennet, prepared as for the dairy, was given thrice a-day. In eight days the density of the urine was reduced to 1025, and it contained lactic acid, but only a trace of sugar. In twenty-five days the quantity was sixty-four ounces, the density 1022·5, and the sugar gone entirely. In six weeks the urine continued free of sugar; the man had gained weight considerably; his strength was such as to enable him to return to his employment; he thought himself in as good health as before his illness; and nevertheless he had been ten days on nearly his usual allowance of wheaten bread.

Now I am far from meaning to say, nor does Dr Gray say, that rennet is thus proved to be a remedy for diabetes by its apparent success in a single case. But it is surely the most feasible remedy that has been proposed for many a day;—so feasible, that I hope many will give it at once a fair trial, which is his object in allowing me to give this brief notice of it to you all. Should it prove as successful in other hands as in his, we shall owe another therapeutic discovery to therapeutic theory.

*Obstetricus.* Were all inventors in the *Materia Medica* as well trained in therapeutics as Dr Gray appears to have been, we should have fewer new remedies to deal with, and probably more good ones. It is certainly a striking confirmation of your criticism on London therapeutics, that, among the many new London remedies, not one has been announced for some years, which has stood the test of experiment elsewhere.

*Medicus.* A very natural consequence of the contempt manifested everywhere in London for therapeutic instruction. By the way, I forgot to advert to a most extraordinary circumstance connected with the discountenancing of this branch of medical knowledge by the London boards of education,—viz., the complete and universal silence and submission with which their degrading regulations have been received. Not a single teacher has publicly uttered a single remonstrance. Not a journal has issued one word of criticism. Thera-

peutics, it seems, has not a patron in the whole metropolis. But enough of this for the present.

*Physiologus.* You mentioned a little ago that we had arrived at something like a sound pathology of diabetes, and that it seems to be a disease of digestion. But you are aware that this view may require revision, since the recent discoveries of M. Bernard, relative to the functions of the liver, by which he has proved that

#### SUGAR IS A NATURAL PRODUCT OF THE LIVER.

*Medicus.* That is possible. We do not yet see how the singular observations of Bernard are to affect the pathology of diabetes; but that they must have important bearings on it we can scarcely doubt. His inquiries have received too little attention in this country as yet. You have studied them carefully, and indeed have witnessed his leading experiments. Will you give us some account of them?

*Physiologus.* Within the last two years M. Bernard has brought forward a theory as to the production of sugar in the blood, which is supported by an amount of experimental proof that cannot be easily set aside. He admits that sugar may be formed in the process of digestion, and that a certain amount of it may, as the result of absorption from the alimentary canal, find its way into the blood. But he has shown that in man and animals of various orders, even so low down in the scale of creation as acephalous mollusca—if they are even fed entirely upon flesh—the blood from the hepatic vein invariably contains sugar. It is the result, however, of digestion of the food: for it disappears when an animal is starved, and it re-appears when food is again given. He further observed, that sugar is found in the liver independently of the nature of the aliment. In dogs fed exclusively on animal food for several months, though he could find no sugar in the intestines or portal blood at its entrance into the liver, he always found it in the liver itself, and in the hepatic vein. In the spring of 1851 M. Bernard was good enough to perform the following experiment in my presence, during a visit I paid to Paris. A ligature was tied round the vena portæ where it enters the liver, and the dog was immediately killed by dividing the medulla oblongata. On opening the abdomen, the portal blood below the ligature, and blood from the hepatic vein, were immediately collected in separate glass vessels; and it was at once demonstrated, by applying the same test to both, that the latter contained sugar in abundance, but the former none. Sugar was also found in water in which a piece of the liver had been boiled in chips. Such an experiment seems decisive of the fact, that sugar is formed in the liver, and not conveyed to it with the blood through the vena portæ. Subsequently M. Bernard found that sugar is formed even by the foetal liver; for he detected it in that organ both in mammals at different stages of intra-uterine life, and in birds before being hatched.

In all cases the sugar so formed presents the characters of grape-

sugar. In all cases it is quickly decomposed on coming in contact with the blood and animal tissues. Hence, even in the livers of animals, it can be discovered only for a short time after death.

M. Bernard next discovered, that section of both pneumo-gastric nerves, as well as any violent shock to the nervous system, destroys the power of the liver to form sugar. The most interesting, however, of his observations, and that which bears most pointedly on the pathology of diabetes, is, that irritation of the root of the pneumo-gastric nerves in the fourth ventricle of the brain increases the formation of sugar in the liver, and causes it so to abound in the blood that it is secreted with the urine: in short, this operation produces artificial diabetes. M. Bernard showed me this remarkable experiment. Having squeezed some urine from the bladder of a healthy rabbit, he proved that it did not contain sugar. He then passed a needle through the skull in such a way as to irritate the pneumogastric roots, and let the animal rest for an hour after the slight convulsions excited by the injury. Sugar was then found largely in its urine. On then killing the rabbit, it was found that the needle had wounded the intended part. I have since repeated this interesting experiment, with the same result; and so has my former assistant, Dr Drummond: so that there can be no doubt of the fact.

*Medicus.* It has also been lately repeated with success in many trials by Dr Schrader, as announced to the Royal Society of Sciences at Göttingen in the beginning of the present year.

*Physiologus.* M. Bernard has since informed me of the results of his farther researches on this subject. He has now discovered, that, although section of the pneumogastric nerves destroys the formation of sugar in the liver, it is restored by artificially irritating their cut extremities; and that diabetes is produced exactly in the same manner as by irritating their origins in the brain. He was therefore led to conclude that the nervous action on the liver, necessary for the secretion of sugar, is not direct along the pneumogastriacs, as he formerly supposed, but indirect, or reflex, through these nerves as incidents, the medulla oblongata as the centre, and the spinal cord communicating with the solar ganglion as the excident channels of communication. And following out this theory, he likewise found that whenever the respiratory function is violently stimulated, sugar appears in the urine, and that whenever ether or chloroform is given, a temporary diabetes is occasioned. It follows that the formation of sugar by the liver is analogous to those kinds of secretion which are produced by reflex action through the agency of a sympathetic ganglion, and the influence of certain stimuli—such, for instance, as the secretion of saliva caused by the presence of sapid bodies in the mouth, where the sensitive and motor branches of the fifth pair operate in a reflex way through the agency of the sub-maxillary ganglion. In this case, stimulating the tongue is necessary to cause a flow of saliva; and in like manner, a certain stimu-

lus of the lungs (normally by the air) is necessary to cause the formation of sugar by the liver. M. Bernard further supposes, that in the same way that the lungs thus act by reflex nervous influence on the liver, so does increased action of the liver act upon the kidney; consequently, that the sugar, produced in excess by one organ, is excreted by the other.

Such is the present state of the question. Various pathological considerations might be stated which seem to show that Bernard's liver theory of the origin of diabetes is as consistent with facts as the theory which ascribes it to disorder in the stomach. But further inquiry is necessary before we can positively settle the real cause of that very mysterious disease. Meanwhile, it is not easy as yet to see how the discoveries of Bernard will enable us to improve the treatment of diabetes, unless the well known symptom of dryness of the skin, by exciting the lungs to increased transpiration, be connected with the cause of the disorder, in which case diaphoretics, though they have been often used with some benefit, would be more strongly indicated. But I think something will be learnt on this head ere long.

*Editor.* Gentlemen, I must beg you to excuse me for breaking up this colloquy so soon. I must prepare for an early start to Rotterdam.

*Physiologus.* And I to Paris.

*Chemicus.* And I to the Doune of Rothiemurchus.

*Chirurgus [aside].* And *Medicus, Obstetricus,* and I, to the top of The Cobbler.

## Part Third.

### CLINICAL REPORTS, LECTURES, ETC.

#### CLINICAL MEDICINE.—PROFESSOR BENNETT.

##### ERUPTIVE FEVERS.

There are certain diseases which, in an arbitrary classification, may be considered as febrile eruptions, or as eruptive fevers. They comprehend especially scarlatina, erysipelas, variola, and rubeola. Occasionally roseola, herpes, or other cutaneous eruptions, may be attended with fever, but they are separated from the others by their non-contagious or infectious nature. Plague and glanders, on the other hand, are true eruptive fevers; and, with the others mentioned, obey certain laws, which may be shortly noticed.

1. They may be infectious and contagious. By infection is understood the power of being propagated through the inhalation of air tainted by the breath or perspiration of the affected person. By contagion is understood communication by actual contact.



2. The present theory, with regard to the cause of these diseases, is, that it depends upon a morbid poison, a small quantity of which entering the blood produces in that fluid a peculiar change which is analogous to that of fermentation. To distinguish this change in animal from what occurs in vegetable fluids, the term *zymosis* has been introduced by Mr Farr (from *ζυμώω*, to ferment).

3. Some of these animal poisons, if excluded from the air, or carefully dried, will retain their communicating property for a longer or shorter time. This enables us to preserve matter for artificial inoculation. Hence also they have been supposed capable of attaching themselves to fomites—that is, substances of a rough surface or downy texture, such as wool, cotton, wearing apparel, dust, etc. It is on this theory that quarantine regulations are founded, the whole of which, together with the facts, real or supposed, that support them, require a thorough revision.

4. All the animal poisons are distinguished by peculiarities in their mode of incubation and development. Thus a period of latency exists between exposure to the poison and accession of the fever, or first rigor. Again, the eruption appears at different periods after the fever is declared. Thus—

	Period of Latency from	Appears after first Rigor in from
Scarlatina, .....	4 to 8 days	..... 18 to 24 hours.
Erysipelas, .....	4 to 7 days	..... 24 to 60 hours.
Variola, .....	8 to 14 days	..... 48 hours.
Rubeola, .....	7 or 8 days	..... 72 hours.

5. All the eruptive fevers, strictly so-called, invariably run a natural course, and cannot be cut short. It follows that—

6. The treatment of febrile eruptions has for its object conducting these cases to a favourable termination. To this end exactly the same general rules are to be followed as I previously gave when speaking of continued fever, and the same indications exist for the use of salines and laxatives, cold to the head, wine and stimulants, and regulation of the diet. These I need not again repeat, and I shall confine my observations at present to the more special treatment of the diseases we have studied in the wards.

#### *Scarlatina.*

CASE I.—Mary Fraser, æt. 9, was admitted on the 19th of November 1851. On the 17th inst. she was attacked with rigors, followed by nausea, great thirst, hot skin, and quick pulse; and on the 18th, a moderate scarlatina eruption appeared upon the skin; it is best marked upon the arms. The eyes are considerably suffused, and the skin very hot; the pulse is 140; breathing difficult, tongue furred, and considerable pain on deglutition; the tonsils are enlarged, as also the submaxillary glands, to a slight extent; there is great nausea, with occasional vomiting. *Four leeches to be applied over larynx.* R. *Pulv. Doveri*, gr. iv.; *Pulv. Jacobi*, gr. ij. M. *Sumat i. tertia* q. q. *horâ.* November 20th.—Pain in throat diminished; pulse 130. *A blister, two inches by three, to be applied to the throat.* R. *Aquæ Acet. Ammon.*, ʒij.; *Tinct. Opii.*, ʒss.; *Aquæ*, ʒij. M. *Sumat, ʒj. secunda* q. q. *horâ.* To continue the powders. November 23d.—Febrile symptoms increased. To substitute a mixture containing a little antimony and morphia for that ordered on the 20th. November 26th.—Eruption is gone, skin is cool, and there is no feverishness. To discontinue the antimony. Continued gradually to improve, and was dismissed cured on the 13th of December.

CASE II.—Mary Clark, æt. 17, servant, admitted December 20th, 1851. On the afternoon of the 17th her throat became sore, and in the evening she was attacked with rigors, followed by pain in the head and back, and other febrile symptoms. Last night she first observed a red rash upon her chest and arms;

this is of a reddish-brown colour, and resembles the ordinary eruption of scarlatina; it disappears upon pressure. Pulse 126 and feeble; fauces, tonsils, and back of pharynx red and congested; has great thirst and anorexia; tongue moist, with a white fur in middle, through which the red papillæ project; bowels costive; urine, sp. gr. 1030, contains no albumen,—a deposit takes place, containing epithelium scales and crystals of triple phosphate. R. *Tinct. Hyocy.*, ℥ss.; *Liq. Ammon. Acet. et Aquæ puræ ad*, ℥iij. *M.* ℥j. *tertia q. q. hord.* December 22d.—Rash disappeared from arms, still visible on the chest; pulse 86 and soft; less pain in the throat, but fauces and palate are still congested. December 24th.—Convalescent, and she was dismissed on the 27th of December cured.

CASE III.—Isabella Husketh, æt. 22, a woman of abandoned character, and addicted to intemperance, was admitted 19th December 1851, in a state of high delirium. It was ascertained that, on the 14th, she had been seized with rigors, followed by great debility, catarrh, and general febrile symptoms. On the following day an eruption appeared on her skin. On admission, she is in a state of violent delirium, and obliged to be tied down in bed. Her eyes are suffused, and very sensitive to light; pulse 120; tongue dry and parched, florid-red at the edges, with the papillæ projecting through a white fur in the centre; teeth covered with sordes; great pain in throat, increased by swallowing; submaxillary glands tender on pressure, but not enlarged; eats nothing, but has great thirst; bowels costive; skin hot and pungent; arms and chest covered with a bright scarlet exanthematous eruption. *Six leeches applied to the throat—saline mixture.* December 20th.—Delirium continues; pulse 125; pain in throat relieved. *Vespere.*—Delirium greatly increased. *Nine leeches applied to temples, and to have a draught of morphia, and some wine.* December 21st.—Slept during night, and is nearly sensible to-day; tongue dry and florid; eruption fading; considerable sore throat. *Blister to be applied to the throat.* On the 23d the eruption had quite disappeared. The throat symptoms, however, gradually increased. On the evening of the 26th, the breathing was observed to be very short and hurried, and on the morning of the 27th the patient died.

*Commentary.*—The two first cases are instances of the mild scarlatina, running its ordinary course, and terminating in recovery on the ninth and seventh days. The third case is an example of severe scarlatina, occurring in a woman addicted to intemperance, and in whom all the symptoms of typhus fever, associated with sore throat, were present, proving fatal on the thirteenth day. Of all the eruptive fevers, scarlatina is the most rapid in its invasion, and the most simple in its course. Great watchfulness is therefore demanded on the part of the practitioner, especially when the crisis is to be expected, so that if the pulse falters, and prostration comes on rapidly, he may be prepared to meet it. Perhaps, also, scarlatina is the most infectious of the eruptive fevers, so that complete separation of the patient from the other members of a young family is at all times to be insisted on as soon as possible.

A chief peculiarity of scarlatina is, that, in addition to the general fever and characteristic eruption, the tonsils and mucous membrane of the mouth and pharynx are also inflamed. This occasions difficulty of deglutition, with soreness of the throat, symptoms which require for relief topical remedies,—such as leeches, fomentations, astringent and slightly acid gargles, or a linctus, etc. If sloughing or ulceration occur, the application of the stronger acids, or the nitrate of silver, is often necessary. The difficulty of deglutition sometimes impedes the introduction of food into the stomach, and in this way assists in producing prostration, and prevents the administration of stimulants or medicine. It may also, in severe cases, impede respiration, and assist in producing asphyxia directly. A fatal result, however, when it does occur during the primary attack of scarlatina, is generally dependent on the same causes which induce it in typhus fever—namely, congestion of the brain, as indicated by delirium, passing into coma, and followed by prostration of the vital powers.

In addition to the throat complication, there are various others, all of which may require a special treatment. In the vast majority of cases, however, a general treatment, directed in the first place to subduing the excess of fever, and afterwards to supporting the strength, is indicated.

Many efforts have been made by different practitioners to check or modify the intensity of the disease by administering various drugs, or carrying out particular kinds of treatment. Hence, during certain epidemics, or in its visitations to particular educational institutions, various practitioners have been sanguine enough to believe that their especial mode of practice has been more successful than any other. I do not consider it necessary to direct your attention to the numerous plans which have been thus proposed, because all of them have been only partial in their operation, and no one of them has been more successful than another. You must remember that the causes of scarlatina are as mysterious and unknown as are those producing every kind of fever; and that its fatality, like that of fever, is to be traced to constitutional circumstances in individuals, to unhealthy localities, or to the so-called type of the particular epidemic. Nothing, therefore, is more difficult under such circumstances than to judge whether the non-fatality observed at one time, or in a certain establishment, is referable to this or that practice. At all events, I have been unable to satisfy myself that any general rule of empirical or rational practice is to be derived from the contradictory accounts which have from time to time been made public on this subject.

The most recent system of treatment which has been brought forward is that recommended by Dr Andrew Wood; and I notice it in deference to the great experience that gentleman has acquired from his position as physician to Heriot's Hospital and other educational establishments in this city, which have been attacked by numerous epidemics of the disease. He considers that the most efficient and safe method of treatment consists in acting powerfully on the skin, with a view of thereby assisting nature to eliminate the scarlatinal poison from the system. As ordinary diaphoretics frequently fail, he has recourse to the following method:—Several common beer bottles, containing very hot water, are placed in long worsted stockings, or long narrow flannel bags, wrung out of water as hot as can be borne. These are to be laid alongside the patient, but not in contact with the skin. One on each side, and one between the legs, will generally be sufficient; but more may be used if deemed necessary. The patient is to lie between the blankets (the head of course being outside) during the application of the bottles, and for several hours afterwards. In the course of from ten minutes to half an hour, the patient is thrown into a most profuse perspiration, when the stockings may be removed. In mild cases, the effect is easily kept up by means of draughts of cold water, and if necessary, by the use of two drachm-doses of *sp. mindereri* every two hours. In severe cases, where the pulse is very rapid—the beats running into each other—where the eruption is either absent or only partial, or of a dusky purplish hue—where the surface is cold—where there is sickness or tendency to diarrhoea—where the throat is apthous or ulcerated, and the cervical glands swollen, then he follows up the use of the vapour-bath by four or five grain-doses of carbonate of ammonia, repeated every three or four hours. Should this be vomited, then brandy may be given in doses proportioned to the age of the patients. Carbonate of ammonia he considers to act beneficially: 1st, by supporting the powers of life; 2d, by assisting the development of the eruption; and, 3d, by acting on the skin and kidneys. Where the vapour-bath was used early in the disease, and its use continued daily, or even twice or thrice a-day, according to circumstances, he has found that the chance of severe sore throat was greatly obviated. In regard to supervening dropsy, he considers that, by the use of the vapour-bath, with the other necessary precautions as to exposure, diet, etc., its recurrence is rendered much more rare. In the treatment of the dropsical cases, it was also very useful, and even might be trusted to entirely in some cases. Dr Wood also

condemns all depleting treatment, and even purgatives, during the first ten days, as not only not required, but positively dangerous, as tending to interfere with the development of the eruption. In the later stages, as well as in the dropsy, however, he thinks purgatives are often beneficial.

The general plan of this treatment appears to be so far rational that its object is to hurry forward the disease by applying damp heat to the skin, and by thus assisting nature to make her operations more perfect than they might otherwise be. In other words, by rendering the febrile eruption more complete, diminish the risk of its leaving behind it a tendency to subsequent disease. Whether this plan as a whole will, in practice, prove more extensively beneficial than any other, can only be determined by an extensive trial and careful comparison of the results. I propose, however, to try it in the next case which enters the wards.

CASE IV.—Margaret Welsh, æt. 18. Admitted 2d July. She is a servant-girl, who has always enjoyed good health until June 29th, 1852, when she experienced distinct rigors, followed by sore throat and febrile symptoms. She admits having called previously on a family in which the disease existed. On the evening of the 30th, a bright red rash appeared on the skin, and has continued ever since. On admission, the scarlatinal eruption is well characterised on the chest and arms. The skin is hot; pulse full, hard, and 132 in the minute. Tongue furred, with elongated red papillæ projecting through the white crust; great difficulty in deglutition; throat sore; tonsils and mucous membrane of pharynx swollen and red. There are also cephalalgia, slight deafness, and restlessness at night. Respiratory functions normal; urine healthy; catamenia regular. She was ordered by the resident clerk eight leeches to the head, a saline antimonial mixture, and eight grains of Dover's powder. On first seeing her the following day, 3d July, I found her in much the same condition as is described in the previous report; the skin still being hot and dry, and the eruption very vivid on the chest and arms. Hot bottles were ordered to be applied, encased in worsted stockings wrung out of hot water, as recommended by Dr Andrew Wood. July 4th.—A slight perspiration followed the use of the vapour-bath last night. To-day, the rash has partly disappeared from the arms, but is now present on the legs as well as chest. Pulse 130, small; urine not coagulable. *An astringent gargle for the throat—the vapour-bath to be again applied.* July 5th.—Profuse perspiration resulted last night from the use of the vapour-bath. To-day the rash has entirely disappeared; but there is great tenderness of the skin and in the joints on motion. July 9th.—Has continued much in the same condition, but to-day the appetite has somewhat returned, and she has eaten a good breakfast. Her joints are swollen, and there is still considerable pain on moving them. Desquamation commencing; throat ulcerated, and to be touched with a weak dilution of nitric acid; pulse 84, soft; *3iv. of wine daily.* July 26th.—Since last report has been slowly gaining strength, but is still far from well. The urine has been carefully examined daily, and has never presented coagulability on the addition of heat or nitric acid. To-day a distinct blowing murmur was discovered with the first sound of the heart, loudest at the base, and propagated along the vessels of the neck; pulse 76, of good strength. August 4th.—Went out a little to-day, and in the evening the feet commenced to swell. August 6th.—Swelling of feet increased. *To have a squill and digitalis pill three times a-day.* August 9th.—Edema of feet continues; urine healthy. *Venesectio ad ℥viij.* August 11th.—Edema of feet disappeared. This morning had a rigor. *Was ordered an emetic.* August 12th.—To-day is feverish, with great thirst and heat of skin; pulse 128, strong. *A saline mixture ordered.* August 17th.—Febrile symptoms continue, with tenderness over epigastrium; and *eight leeches* were ordered to be applied there. The cardiac dulness is extended. No friction, but a blowing murmur as formerly noticed at the base of heart; respiration somewhat embarrassed. August 20th.—Respiration normal; no

tenderness over epigastrium ; pulse 100, regular, and soft. The urine all this time has been tested daily, but has never been coagulable. To-day, however, a deposit existed in the urine, and several casts of the tubuli uriniferi may be observed in it with a microscope. September 7th.—Since last report she has been convalescent, and all her symptoms have gradually disappeared. The blowing murmur over base of heart is still present, but not so loud, and the increased dulness has disappeared. Dismissed.

*Commentary.*—On a former occasion I told you that I should try the diaphoretic plan of treatment recommended by Dr Andrew Wood, in the next case that entered the ward ; and accordingly it was followed in the one I have just detailed. On the first occasion the vapour-bath produced little effect, but on the second copious diaphoresis was induced. Yet it so happens that the disease, instead of being shortened or rendered milder, was unusually prolonged, and was followed by rheumatism, dropsy of the inferior extremities, and by pericardial effusion. The febrile symptoms terminated by critical deposition in the urine so late as the fifty-second day. Although admitted June 29, she was not strong enough to be dismissed from the Infirmary until September 7th. This was certainly an unfortunate case to commence the trial of a new treatment with ; and yet observe, the girl had been always healthy, and there was nothing to indicate at the commencement that the sequelæ would be so severe or so prolonged. It would be absurd, however, to suppose that we can test the value of any kind of treatment by one case. I only give you the facts as I find them. I shall certainly continue the practice until I am satisfied either that it is really beneficial ; or, on the other hand, no better than the simple treatment formerly pursued.

It has frequently been observed, that the urine in scarlatina, especially when dropsy supervenes, becomes albuminous. Dr James W. Begbie, who has tested the urine in a considerable number of cases of this disease, considers its presence almost uniform. Aware of what he has written on this subject, I gave directions to the clinical clerk to test the urine daily, which was done during the whole time the patient was in the house. The result was, that on no one single occasion was the urine in the slightest degree albuminous. Even on the day when a slight deposit appeared, which was made up of casts and epithelium of the tubes, the report says, "No albumen in the urine when tested by heat and nitric acid." This coagulability of the urine, as well as various deposits which appear in it on critical days, must be considered as an evidence of the excretion of the morbid products which have circulated in the blood. Hence it is common, not only in scarlatina, but in all inflammatory affections, as well as fevers. This point you must have seen me very observant of in watching for the resolution of inflammations and fever at the bedside. For the theory of its occurrence, I must refer you to my "Treatise on Inflammation," p. 65, and to a former lecture on "Exudation,"—"Monthly Journal," Feb. 1850, p. 161. It sometimes happens, however, that the critical discharge is comparatively slight, and that the organic elements are not dissolved so as to constitute fluid albumen. This appears to have occurred in the present case, for whilst morphological evidence of the crisis existed in the urine, in the form of cells and casts, it is distinctly stated no albumen could be detected by heat and nitric acid. Yet the girl's convalescence commenced from that day.

### *Erysipelas.*

CASE I.—Marian Smales, æt. 28, admitted January 8th. She stated that, on the morning of the 6th, she was quite well, but that, after being out for some time, she felt a burning pain in her left cheek, and observed a red spot upon it. This redness gradually extended down towards the neck, and was accompanied with considerable swelling. She applied a mustard poultice to her cheek, which relieved the pain somewhat at first, but afterwards caused a great aggravation of it. On admission, besides the local pain, she complained of



great thirst and of a bad taste in her mouth. The tongue was moist; bowels regular; pulse 66, full and strong. The cheek was ordered to be fomented with a lotion of lead and opium. January 11.—Swelling and redness are much less, as is also the pain. January 17.—Redness of the skin completely disappeared. Complains only of a slight soreness in the throat. Dismissed cured.

CASE II.—James Maclaren, æt. 59, a porter, of intemperate habits, admitted November 16th. Eight days ago was seized with rigors, followed by intense febrile symptoms, which prevented sleep. On the 13th, he experienced pain in the left side of his nose, accompanied by redness of the integuments, which rapidly spread over the cheek, eye, and brow of the same side. On the following morning the redness appeared on the right cheek, and in the evening had covered the whole face. On admission there is great thirst; loss of appetite; furred tongue; hot skin; full and burning pulse, 100 in the minute; great headache, with drowsiness; tingling pain in the face, which is of a deep red colour, in some places approaching purple. The blush extends over the forehead and anterior part of the scalp, and pits on pressure. Two bullæ have broken, and recently formed scabs on the right side of nose. *Ordered an antimonial saline mixture, and the face to be dusted with flour.* November 17th.—Last night there was low muttering delirium, and this morning vomiting. In the evening, pulse of the same frequency, but more soft. *To omit the mixture.* November 18th.—Redness more extended over the scalp, and fresh bullæ have appeared on the forehead. Pulse 80, soft; constipation. *To have ℥iij. of brandy daily, and to take at present half an ounce of castor-oil.* November 19th.—To-day much better. Pulse 80, of good strength; swelling of eyes diminished; redness fading; bullæ scabbing. From this time he gradually got well, and was dismissed cured, November 30th.

*Commentary.*—The first of these cases was so mild as, perhaps, to merit the name of erythema. The latter was a very severe one, occurring in a man of intemperate habits, but terminating in convalescence on the twelfth day. In this latter case a study of the symptoms will show we have again, as in scarlatina, all the phenomena of typhus fever; and when erysipelas proves fatal, so in like manner it is by coma and subsequent collapse. Erysipelas, however, is opposed to scarlatina, in being the least infectious of the eruptive fevers, in being the least fatal, and in running a much slower course. In many other respects there is a close analogy between them observable in the kind of fever, the sequelæ, and critical discharge of coagulable urine, as lately pointed out by Dr Alexander Wood and Dr James W. Begbie. The general indications for treatment are the same. The special treatment is directed by means of topical applications to diminish the local inflammation. For this purpose numerous remedies have been tried,—such as dusting the part with flour, lotion of acetate of lead and opium, cerates, oil, etc. etc.,—any of which serve the purpose of cooling the surface, rendering it more soft, and diminishing irritation.

There can be no doubt that erysipelas is occasionally a fatal disease, from the intensity of the fever, and amount of integument involved. It is generally supposed that, when it attacks the face and scalp, it is more dangerous than when a similar amount of surface in any other part is affected. This opinion does not appear to be founded on very exact observation. Even when the scalp is extensively invaded, death from erysipelas is a rare occurrence. On going round the wards of the Hotel Dieu in May 1851 with M. Louis, I saw several severe cases of erysipelas of the scalp, which, I was told, were under no treatment whatever,—because, as M. Louis informed me, according to his experience, erysipelas of the scalp was *never* fatal, unless it occurred in individuals of bad constitutions, or was associated with some complication. I need not say that, without forming any such exclusive opinion as this, it must be very difficult, in a disease that so generally tends to recovery, to judge how far this or that remedy is beneficial. Mr Hamilton Bell has lately recommended fifteen to twenty-five drops of the *Tr. Ferri Murialis* every second hour, as a most



beneficial remedy in erysipelas. But how this medicine is more successful than the spontaneous operation of nature, it must be very difficult to demonstrate.

*Variola.*

Mary Hogan, æt. 7, was admitted December 9th. Never had been vaccinated. Felt slightly indisposed December 4th; and on the following day complained of severe headache, pain in the back, nausea, loss of appetite, and great thirst. These symptoms continued, and on the afternoon of the 7th a bright red blush was observed on the face and chest, gradually spreading over all the body. On the 8th the red blush became covered with numerous minute elevated papulæ; and on the 9th, when admitted, numerous vesicles could be detected on the face, arms, and legs. Tongue furred, but moist. No dysphagia. *Was ordered a purgative of sulphate of magnesia.* December 10th.—The vesicles are numerous and close together on the face, and in some places confluent. Eyelids much swollen and nearly closed. Bowels are open; pulse 140; tongue florid. The hair was cut short, and mercurial ointment, thickened with starch, spread over the face. She was also vaccinated. December 13th.—Pustules fully matured and umbilicated over the trunk and extremities. The mercurial paste forms a thick indurated crust over the face. December 15th.—Many of the pustules all over the body have burst and discharged their contents. No constitutional disturbance. No pain or itching of the face; all swelling of the eyelids disappeared. December 18th.—Pustules have all burst, except a few on the feet. Was dismissed, January 6th, cured. The face scarcely presented any trace of the disease, and afforded a remarkable contrast to those other parts of the skin which had not been covered with the paste.

CASE II.—Michael Hogan, æt. 9, admitted December 10, 1851, a brother of the former case, and also never vaccinated. Felt unwell on the 8th, with shivering, pain in the head, and usual febrile symptoms. On the next day vomited, and then observed an eruption on the skin. On admission, the face, trunk, arms, and legs are spotted with bright papules at considerable distance from each other, and he says the fever has considerably abated. On the 15th the pustules on the face were fully matured, and here and there a few of them were observed to be confluent. On the 18th those on the inferior extremities were in the same condition. Last night he experienced again considerable headache, and to-day the pulse is 120, full; the skin hot, and febrile symptoms well developed. 19th.—Headache violent last night, with great restlessness and insomnia; but to-day these symptoms have abated. From this time convalescence commenced, but he recovered slowly, and was not strong enough to go out until December 19th. A few pits existed on the face, where the pustules had been confluent.

*Commentary.*—The general treatment of small-pox is similar to that of the other eruptive fevers. There is a special treatment, however, applicable to it, which deserves some consideration. On a former occasion I directed your attention to the ectrotic treatment of variola, and since then numerous cases have been admitted into the wards, which have exhibited the good effects of the practice. The two cases you have just had an opportunity of observing, however, especially demonstrate this. Case I. presented the most confluent form of the disease I ever saw. The entire face was so crowded with the papules and minute vesicles of the incipient stage, that there was literally not room to place a pin's head anywhere on the sound skin. It was evident that the whole surface of the face would be one mass of suppuration; and such of you as have had an opportunity of observing such a case of the disease, must be aware of its horrible aspect, the excessive agony produced, the great swelling of the eyelids, the dreadful suppuration and foetor of the discharge, the violent secondary fever, and the frightful cicatrices with which the countenance is afterwards covered. In this case none of these symptoms were present, and there can be no doubt that the ectrotic treatment really checked the progress of suppuration and modified the disease. From the moment the plaster

was applied, all smarting and pain in the face ceased; the eyelids were never swollen; no suppuration occurred; there was no secondary fever; and on the mask leaving the face there was no pitting or suppuration. In other parts of the body the eruption passed through its usual stages, and the girl was dismissed from the house well, thirty days after the first commencement of the eruption. Considering this case was likely to be a very severe one, I felt myself authorised to use every means in my power to check the disease; and as it has been asserted that vaccination, even after the commencement of the eruption, modifies its progress, I caused the girl to be vaccinated on first seeing her. At that time the face, as we have seen, was closely covered with papulæ and vesicles; and I do not think that vaccination alone could have produced the remarkable result we have witnessed. I do not mean to deny altogether the influence of vaccination in such cases, but I have no hesitation in ascribing the beneficial result almost entirely to the ectrotic treatment.

To satisfy yourselves still more, if possible, as to the great advantage of this treatment, the case of the boy (Case II.) may be contrasted with that of the girl (Case I.), who also had never been vaccinated. His was evidently a very mild case, the eruption discreet, and the constitutional disturbance slight. I allowed it to run its natural course, and the result was in every respect different from that in which the plaster had been applied to the face. The secondary fever was tolerably smart, the subsequent prostration proportionally severe; recovery was delayed to the 39th day, and notwithstanding the generally discreet character of the eruption, a few pits existed on the face.

The general subject of small-pox opens up to our consideration a multitude of considerations, of which we may notice three.

1. There can be very little doubt that small-pox is again becoming frequent amongst us, a circumstance which some have attributed to deterioration of the vaccine lymph. That this cause does operate to a certain extent is very probable; but, for my own part, I have been led to the conclusion, that the terror for the disease which formerly prevailed among the public, has, through the protective discovery of Jenner, and the energy with which vaccination was originally pursued, in a great measure declined, and that this is the principal cause. At present, multitudes of the lower orders no longer have their children vaccinated, and hence why our hospitals are so frequently encumbered with cases such as those we have just witnessed. We have no remedy for this but rendering vaccination imperative by penal enactments, as is done in some continental states.

For the mode of vaccination, I must refer you to the account given in systematic works on the practice of medicine. It consists, as you know, of making just a sufficient puncture to penetrate the epidermis of the skin and to enable the vaccine lymph to be applied to the vascular dermis. For doing this surely and rapidly, the little instrument I now show you, invented by Dr Graham Weir, is the best you can employ.—(See "Monthly Journal," July 1847, p. 69.)

2. Sometimes small-pox occurs epidemically in a remarkably benign form. It then presents all the characters described by some authors as varioloid. Occasionally it occurs twice, or becomes what is called recurrent, and it has been known to arise frequently after vaccination. In all these circumstances, when mild it so resembles chicken-pox as not to be distinguished from it. But more than this, it was observed in the epidemic that prevailed in Edinburgh in 1819 and 1820, that small-pox and chicken-pox existed together frequently in individuals inhabiting the same room, and sleeping in the same bed; well authenticated cases occurred of individuals inoculated with small-pox, in whom the eruption assumed the appearance of chicken-pox; and again persons inoculated with chicken-pox had small-pox well characterised. The work of Dr John Thomson, entitled "An Account of the Varioloid Epidemics in Scotland, 1820," contains many facts of this description, which were well known at the time, and an account of numerous experiments

carried on in the Castle garrison of this place, which have never been controverted, and fully establish an essential unity in the nature of the two affections. It is evidently inconsistent to suppose that two distinct contagions should exist at the same time, each of which is protective of the other. Those who admit this doctrine must maintain that whenever the chicken-pox contagion prevailed, the small-pox contagion was excluded, or the reverse; or, on the other hand, they must admit that variola is produced by the same contagion that gives rise to chicken-pox. The work of Dr Thomson furnishes ample proof of the correctness of the latter proposition. Dr Gregory and others who oppose this opinion, do so on the ground of the inoculative stage being shorter; the whole disease less prolonged, and the constitutional symptoms being mild. These circumstances, you will observe, only point to difference of degree and intensity, not of kind. Dr Gregory also alleges that he has seen variola occur after cow-pox, and cow-pox after variola, and therefore they cannot be identical. So far, however, does this appear to me no argument, that, if possible, it confirms Dr Thomson's observations. The variola he speaks of occurring after cow-pox is evidently modified small-pox, and cow-pox may, in the majority of cases, be reproduced at pleasure.

3. Dr Jenner, through life, was of opinion that cow-pox, the grease in horses, swine-pox, and small-pox, were only modifications of each other. He believed that in giving to man cow-pox, he was in reality giving to him small pox in its primitive and mildest form. Whether cow-pox or small-pox is the original form has been disputed. It occurs to me as more probable that cattle caught it from man, rather than man from cattle, an opinion confirmed by the experiments of Mr Ceely of Aylesbury, recorded in the "Transactions of the Provincial Medical and Surgical Association" (vols. 8 and 9). He showed that by operating on the mucous surfaces of the animal, the cow readily receives the poison of human small-pox, which the constitution of the animal converts into the vaccine. I need not enter at length into the discussion which has been raised on this subject. Suffice it to say, that the identity of the two diseases appears to me to be established by the following incontrovertible facts:—

1. The prevalence at the same period of the cow-pox among cattle, and the small-pox among men.

2. The transmission by *contagion* of the small-pox to cattle, and the consequent development of cow-pox in these animals.

3. The transmission by *inoculation* of the small-pox to cattle, and the resulting development of cow-pox in those animals.

4. The transmission by *inoculation* of the cow-pox to man, and the development thereby of a pustule similar in character to the vaccine pock of the cow.

5. The transmission by *inoculation* of the cow-pox to man, and the consequent development of an eruption similar if not identical with small-pox.

All these propositions have been established by numerous facts, which you will find ably stated in the "Report of the Vaccination Section of the Provincial Medical Association." Notwithstanding these, however, I must inform you that Dr Gregory, undoubtedly a great modern authority in eruptive febrile diseases, appears to be fond of numerous divisions, and is still sceptical as to the identity of those diseases.

Part Fifth.

MEDICAL NEWS.

ON THE PRESENCE OF FLUORINE IN THE STEMS OF GRAMINEÆ, EQUISETACEÆ, AND OTHER PLANTS, WITH SOME OBSERVATIONS ON THE SOURCES FROM WHICH VEGETABLES DERIVE THIS ELEMENT. BY GEORGE WILSON, M.D.

The author commenced by stating that the earliest observer of the presence of fluorine in plants was Will of Giessen, who found traces of it in barley, the straw and grain of which were analysed together. The author reported to the Botanical Society, some four years ago, the results of his earlier researches into the distribution of this element throughout the vegetable kingdom, which were not very numerous or very encouraging. One reason of this was the small extent to which fluorine occurs in plants; another, and practically as serious a reason, was the difficulty of separating and recognising fluorine when accompanied by silica. The presence of this body in a plant, besides greatly complicating the investigation, rendered the employment of platina vessels essential, and thus limited the amount of material which could be subjected to examination, besides making it difficult or impossible to observe the progress of analysis.

The author then stated, that, in the course of some recent investigations into the presence of fluorine in siliceous rocks, he had succeeded in devising a process which was also applicable to plants, and could be carried on in the ordinary glass vessels of the laboratory. The process in the case of plants was as follows:—The plant under examination was burned to ashes as completely as possible; the ashes were then mixed in the cold with oil of vitriol, so as to secure the decomposition of the salts of volatile acids present; the mixture was then transferred to a retort, or flask, provided with a bent tube dipping into water, and the liquid raised to the boiling point, when fluorine, if present, was evolved in combination with the silicon of the silica, as the gaseous fluoride of silicon, which dissolved in the water with separation of some gelatinous silica. The resulting solution was neutralised with ammonia, and evaporated to complete dryness, when the whole of the silicon passed into the condition of insoluble silica, and water dissolved the fluoride of ammonium. The solution of this fluoride could then be dried up and moistened with sulphuric acid, when hydrofluoric acid was evolved, which might be made permanently to record its presence by causing it to etch glass in the usual way. The author has in the meanwhile applied this process almost solely to the stems and trunks of plants, especially to those containing silica, reserving for subsequent investigation their other organs, especially their seeds and fruits. The following were the results obtained:—

*Table of Plants examined for Fluorine. The numbers represent grains of ashes, except in the case of Tabasheer and Wood Opal. The blanks imply that the weight was not known:—*

Ashes in grains.	Name of Plant.				
200	<i>Equisetum limosum,</i>	...	...	...	Distinct etching.
	<i>Bambusa arundinaceae,</i>	...	...	...	ditto.
	Charcoal (derived chiefly from oak, and to a smaller extent from birch),	...	...		ditto.
	Coal,	...	...	...	ditto.
	Barley straw,	...	...	...	ditto.
	Hay (ryegrass),	...	...	...	ditto.

Ashes in grains.	Name of Plant.				
35	<i>Equisetum variegatum</i> ,	...	...	...	Faint etching.
19	———— <i>hyemale</i> ,	...	...	...	ditto.
255	———— <i>palustre</i> ,	...	...	...	ditto.
	<i>Dactylis cæspitosa</i> ,	...	...	...	ditto.
99	<i>Elymus arenarius</i> ,	...	...	...	ditto.
495	<i>Saccharum officinarum</i> ,	...	...	...	ditto.
1040	African Teak,	...	...	...	ditto.
	<i>Smilax latifolia</i> ,	...	...	...	No etching.
	<i>Rosmarinus officinalis</i> ,	...	...	...	ditto.
235	<i>Bambusa Nepalensis</i> ,	...	...	...	ditto.
	<i>Polypodium vulgare</i> ,	...	...	...	ditto.
537	Tree Fern,	...	...	...	ditto.
24	<i>Phalaris arundinacea</i> ,	...	...	...	ditto.
240	Malacca Cane,	...	...	...	ditto.
50	Cocoa nut Shell,	...	...	...	ditto.
127	<i>Tectona grandis</i> ,	...	...	...	ditto.
80	Tabasheer,	...	...	...	ditto.
1680	Wood Opal,	...	...	...	ditto.

On this table the author remarked, that the siliceous stems which he had found to abound most in fluorine, were exactly those which contained most silica. In particular, deep etchings were procured from the equisetaceæ and from the gramineæ, especially the common bamboo. The last was known to contain silica in such abundance that it collected within the joints in white masses, nearly pure, and had long, under the name of tabasheer, been an object of interest to natural philosophers. The horse-tails were scarcely less remarkable for the amount of silica contained in their stems, which had led to the employment of one of them (*Equisetum hyemale*) in polishing wood and metals. The African teak, which, like the bamboo, is known sometimes to secrete silica, was also found to contain fluorine, though much less largely than the plants named; whilst the strongly siliceous stems of barley and ryegrass also yielded the element in marked quantity. The sugar-cane, however, gave less striking results than might have been expected, and the same remark applied to the Malacca-cane. Two specimens of silicified wood and one of tabasheer gave no evidence of the presence of fluorine. So far, however, as the plants named in the preceding table are concerned, the author does not wish it to be inferred from the negative results which are detailed, that the plants in question are totally devoid of fluorine. With larger quantities of their ashes, positive results would, in all probability, be obtained.

The author's general conclusions were as follows:—1st, that fluorine occurs in a large number of plants; 2d, that it occurs in marked quantity in the siliceous stems of the gramineæ and equisetaceæ; 3d, that the quantity present is in all cases very small; for although exact quantitative results were not obtained, it is well known that a fraction of a grain of fluoride will yield with oil of vitriol a quantity of hydrofluoric acid sufficient to etch glass deeply, so that the proportion of fluorine present, even in the plant-ashes which contain it most abundantly, does not probably amount to more than a fraction per cent. of their weight. The proportion of fluorine appears to be variable, for different specimens of the same plant did not yield concordant results.

In this, however, there is nothing anomalous, for some bamboos yield *tabasheer* largely, whilst others are found to contain none. It seems not unlikely that soluble fluorides ascending the siliceous stem of a plant, on their way to the seeds or fruits in which they finally accumulate, may be arrested by the silica, and converted into insoluble fluosilicates (fluorides of silicon and of a metal); and a bamboo, for example, secreting tabasheer, may effect this change where one less rich in silica cannot determine it. The slow or quick drying of a stem may also affect the fixation of fluorides in the stems or trunks of plants.

The sources of the fluorine found in plants may be regarded as pre-eminently two,—1st, simple fluorides, such as that of calcium, which are soluble in water, and through this medium are carried into the tissues of plants; and 2d, compounds of fluorides with other salts, of which the most important is probably the combination of phosphate of lime with fluoride of calcium. This occurs in the mineral kingdom in apatite and phosphorite, and in the animal kingdom in bones, shells, and corals, as well as in blood, milk, and other fluids.

A recent discovery of the author's, communicated to the Royal Society of Edinburgh, has shown that fluorides are much more widely distributed than is generally imagined, and that the trap rocks near Edinburgh, and in the neighbourhood of the Clyde, as well as the granites of Aberdeenshire, and the ashes of coal, contain fluorides, so that the soils resulting from the disintegration of those rocks cannot fail to possess fluorides also. All plants, accordingly, may be expected to exhibit evidence of their presence in the following portions of their tissues or fluids:—

1. In the ascending sap, simple fluorides.
2. In the descending sap, in association with the albuminous vegetable principles, and in the seeds or fruits, in a similar state of association, fluorides along with phosphates.
3. In the stems, especially when siliceous and hardened, fluorides in combination with silica. The investigation is still in progress.—*Proceedings of the Botanical Society of Edinburgh*, July 8, 1852, and in *Chemical Gazette*, August 16, 1852.

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ALTERED REGULATIONS OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND—  
IMPORTANT FOR STUDENTS.

Hitherto the Royal College of Surgeons of England, while it has required attendance on three distinct courses of physiology from medical students in London, in addition to three courses of anatomy and of dissections, has not required attendance on a single course of physiology from students in Scotland. There the College has been satisfied with the certificates of the professors of anatomy, who, although their commissions only authorise them to teach one branch of medical education, have not scrupled to teach, and certify that they teach, two; and these certificates have been received by the London boards as a matter of course. This laxity has led to great abuse. It has caused the lecturers on anatomy, in order to meet the regulations of the London boards, to give two distinct courses of lectures every session, in one of which they have taught physiology; so that the medical student has been obliged to attend his anatomical teacher six separate times. But there is a professor of the institutes of medicine in the Scotch schools, whose course of lectures is analogous to that given by Professors Sharpey, Todd and Bowman, Paget, etc.; and as attendance on his lectures is required by the University, and by the Edinburgh College of Surgeons, the Scotch student, in point of fact, has been obliged to attend seven six months' courses, in order to be taught anatomy and physiology. We are happy to announce that this state of things is at length put an end to. In future the Scotch student, who becomes a candidate for the London diploma, will be required to attend two courses of anatomy, and one course of the institutes of medicine. That is, three systematic courses instead of seven, in addition to dissections and demonstrations. Special schedules have been issued by the London College for the Scotch schools, which the student should be careful to obtain, in which two certificates of courses of anatomy are to be filled up and signed by the professor of anatomy, and one course of the institutes of medicine by the professor of the institutes.



## CORRESPONDENCE.

## POWERS OF THE PROCURATOR-FISCAL.

*To the Editor of the Edinburgh "Monthly Journal."*

Sir,—I beg you will have the goodness to let the enclosed letter appear in the next Number of your Journal.—I am, etc.

Leith, September 1852.

THOS. WILLIAMSON, M.D., Edin.,  
L.R.C.S.E., etc. etc.

*To the Secretary of State for the Home Department.*

"Leith, 10th August 1852.

"Sir,—I have the honour of addressing you in regard to a matter which, while it affects myself personally, has an important bearing on the general interest of the medical profession.

"The sense of individual wrong might hardly have warranted such a liberty, but the persuasion that my silence, under the circumstances about to be mentioned, would tend to confirm a system well calculated to disturb the peace, and degrade the character, of medical men, leads me to take this step without any hesitation.

"I am a medical practitioner in Leith. On the 28th ultimo, a female child of respectable parents, who have been my patients for years past, was criminally assaulted and extensively injured.

"Having been sent for to see her, and finding the case so serious, I requested the co-operation of Dr MacLagan of Edinburgh, and with him again examined the child, with reference to future proceedings for punishing the culprit, who had been apprehended. I also gave information of what had happened to the proper authorities.

"Next day I learned that the Procurator-Fiscal had requested Dr MacLagan, together with a medical practitioner in Leith, to make a report upon the case, thus completely excluding me from any concern in it beyond the medical treatment. I remonstrated with the Procurator-Fiscal and Sheriff, but received no reply.

"I then addressed a statement to the Lord Advocate, who, without any further communication with me, has decided,—'That no blame whatever attaches either to the Procurator-Fiscal, superintendent of Leith police, or Dr Paterson; and that there are no good grounds for a charge of irregularity or want of courtesy against any official person.'

"The effect of this decision is to sanction a system, so far as I know, altogether unprecedented in this or any other country, and which is in the highest degree calculated to disturb the peace of the medical profession, degrade the character of its members, and defeat the ends of justice.

"It is the undoubted right of the Crown to associate one or any number of scientific or practical men, with the original medical attendant of the patient whose case becomes the subject of judicial inquiry. But it is a no less unquestionable rule, that the original attendant is always to be recognised throughout the investigation, unless he is an unqualified or notoriously disreputable practitioner.

"I, therefore, venture to hope that, in the circumstances which have now been stated, I shall be pardoned for addressing you, sir, instead of appealing to the sympathies of my professional brethren; and I have only further to entreat, that, before pronouncing a decision, you will ascertain the sentiments of the medical profession with regard to the matter in question.

"I beg to enclose a copy of the Lord Advocate's deliverance,—and have the honour, etc.

"THOS WILLIAMSON, M.D., Edin., etc etc."

*Reply of Secretary of State to Dr Williamson.*

“Whitehall, 4th September 1852.

“Sir,—I am directed by Mr Secretary Walpole to inform you, that he has made inquiry into the subject of your letter of the 16th ultimo, and that he finds that your complaint is quite without foundation.—I am, etc.

“H. WADDINGTON.”

[The question here at issue seems to us of the greatest importance to the medical profession in Scotland. If every procurator-fiscal throughout the country should be admitted to have the power of displacing the original medical attendant, in cases of judicial investigation, at the mere suggestion of his own personal feeling, the character of no practitioner will be safe from injury, and the ends of justice may be thwarted through absence of the principal witness.]

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ON THE BEER PUFF.

*To the Editor of the “Monthly Journal of Medical Science.”*

Sir,—I had much pleasure in reading the amusing dialogue, in your August Number, on the late attacks on pale ale, and the means I had adopted of self-defence against such attacks. So far from feeling offended at the strictures you therein pass upon the course adopted, I, on the contrary, recognise in them a very proper susceptibility of the dignity of the medical profession, and am grateful that they give occasion for what I value especially—a high testimonial in favour of our ale—spontaneous, unsolicited, and, I might almost say, unintentional. This, sir, I have instructed my agents to insert in their advertisements throughout the public press; and I trust the Proprietors of the Edinburgh “Monthly Journal” will find no objections to its appearance in their advertising columns.

Any attempt on my part towards a defence of those eminent medical gentlemen who have honoured me with their testimonials—against the charge of interested motives implied in your columns, would be at once impertinent and unnecessary. Such a thought never entered my head; and you, sir, have yourself suggested their full acquittal. But I find in your Number for the present month, a letter from Dr Glover, of Newcastle, which calls for some notice on my part.

The circumstance that gave rise to my application to Dr Glover, was his writing a letter to the Editor of the “Lancet,” in which he seemed, unnecessarily, to add the weight of his professional opinion to the prejudice against bitter beer, so unexpectedly raised in the public mind, by the suggestion of the use of strychnine in its manufacture. Many distinguished members of the medical profession pronounced such an adulteration impossible. Dr Glover, however, volunteered his opinion, that it was not only possible, but probable; and the Editor of the “Lancet” had given insertion to this letter, though, certainly, with a strong remark of his own on the absurdity of the supposition. Copies of these I subjoin.

*From the “Lancet,” April 3, 1852.*

“ALLEGED IMPORTATION OF STRYCHNINE.

*“To the Editor.*

“Sir,—I beg to state that I have found by experiment that one grain of strychnine will give a strong bitter taste to three or four bottles of ale, and will be efficient to even a greater extent. The wholesale price of a grain of strychnine is about a halfpenny. Probably this poison can be manufactured even more cheaply in France. There is, therefore, every inducement for an unprincipled brewer to adulterate his ale with this substance. Of the injurious effects of the long-continued use of such a beverage, there cannot, I think, be a doubt. There is only one remedy for such adulterations,—viz., for the Legislature to repeal the

absurd protection which hop enjoys, and to levy no tax on hops, and to allow any *wholesome* bitter to be employed. Then I have no doubt that many vegetable bitters would be openly, cheaply, and beneficially employed.—Yours, etc.

“ R. M. GLOVER, M.D.

“ Newcastle-upon-Tyne, March 1852.”

“ \* \* Dr Glover and the public may feel perfectly confident that the odious allegation made by the French writer, that strychnine is commonly used in this country in the manufacture of bitter beer, is as foul a calumny as was ever invented. It is just possible that a few unprincipled and needy speculators may have resorted to the use of that poisonous drug; but that houses of established reputation and wealth should have employed it in their manufactures, is a supposition far too preposterous to be entertained.—ED. L.”

Upon this, courting inquiry as I wish to do, I wrote to Dr Glover, offering him an inspection of our brewery and stores, in any way he thought proper; and also (adopting a suggestion of the late Lord Tenterden, in the case of the *Burton Brewers v. the Society for the Diffusion of Knowledge*, in 1830), I put my head brewer in communication with him. The answer I received from Dr Glover was, I must say, not quite so satisfactory as I could have wished, inasmuch as I considered that, as he had lent himself to fan the prejudice, it was but fair he should assist in putting the matter right as publicly as he had aided the mischief.

Dr Glover appears to be dissatisfied with my publication of a portion of his letter, of which, by-the-bye, I have made no exclusive use, giving it as a testimony in favour of all pale ales, as well as my own. But Dr Glover, though expressing to me a modification—which rendered it harmless—appeared unwilling to give the same publicity to the explanation as he had volunteered to the charge. I could, therefore, regard him only in the light of a public accuser, to be defeated by the weapons with which he had himself supplied me; and the public, I thought, might be the better convinced by seeing how the doctors, whose curious learning was frightening them from drinking pale ale, could find no objection to enjoying it themselves.

If, however, there were any impropriety in the publication of Dr Glover's good opinion in the first instance, it is now quite removed, by the permission he has now given me to that effect, with the explanation, that he had not originally meant it for publication, and that the sentence quoted is not such as he would have written in a letter intended for publicity. That you, sir, and the public may form an opinion how far I have compromised Dr Glover, I beg (with his permission) to enclose you the letter, the publication of which, *in extenso*, will, I trust, do full justice both to Dr Glover and myself.

*Dr Glover to Mr Allsopp.*

“ Newcastle-on-Tyne, April 11.

“ Sir,—It was not my intention, in writing the hasty note to the ‘Lancet,’ to cast any reflections upon, or to implicate in any way, respectable brewers of pale ale.

“ When I first saw the statement about the alleged use of strychnine in bittering ale, I looked upon the assertion as incredible, both on account of the price of the drug, and the symptoms it would create; but on experiment, I found that strychnine possesses such wonderful bitterness, that it might perhaps be used as *an adjuvant*, at least by *unprincipled persons*. In short, my object was simply to show that the thing was not altogether so impossible as it appeared at first sight to be.

“ My own opinion is, that hops should not enjoy the exclusive privilege of being used for bittering beer; but I do not pretend to discuss the point with practical men. I know there are bitters which might be used beneficially in a medical point of view.

"With regard to analysing your beer, my time is taken up, so far as analysing and chemistry are concerned, with two kinds of inquiries,—1st, Those which are purely scientific; and, 2d, Those which are profitable. If you wish me, in the latter capacity, to analyse and report on your beer, I of course can have no objection.

"I have to prepare for an absence of three or four days to-morrow, and so beg you to excuse me replying to the letter of Mr Bottinger, for which I am much obliged.—Yours, etc.

(Signed) "R. M. GLOVER.

"H. Allsopp, Esq.

"P.S.—I presume you will hardly expect me to write to the 'Lancet.' However, I shall be home on Thursday evening, and most assuredly I have no desire to say anything which could weaken the confidence of the public in your beer. But that I am not now in the habit of drinking bitter beer, I should be glad to show my confidence by drinking plenty of it."

I consider any further observation unnecessary, save that I inserted Dr Glover's good-natured remark on my bitter beer as an "incidental testimonial,"—no more. I never called it "a certificate;" nor did I apply to him, or any other medical gentleman, for one. I am not responsible that such a construction has been placed upon the off-hand expressions of good opinion which have been sent to me from all quarters; and I have only to express my regret to Dr Glover, that he has been exposed to an imputation so unfounded.—I am, etc.

HENRY ALLSOPP.

Brewery, Burton-on-Trent, September 22, 1852.

[If Mr Allsopp has now done justice to Dr Glover, we at least have done no injustice to Mr Allsopp. The remarks in this Journal, addressed to the medical profession, which have called forth all these reclamations, were solely intended to expose and check a variety of the puff-medical to which it appeared to us that some of our brethren were apt to lend their sanction with inconsiderate and unbecoming facility. We are glad to find that our remonstrance is likely to prove not altogether ineffectual. In mercy to our readers we must, however, announce, that no more letters on the subject of the beer-puff can be inserted except in our advertising columns.—Ed.]

#### PUBLICATIONS RECEIVED.

A Commentary of Medical and Moral Life: or, Mind and the Emotions considered in relation to Health, Disease, and Religion. By William Cooke, M.D., M.R.C.S., etc. London. Longmans. 1852.

Medical Report of the Female Side of Colney Hatch Lunatic Asylum. By James George Davey, M.D. London, 1852.

Leisure Moments. A Monthly Serial. September 1852.

A Letter to Dr Lyon Playfair on the recent Analysis of Buxton Tepid Water. By William Henry Robertson, M.D. London. Bradbury and Evans. 1852.

History of Small-Pox. By Henry George, Surgeon. London. Churchill. 1852.

## Part First.

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### ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Incarcerated Femoral Hernia.* By KELBURNE KING, M.D., Edin.

ON the 24th February 1852, I was asked to meet Dr Sandwith, of Hull, in consultation regarding A. T., a young woman from Lincolnshire, aged 27, in consequence of a tumour of the left groin, from which she had long suffered, and of which she gave the following history:—

Six years ago, when raising in her arms the body of her mistress, who had just died, she felt “a crack in her inside,” and immediately became sick and faint. On examining her body shortly afterwards, she found in her left groin a tumour about the size of a nut, which was the seat of considerable pain, and which gradually increased in size, becoming, in the course of a few weeks, as large as an egg. She then got alarmed about it, and consulted a physician, who advised her to wear a truss, which she did for some time; but the tumour could not be reduced, and the presence of the instrument aggravated the local uneasiness so much, that she soon ceased to apply it. From the time it was first noticed up to the present moment, it has never been reduced: nor has it even been sensibly diminished, though it has sometimes remained stationary for a considerable time. She has all along been subject to occasional severe attacks of pain and sickness, and each paroxysm has been attended by an increase in the size of the external swelling. For the last two years especially, she has suffered almost constantly from extreme irritability of the stomach,—her food being hardly ever retained, and a feeling of sickness being caused by the slightest exertion, even when the stomach has been empty. She has also complained much of a continual dragging sensation about that organ, accompanied by a feeling of cramp, and what she describes as a bearing down pain. Of late she has been able to obtain sleep only when lying on her face; and has spent the greater part of the night resting on her hands and knees upon the floor,—a position which she describes as affording her most relief, and to which she has had continually to resort during the paroxysms of pain. She has become weak and low-spirited, being;

as she says, worn out by want of rest and nourishment; and having long tried all sorts of palliative treatment without experiencing any permanent benefit, she is most anxious to submit to any measures which may be thought likely to afford a chance of restoration to health.

Such was her statement, and her appearance corresponded with the history she gave. She was much emaciated, her complexion was sallow and unhealthy, and her countenance had a care-worn, anxious expression, as of one who had long laboured under a debilitating complaint. Her gait was most peculiar; she could scarcely stand erect; and when she walked she had a great and remarkable stoop—her head and the upper part of the trunk being drawn downwards and forwards to the left side, in the direction of the tumour. This was found, on examination, to be about twice the size of a man's closed fist. It occupied the whole of the upper and inner part of the left thigh, and extended upwards for some inches above Poupart's ligament. It was of an oblong, rather spheroidal form, its greatest diameter being transversely from within outwards. Its base was flattened, and seemed to be superficial to the femoral and abdominal aponeurosis. It projected forwards some three or four inches, being covered only by the skin, to which it was not adherent. It could be readily raised up from the surrounding, or rather subjacent, tissues; and, indeed, on a cursory examination, might have been supposed to have been altogether detached from them; but more careful manipulations proved it to have deep-seated connections, considerably narrower, however, than its base. It had a hard, firm, lobulated feel, except a space near its centre about an inch in diameter, which presented distinct fluctuation, characteristic of liquid contents; and was dull on percussion. Coughing produced no perceptible impulse; handling it caused considerable pain.

Several circumstances connected with the history and physical characteristics of this tumour are deserving of notice. The account given of its first appearance seemed to prove, that it consisted primarily of a small portion of the contents of the abdominal cavity, the increasing displacement of which produced its gradual enlargement; but, on the one hand, the want of impulse on violent expiratory efforts, as coughing, the hardness and general form and appearance of the tumour, forbade the idea of its containing intestine—of its being an enterocele; on the other hand, it had not the doughy, soft consistence, of an ordinary omental hernia; and the fact of its never having been reducible, its slow and painful growth, its tenderness to the touch, and the severe constitutional symptoms which attended it, caused a suspicion in the mind of an experienced practitioner who saw it, that it might be a malignant tumour. Still, taking everything into consideration, I had no difficulty in coming to the conclusion, that it consisted of a portion of protruded omentum, which had at various times been the seat of



inflammation, terminating in the effusion of lymph, and thus leading to great solidification of the mass, and the formation of adhesions to the surrounding tissues. I may add, that the descent had evidently taken place through the crural canal.

The nature of a disease being ascertained, there is seldom much difficulty in determining the treatment. This, however, seemed to me rather a peculiar case. It is common enough to find omental hernia both by itself, and as a complication of enterocele; but cases such as the one described are rare: nor in the works to which I have referred have I been able to discover one precisely analogous. Its peculiarity consisted in the long continuance, and the increasing severity, of the symptoms, which, though not immediately threatening a fatal result, were extremely painful and embarrassing. Inflammation had, no doubt, been frequently set up in the protruded mass, and was now probably always present in a chronic form; but it seemed not to extend beyond it, nor had it ever acquired so acute a character as to threaten to terminate in gangrene. For this reason, I have spoken of it as a case of incarceration, understanding by that term a condition in which reduction is impracticable, and inflammation exists, but not in such a form as to lead to the suspicion of incipient mortification—a condition intermediate between that of the simply irreducible and the strangulated hernia. This is not a state in which operative interference is usually called for; but in this case leeches, and other palliative remedies, had been often tried, and failed to give even temporary relief; and though there might be no immediate danger to life, the patient was in a very miserable condition. Bent almost double, she had latterly become incapable of the slightest exertion, and had experienced no cessation from pain and sickness, except when lying on her face, or resting on her knees and elbows. Nor was her condition altogether exempt from danger. Strangulation might have taken place at any moment. Deaths from this cause occurring in omental hernia have been frequently recorded. In the "Lancet" for November 14th, 1846, an account is given of "a female, aged 84, who had suffered from symptoms of strangulated femoral hernia for ten days before death, the rupture having existed a few months; the vomiting was stercoraceous on the fourth day, and she died from gradual exhaustion, the friends of the patient having resolutely opposed the operation. In the inspection, twenty-four hours after death, the convex portion of the stomach was seen within half an inch of the pubis, being dragged down by the omentum, which, in passing over the small intestines to enter the hernial sac, appeared slightly to constrict a portion of the ileum. The hernial sac was filled with omentum, so begirt by the neck of the former, that it could not be returned; no trace of inflammation in the peritoneal cavity." "Mr Crisp was inclined to believe that the altered position of the stomach was the chief cause of death." This case I have quoted, as it shows that death

may take place with all the symptoms of strangulation, although no part of the intestinal canal be directly involved in the hernia, and though there be no inflammation of the general peritoneal cavity. Taking all the circumstances together—the actual condition—the prospective danger—and the wishes of my patient, I came to the conclusion, that any operation would be justifiable which would afford relief without involving too great a risk of life.

It was quite evident that the first step, and indeed the indispensable condition, without which nothing could be done, must consist in getting quit of the tumour. While it remained, no pressure could be brought to bear on the crural canal, so as to prevent further protrusion. Nor was this a case in which the taxis offered any probability of affording relief. The length of time it had existed, without being reduced, rendered it certain that it must have formed extensive connections, independently of which its size and firmness rendered it impossible for it to be returned by the crural opening; and if this had been practicable, it would not have been prudent to return into the general peritoneal cavity a great tangled mass of inflamed omentum. Failing milder measures, the only mode of accomplishing the object in view seemed to be, to cut down on the tumour, open its sac, follow it up to the superior opening of the crural canal, and then remove it.

Such a proceeding of course was not without a certain amount of danger. In the first place, it involved incision of the peritoneum, which, at all times hazardous, is by some supposed to be peculiarly so under these or similar circumstances. Mr Key makes the following remark on this subject:—"When, from the nature of the symptoms, the case appears to be merely an omental hernia, the operation must not be hastily proposed; for it is not easy to distinguish between inflammation of omentum, which has been irreducible, and strangulation; for the inflamed state of the omentum, without strangulation, the operation will afford no relief: on the contrary, it will aggravate the inflammation. The result of operations on omental hernia, which have been attended by acute symptoms, as great tenderness of the part, continued sickness, tense and tender belly, has been such as to induce me to try every means of allaying the inflammatory action before resorting to the operation. The operation in the cases which have come under my notice has not suspended the symptoms as it generally does in enterocele; but the patient has sunk within a few hours from the effects of the inflammation." South, in quoting this, speaks of it as "a very important remark," "which, however," he adds, "I do not remember to have observed." In the second place, something more than the simple incision for the relief of stricture was here contemplated,—the omentum was to be removed. This may be accomplished in two ways—by incision or by ligature, both of which have been frequently resorted to in cases where omental has complicated in-

testinal hernia; and in the absence of any case exactly similar, I considered that the remarks made in connection with such cases were sufficiently applicable. Both methods have their advocates. Chelius says, "If the omentum be converted into a tangled lump, it must not be returned into the belly." The ligature of the omentum he forbids, on the ground, that it "causes a new strangulation;" and adds, that "the omentum cut off or without tying may be returned into the belly without injury; but if the vessels of the cut edge bleed, they must be tied singly," etc. In the article "Hernia" in the "Dictionary of Practical Medicine and Surgery," Paris 1833, page 563, occur the following remarks:—"Le procédé le plus rationnel consiste, à déplier, à étendre, quand cela est possible, l'épiploon près de l'anneau, et à le couper avec des ciseaux peu à peu, et d'un bord à l'autre, en liant les vaisseaux, à mesure qu'on les divise—c'est ce procédé que je mets en usage; je l'ai employé souvent;—et je n'ai jamais vu qu'il soit résulté aucun accident," etc. Mr Key also advises that "the omentum should be unfolded before it be divided by the knife, otherwise the cutting off the omentum in a mass prevents all the vessels being seen, and when returned into the abdomen they bleed profusely." He relates a case in which he cut off "a large portion of omentum with one stroke of the knife, securing the bleeding arteries before returning it to the mouth of the sac. In four hours after the operation, blood of an arterial character began to ooze from the sac." He used pressure and cold water to no purpose; "her pulse began to falter, and her face was bedewed with a cold perspiration." He opened the sac, removed the coagulum, and was proceeding to look for the bleeding vessels, when he fortunately observed the hemorrhage had suddenly ceased. I have quoted this passage, because it forcibly illustrates a danger peculiar to the operation by incision, and to obviate which the use of the ligature, previous to cutting off the omentum, has been resorted to by surgeons from the time of Galen downwards. In a note to the remarks of Chelius before quoted, his English translator, South, says, "I have tied the omentum, and cut off the part below the ligature, several times, without any of the results commonly, and as by Chelius, assigned to this practice." Scarpa recommends the ligature, but advises that it be tightened by degrees; Hey advocates the same principle, but with the proviso, that the thread be not tightened until the inflammatory action has been subdued—modifications which would not, in the present day, find many advocates. All these authors represent the removal of portions of the omentum as sufficiently safe, if done in the way they recommend; and I did not see that greater danger would attend it when done in such a case as I have related, than when it formed only one step in a more hazardous operation. In spite of the absence of impulse on coughing, I thought it not impossible that a knuckle of intestine might be contained in the tumour; but, if so, I did not see that it would interfere with the main object,—viz., the removal

of the obstacle to the use of mechanical support to the weakened abdominal walls. I thought that this, and other circumstances which could be accurately ascertained only during the course of the operation, would determine which of the methods—incision or ligature—would be most suitable in this case.

These opinions were coincided in by Dr Sandwith, and, assisted by him and Mr Shearwood of Barrow, on the 4th of March I performed the following operation:—The patient having been submitted to the influence of chloroform, an incision, about eight inches in length, was made in the direction of Poupart's ligament, over the whole length of the tumour. Careful dissection was made in the direction of the fluctuating spot, the sac was opened, and about an ounce of brownish serous fluid escaped. A great "tangled" mass of omentum was then brought into view. The sac was found to have contracted adhesions to the crural canal and cribriform fascia, and likewise to its contents; some of these, on being divided, bled very freely. At length, however, the contents of the sac were isolated, and were found to be attached by a very narrow neck to the interior of the abdomen. They were entirely omental, and presented the ordinary characters of this structure, except that they were thickened and stiffened, so to speak, by the effusion of lymph. In some parts they presented a reddish look, but generally they were white and glazed. They consisted of two separate portions of omentum, of about equal size. Some large vessels could be felt pulsating in them. The neck of the tumour seemed to be adherent to the upper opening of the crural canal. Under these circumstances, and considering the embarrassing effects of hemorrhage occurring at the bottom of so deep a wound, and the approach which the peritoneum seemed to have made to ordinary cellular tissue, we agreed that the treatment by ligature was the most suitable to the case. Accordingly, an armed needle was passed through the neck of each of the portions of omentum, and the loop being divided, a double ligature was tied round each. The parts beyond were then cut off, and the ligatures, four in all, allowed to hang out of the wound, which was brought together by a few interrupted sutures. A few strips of adhesive plaster, dry lint, and a bandage, completed the dressing. No constitutional irritation of any sort supervened. She was kept in the recumbent position till the 30th of March, when the ligatures having come away, and the wound healed, without any re-appearance of the tumour, she was suffered to go home, with the advice that, for some time, as a matter of precaution, she should wear a truss.

On the 20th of April she called on me to say, that having ceased to apply the truss, after lifting a heavy weight, she had observed a return of the tumour. On examination, I found a small reducible hernia, and explained that she must now constantly wear a well-fitting truss.

On the 13th July I again saw her. She was looking well and

healthy, and said that she had never felt better. The truss gave her no inconvenience, and prevented the descent of the hernia. I remarked that there was still a slight peculiarity in her gait, which, however, she assured me was only the result of long habit.

I have dwelt so long on the principal features of this case that I have no remark to make, except that the result appears to justify the treatment. There was not the least constitutional disturbance either as an immediate or secondary result of the operation. The peritoneum, indeed, seemed to have lost much of its peculiar character, and to have assimilated to a certain extent to the cellular tissue, with which it had contracted adhesions—a circumstance which encouraged me to resort to the application of ligature to the neck of the tumour, in place of trusting to deligation of the wounded vessels singly. Some of the adhesions which were divided were liberally supplied with blood, and some of them presented arteries sufficiently large to require ligature. The consideration of the probability of still larger vessels being found in the omentum itself, of the difficulty of tying them in so deep a wound, and of the possibility of the divided edges being retracted into the general peritoneal cavity, and there giving rise to almost uncontrollable hemorrhage, as in Mr Key's case, in conjunction with the metamorphosed character of the protruded peritoneum, induced me to apply ligatures to the neck of the tumour, though I should prefer, under many other circumstances, to avoid the additional risk of inflammatory action spreading upwards, which this proceeding necessarily entails. It is to be much regretted that the use of the truss was not more diligently and longer persevered in after the operation, as there is every reason to suppose that adhesions had formed which, if not too soon exposed to the pressure of the abdominal contents, would have resulted in the closing of the crural canal, and thus have led to a radical cure of the hernia.

My object in bringing this case before the profession, is not with the view of recommending such an operation in ordinary cases of irreducible omental hernia, but to show that relief may in this way be obtained, when symptoms of great severity occur. It should not, however, be resorted to until milder measures have been fairly and perseveringly tried; and the circumstances of the case urgently demand interference.

12, North Street, Charlotte Street, Hull,  
27th September 1852.

ARTICLE II.—*Remarks on the Yellow Fever which appeared of late years on the Coast of Brazil.* By WILLIAM M'KINLAY, M.D., late Surgeon of H.M.S. "Cormorant," employed on that Coast.—(*Concluded from p. 352.*)

It has been so often observed, with reference to our men-of-war in the West Indies, that, *after* a severe visitation of yellow fever, the commanding officers become very active; they clear out their ships, clean, fumigate, ventilate, and whitewash the holds, well, etc.; and this proceeding has been so often followed by a fresh outbreak of the disease, that many acute observers looked upon them as standing to each other in the relation of cause and effect; and, in regard to these six cases of fever which occurred in the "Cormorant" at Monte Video, I must observe that, from the 2d April onwards, more than the usual attention had been paid to keep the ship clean, more particularly in a part of the ship, the arrangements of which I had been previously unacquainted with—the bilges under the engines and boilers; that a collection of coal dust, ashes, the droppings of oil and grease from the engines, mixed with bilge water, was found here, which had evidently remained undisturbed for a long time, and which, on being stirred up preparatory to removal, gave out a mixture of hydrosulphuric acid and other gases, very offensive to the nostrils. This smell adhered to the place so much, that it required the frequent repetition of washing, drying, and application of the chloride of zinc solution of Sir W. Burnett, before it would remain any length of time free from the offensive odour. But giving this place and smell credit for being capable of doing any amount of mischief as a cause of disease, the stokers were more exposed to this source than any others in the ship, and none of them were attacked by the fever after this time. There was unrestricted intercourse between the "Cormorant" and Monte Video after the 16th April until the 3d of May. The men went on shore on leave, and some of the officers, including the commander, who was then only convalescing from fever, lived on shore in the town during the greater part of this period. The same freedom of communication was held with H.M. steamer "Harpy," to which vessel and her officers we had been indebted for many acts of kindness during our period of quarantine; and in no one instance was this communication suspected even of causing the disease. We have no reason to believe that the disease would have extended at Monte Video, even if no restriction had been placed on our motions from the time of our first arrival there. I feel convinced it would not; but it would be too much to expect such liberality of sentiment on this subject on the part of the authorities of Monte Video for some time to come. Had the disease made its appearance in or at that place soon after our arrival, we should no doubt have had the credit of having introduced it.

Dr Wilson, in his admirable and concise "Reports on the Health



of the Navy," says :—" Another circumstance ought to be stated, not for its rarity, but for its frequency and its importance in considering the cause of this disease, more especially in ships. It is this : almost every person who joined the 'Vestal' during the prevalence of fever was affected by it, but no person leaving her under the disease communicated it to another in another place. And so it happens, if not universally, almost universally. Nearly every man who joins a ship in such a condition has the prevalent disease sooner or later ; but no number of persons taken from such a ship, labouring under the disease in any stage, or in any force, and placed in a situation where the disease does not exist, though in the centre of a mass of healthy people, can excite it in a single instance. An accumulation of such facts, and there is a large accumulation, decides the question of the contagious power of the fever in the negative absolutely. To what then, in a case like this, is it to be ascribed, not only primarily but secondarily, as well during its progress as at its commencement ? The question, surrounded as it is with difficulties, admits of a very general answer only here ; neither the mass of facts, nor the line of inquiry which led to the conclusion, can be entered on ; but the conclusion is to this effect, that the local morbid atmosphere alluded to at the beginning of these remarks must be taken into account as an essential ingredient in the cause, to which must be added, as another essential ingredient, the results of chemical changes in the structural material of the ship herself. Hence change of place from the harbour (of Port Royal, Jamaica) to the Keys (a mile and a half off—a small sandy uninhabited island), or any such change, is unavailing to arrest the progress of the disease. The 'Vestal' sailed for Bermuda on the 18th of April, and the disease finally ceased on the 8th May, in the 27th degree of north latitude." And again, referring to the "Rainbow," in the same year on the same station, he says :—" The removal of the people from a 'sick ship' in a case like the present, is a point of great interest both practically and doctrinally. If the removal were to a place free from the cause of the disease, the writer of these remarks believes that the remedy would be complete, and that with it the disease would entirely cease, excepting in such persons as had imbibed its cause before they left the sick ship. In the case of the 'Rainbow,' the marines, etc., who returned to her for the discharge of certain duties, and who are said to have suffered particularly after being moved to the 'Magnificent,' were evidently exposed by such a return, to a certain extent, to the original cause of the fever." In regard to these remarks, I must observe, the crew of the "Rainbow" had been removed from her to the "Magnificent" after the fever had been some time in progress, "to have the holds cleaned, fumigated, ventilated, etc.," and that the marines, midshipmen, etc., returned to the "Rainbow," to keep watch on board her ; they did not sleep in the "Rainbow," but went back when the watch was over.

The navy furnishes many instances of ships which appear to have been particularly haunted by fever; these it would be interesting to trace, were the materials available. The "Tweed" we have seen to have suffered severely in Brazil; she suffered severely also in a previous commission. The "Vestal" had much yellow fever in the West Indies, and in a subsequent commission in India. I remember she had much fatal fever at Trincomalee, when other ships present and on the station had none. The celebrated "Eclair," now the "Rosamond," furnishes us with a notable example; her history in 1845 will not readily be forgotten; and now, after an interval of seven years she has suffered so much from yellow fever in the West Indies, that the Admiralty have thought it necessary to order her to England. It would be easy to multiply instances, but they would be uninteresting and unprofitable, without a full detail of all the concurrent circumstances, which I am unable to give.

In the "Cormorant," the cooks, carpenters, midshipmen, and stokers suffered most from this fever in the order mentioned. Out of five midshipmen in the ship three died, and only one escaped an attack. This young officer performed the same kind of duties, and was equally exposed in every possible way as the others had been. Three clerks and other young officers lived in the same berth, and slept in the same part of the ship as the midshipmen, without getting the disease.

It has been said that this fever often breaks out in ships in the neighbourhood of the pumps, well, etc. It was not so in the "Cormorant,"—there was no pump near the midshipmen, nor any well in the ship. The seamen and marines suffered comparatively very little, either in attacks or deaths; and the gun-room officers had an extraordinary exemption. None of the six were attacked. I have not been able to account satisfactorily for this exemption; but the relative position of the gun-room to the hold of the ship is so different from that of every other part of the ship, that it must be noticed here, in order that it may have due consideration when inquiring into the etiology of this disease. There is a free space between the gun-room and the hold, called the cockpit, or after troop-deck, where the young officers slept. There is no direct communication between the gun-room and this place; but there is a hatchway both before and abaft the gun-room, each precisely opposite, or rather underneath, another in the upper deck, so that, if any effluvium escaped from the hold, it did not necessarily become mixed with the atmosphere of the gun-room. No other part of the ship is similarly situated, and no other part occupying the same amount of space had an equal exemption from the disease. The gun-room, however, is a large airy place, and this may have contributed to the salubrity of it. One of the six gun-room officers had his cabin before the engine-room, slept and spent the greater part of his time there,—he escaped the disease as well as the others.

It is generally observed that steam vessels have more cases of dis-

ease in proportion, and particularly more cases of severe disease, than sailing vessels, notwithstanding that the accommodations in the former are much better; and it has been questioned how far this depends upon the coals so much used in steamers, or upon the combustion, or the heat evolved in the combustion of coals; and we have seen that it was reported at Bahia, that ships with a cargo of coals on board suffered more from this fever than others. All this, however, must be more a matter for future observation than for present consideration.

One instance of exposure to contagion which came to my knowledge is so remarkable, that I hope to be excused for mentioning it. The wife of a talented and experienced medical officer, living on board ship with her husband, had the fever when it was prevalent in the ship in 1850. Her husband was daily in contact with the fever patients, and slept at night in the same bed with his wife all the time she had the fever, yet did not get the disease; but, as if to show that he had no immunity from the disease, this officer had a smart attack of fever in 1851, when his wife also had a second attack.

All these instances of people escaping the disease after contact with those labouring under it, are equally instances of escape after exposure to any cause which might be supposed to be diffused generally in the atmosphere. This it is which renders the question so difficult of decision still, after having been disputed for so many years. If numbers were to decide it, I believe two out of three of all the medical men who saw and treated the disease on the Brazilian coast in 1850 are opposed to contagion.

My own opinion is, that this disease was not imported into Brazil; on the contrary, that it was of endemic origin. Whether it spread afterwards by contagion, or through the atmosphere, I do not profess to determine, but feel inclined to adopt the latter view. Of this, however, I feel convinced, that people arriving, more especially from a cold country, in a harbour like that of Rio, Bahia, or Pernambuco, when the disease is prevalent there, will almost to a certainty get the disease, even if they never touch a human being or susceptible agent in those places. I may be told that this is infection, or indirect or modified contagion. I do not care what it is called, but to this extent I firmly believe in it; so many instances of this method of getting the disease came under my notice, that it would be gross obstinacy to resist the evidence afforded by them.

It may readily be supposed that, after such a severe visitation from this fever as we had in the "Cormorant" in March and April 1850, we were anxious to avoid *all* the supposed causes of the disease; and, among others, we avoided Rio harbour as much as possible when the disease prevailed there, the ship being employed about the adjacent parts of the coast, to intercept vessels engaged in the slave trade. The different cruises were made as long as possible, and the return to harbour postponed. Want of fuel, water, etc.,

made it necessary to go into port occasionally however ; and, after the time stated above, while the fever prevailed in Rio, both in 1850 and 1851, we never remained over four or five days in the harbour without getting some cases of fever soon afterwards. These, generally speaking, were mild attacks. They gradually became fewer, after being a few days at sea, and generally disappeared before the end of the cruise. This was so often observed, and so clearly marked in the "Cormorant," that several, who at first ridiculed the idea of getting the disease by remaining a few days in harbour, and avoiding it by keeping at sea, ultimately became convinced of the propriety of the recommendations made to that effect ; and the ship often anchored in the little bays along the coast, about the same time ; had free communication with the villages and their inhabitants, without ever being suspected of either receiving or communicating the disease at those places.

Elsewhere we find casual remarks to the same effect. An English merchant ship—the "Columbus," at Pernambuco, "anchored very high up the harbour, far from every other vessel, escaped entirely." The Germans, and other European residents at Petropolis, where the disease was not communicable, if they descended to Rio, were certain of getting the disease. In the same way Englishmen, who had been for years at the mines in the interior, whither the disease never extended, when they came to Rio on their way home, got the fever and died, some before, some after embarkation.

There is reason to believe that a person may become so far habituated to the cause of this disease, as not to contract it ; while another person, similarly exposed under circumstances which appear to be precisely similar, will take the disease readily. And this cannot depend entirely upon acclimatisation ; as, out of two who had been so exposed, and had been residing for an equal length of time in a tropical climate, one took the disease, the other did not. In my opinion, it is more attributable to a quality of the poison itself, which, like many others, both poisons and medicines, loses more or less of its effect by being frequently applied in small quantities. In April 1851, the poison giving origin to the disease appeared to exist in a concentrated form at the island of Cobras in Rio harbour, where the stores for the British squadron are kept. At that time, out of a great many people who went there on duty, very few escaped the disease, if they remained there a few hours, no matter what ship they belonged to. Many of these speedily showed the worst type of the disease, and proved rapidly fatal ; and yet during all this time, the people who resided constantly on the island escaped the disease.

An instance occurred in Rio harbour in the first week of September 1851, when the master of a Swedish vessel died of decided yellow fever. There had been no case of the disease in the harbour for two weeks before or after this case. There had been only isolated cases at long intervals after June. Are we to suppose that all

the yellow fever poison in the harbour had been collected and imbibed by this man? Or rather,—Are we not to suppose that the poison existed in the harbour in a diluted form perhaps, giving credit, at the same time, to any peculiar susceptibility which this master might be supposed to possess, and also to his not being long from a cold climate, which of course rendered him more subject to the disease?

One circumstance which I noticed, without being able to account for it, is this:—On one day all the attacks of fever would prove very severe, the next day all would prove comparatively slight. One day would show two new cases perhaps, both fatal; the next day, four or five cases, all of which would recover; and perhaps in two days, more severe cases would again occur. All this happened after the ship had been several days at sea.

I feel convinced also that it is of the greatest importance to avoid exposure to the heat of the sun, under circumstances such as we were placed in. We had been in Rio harbour, and during our stay there got epidemic fever into the ship. There was no means of ascertaining who had imbibed a quantity of the poison sufficient to produce the fever in their own persons, and who had not. Now I believe it to be not only perfectly possible, but that it has actually occurred, that several people had received into their system a quantity of the poison, which would not have caused fever by itself, but that solar heat caused the development of the disease in these cases before the poison could be expelled from the system. I believe also that several people who had been partially exposed to the poison, and had begun to feel its effects, were able, by a mental effort, to resist its influence, until the greater part of the poison had been so expelled from the system, or had lost its power in some other way.

The disinfecting agents employed in the "Cormorant" were great cleanliness; perfect ventilation; the decks were kept as dry as possible; the men were exposed as little as possible to the night air and to a mid-day sun; windsails were kept set day and night; at Monte Video, however, the nights were so cold, that we were glad to dispense with them, except for a few hours in the middle of the day. Sir W. Burnett's solution of the chloride of zinc was also used, and it was found to be an excellent deodorising agent at all times. It was particularly useful in a case where death was caused by sphacelus of the upper extremity following fever, by sprinkling some of the diluted solution over the dressings, etc. It destroyed completely the offensive effluvia, which otherwise were most annoying to the patient and attendants. But the use and benefit of all disinfecting agents are liable to so many fallacies, that it is very difficult to apportion to each their proper share of the effect resulting from their employment. The principal one, and the one to which I looked forward with most confidence, and still have reason to believe the most effectual, was removing as rapidly as possible to a colder climate,—viz., to Monte Video. Having observed that diaphoresis proved critical in many of



the milder cases, in March 1851, after leaving Rio harbour, and getting the fever there, finding that many cases of the disease were still daily occurring, I recommended the commanding officer to drill the men smartly for an hour or two until they perspired profusely, hoping thereby to expel the poison, if by any means they had imbibed it. This was done. Of course it was a measure of which men will take different views, as their notions of the disease and its cause will differ; and it would be premature to judge of the result from one instance. I believe the measure defensible, both on physiological and therapeutic grounds, and would recommend, under similar circumstances, again to have recourse to it.

Another subject connected with this disease, which we must notice very shortly indeed, as it may be dismissed with few words, is this,—it is said to attack a person only once in a lifetime. We have already referred to the number of second attacks which occurred in the Portuguese men-of-war in Rio harbour in 1850, also to the second attacks in the “Cormorant” in the same year. In 1851, three of those who had an attack in the former year had another attack, but neither of them were severe; another had an attack in 1850, and two in 1851—all slight attacks; another man had two attacks in each year, the longest of them lasting only ten days; and other two had each a severe attack each year: of these two one had decided black vomit in 1850, and the other was also said by the attendants to have vomited “some black stuff.” But then we are told that the attack must be of sufficient intensity and force to secure protection from a second attack. What this intensity amounts to I have not been able to ascertain. If black vomit is to be the test, I do not know of any case that had that symptom twice. A person may be considered fortunate if he escapes after having it once. I believe, however, that all fevers (agues excepted), even the continued fevers of cold countries, give more or less protection against their own return for a time of longer or shorter duration; and why not yellow fever? During the prevalence of plague and the exanthemata, cases often occur without showing buboes or the usual eruption, and sometimes prove fatal. There is seldom any doubt about these being cases of the prevailing disease, yet they are very generally supposed to give less protection against a second attack than cases where the disease is more perfectly developed.

The English merchant ships “Carena” and “Empress,” of Liverpool, were at Bahia when the fever was prevalent there in 1850. There was a mate in each who were brothers; both had yellow fever formerly in the West Indies: and being supposed on that account to possess an immunity from the disease, they were appointed to attend the sick of their own ships. The former vessel lost three of her crew by fever, the latter one; but neither of the brothers got the disease as long as the ships remained at Bahia.

An English vessel, called the “Lady Sale,” had the fever at Bahia, in December 1849. She returned to that place again in



June 1850, with a new crew, the master being the only one who was in her on the previous voyage; he had the fever in 1849, and he was the only one who escaped the disease on the second voyage, when four of his crew died.

We may here make some observations on the disease as it appeared in 1851, in addition to those interspersed throughout the remarks already made. The first case in the British merchant shipping at Rio, and, according to the best information I could obtain, also the first case in the port, occurred in the "Apparition" on the 3d of January, when, discharging a general cargo on that side of the island of Cobras next the city, most of her crew fell victims to the disease before she left the harbour. The disease acquired its greatest prevalence and intensity in April. In that month H.M.S. "Thetis" remained for some time in Rio harbour; her working parties at Cobras got the disease. She left immediately for Monte Video, but lost fourteen from the fever. The "Thetis" had not been a year from England. None of our other ships on the station suffered so much in 1851, or had so many deaths, as the "Thetis."

The "Crescent" also suffered considerably; but any comparison between that ship and the other ships on the station would be an unfair one, inasmuch as she is used as a hospital ship for the severe cases of disease occurring in the other ships of the squadron, whence they are often received in a hopeless state; and she could not run away from the disease by going to sea like the other ships. The whole of her crew are never acclimated, as she is constantly receiving new hands from England, as well as from the merchant ships; her crew also have to perform the greater part of the work required at the stores at Cobras, where the cause of the fever evidently existed in a very concentrated form in 1851. One young man lent to that ship, and doing duty as a clerk at Cobras, got the disease there in April in a very severe form; he was of a ruddy complexion, and in full health. He had hemorrhage by mouth and anus for three days, recovered under the use of ol. terebinth., and enjoyed perfect health afterwards. A more extraordinary case of recovery never came under my notice.

Another case occurred in the same ship, also in a clerk, which deserves a passing notice. He had a very severe attack of the fever early in the year, recovered, but had not regained his usual measure of health and strength when he was again attacked in April, and died with black vomit, etc.

Several of the other ships of the squadron had many cases of the disease in a very mild form. In the "Cormorant" precisely the same number of cases occurred as in 1850, but the result was very different; they all returned to duty except two,—one of whom died, the other was sent to the hospital ship, and ultimately invalided for debility.

This generally favourable result I do not attribute to any improved

method of treatment, but chiefly to the people having been more acclimated, and partly also to the ship having kept at sea as much as possible, avoiding Rio harbour during the prevalence of the disease.

The powerful effect of acclimatisation in guarding the person against this disease, at least in mitigating it, should it make its appearance, is evident from several other circumstances besides the experience of the "Cormorant." In the city of Rio there were not 150 deaths from fever in 1851, I am told, although the proportional mortality in the merchant shipping in the harbour was much greater than in the preceding year. Up to the 30th June in each year, in the British merchant ships' crews, 1 in  $14\frac{8}{13}$  died in 1850, and 1 in  $8\frac{2}{7}$  died in 1851,—although in the latter year a better quarantine had been established, and a lazaretto and hospital for yellow fever cases from the shipping, at Jurujuba in Five Fathom Bay, distant some miles from the city. Such sailors as left their ships and went into lodgings on shore got the disease like those who remained on board, without communicating it to the people they came in contact with on shore.

In April there was a small squadron of small French men-of-war in Rio harbour on their way to the Pacific. They remained some time in the harbour without getting the disease, when other ships around them were getting it. They indulged in false hopes of security, despised the danger; and when it was realised, and they had lost some men by the disease, they departed. The United States' squadron, as in the former year, acted prudently,—avoided Rio harbour during the prevalence of the disease, and, so far as I know, did not get a single case of it in 1851.

The disease was more prolonged into the cold season in 1851 than in the former year. For three weeks, about the end of May and beginning of June, scarcely a case of fever occurred at Rio; before the end of the latter month, although the weather was remarkably cold for the season until as late as the middle of December, a considerable number of cases occurred in the merchant ships. A great proportion of them proved fatal; they also showed themselves at first with catarrhal symptoms, as the last cases in the previous year had done. One case occurred in the first week of September in the master of a Swedish merchant vessel, which proved rapidly fatal,—a decided case of yellow fever. A considerable number of fatal cases occurred again early in October, the weather being still remarkably cold. In fact, not a whole month passed throughout the cold season of 1851 without a fatal case of the disease happening in the port.

The disease was less universal in the merchant shipping in 1851 than in the previous year. Some ships escaped entirely; but such as got the disease suffered very severely.

There was no suppression of urine, black vomit, yellow suffusion, or hemorrhage of any kind observed in any of the cases that oc-

curred in the "Cormorant" in 1851. With the exception of these symptoms, the character of the disease was the same as in the preceding year. There was the same tendency to intestinal irritability which came on generally in the second paroxysm of the fever; the same inactive state of the bowels during convalescence, and the same indistinctness in the remissions. In some cases, instead of their being regular remissions, it would be more correct to say, that the evening paroxysm was absent every alternate evening. In two cases only were there some head symptoms, a tendency to stupor, etc., which soon passed off. In several cases there was considerable determination of blood to the lungs towards evening, as evinced by a dry short cough, a feeling of tightness across the chest, with some degree of pain on taking a full inspiration, slight wheezing, hurried respiration, etc.; these symptoms disappeared towards morning on the skin becoming bedewed with perspiration. In other ships all the more fatal symptoms were present, particularly suppression of urine, which was, at all events, more observed in the merchant ships in 1851, if not more frequent.

Between the middle of October and the middle of December 1851, there were only sporadic cases of fever at Rio, occurring at intervals; few of these were fatal, and none of them I believe had black vomit. On the 7th December the Norwegian corvette "Ornen" arrived at Rio from Europe, and one of her men died before the 20th of yellow fever in the hospital Misericordia. Two Swedish men-of-war arrived soon after the "Ornen," but on their becoming aware that she had got the disease they departed. Several fatal cases of decided yellow fever had occurred about the same time among sailors belonging to the ships of northern nations recently arrived. Some were attacked in the ships, others in the boarding-houses for seamen in the Rua Misericordia; from both they were sent to and died at the hospital on shore. And this occurred when acclimated people, both on shore and afloat, escaped the disease. After 1st January the disease became more generally prevalent.

Places on the coast of Brazil, which escaped the disease in 1850, got it in 1851. Maranhão has already been referred to; it appeared also in Ceará in August, and proved very fatal there. Both French and Dutch Guiana suffered from the disease in 1851.

In regard to the treatment of this disease, the general system pursued in the "Cormorant" (varied, of course, to suit particular cases) was, to give an emetic, and after its operation to move the bowels freely by stool, cooling saline purgatives being generally preferred. It was often no easy matter to excite this free action of the bowels, a repetition of large doses of purgative medicine having been frequently required; when once moved, however, they were easily acted on afterwards. A combination of calomel and antimonial powder was then given in small doses, and repeated generally every

three or four hours, for a longer or shorter period, according to circumstances, with the object in view of restoring the cuticular and intestinal secretions. Cooling saline diaphoretics were afterwards given,—generally a combination of the nitrate and carbonate of potass., of the sulphate and carbonate of soda, or the liquor amnion. acetatis,—in small doses, frequently repeated. During the febrile paroxysm, the skin being hot and dry, sponging the surface with vinegar and water was very soothing to the feelings of the patient. Quinine, and the infusion of quassia and gentian, were found useful during convalescence. Rice-water or barley-water was given freely for drink, and nothing else in the shape of food for at least two days; some of the patients preferred lemonade for drink, and latterly I considered this preference an unfavourable indication.

When head symptoms were the most prominent early in the attack, free purging, by means of croton oil, appeared to be very serviceable in removing them, and in diminishing the frequency and hardness of the pulse.

Irregular symptoms were treated as they appeared. Lumbar pain was often a distressing symptom, for which friction, with oil of turpentine, was often employed with much relief, so were sinapisms and stimulating liniments.

For irritability of stomach, and to check vomiting, sinapisms, turpentine, epitheimata, and blisters, were applied to the epigastrium. Effervescing draughts, with a few drops of tincture of opium, or of the solution of the muriate of morphia; or a few grains of ammon. sesquicarbonas in solution, with a little laudanum or morphia,—by the mouth, were employed with advantage. Enemata containing turpentine were also found useful.

A severe and distressing headache was often present, and often relieved by cold evaporating lotions of Eau de Cologne (a quantity of which had been opportunely found in the medicine-chest of a captured slave vessel a short time previously) and water, or vinegar and water; the hair having been, of course, previously removed.

In the cases where the bowels were so difficult to move at the first onset of the disease, the symptoms of intestinal irritability already described were very apt to supervene, and, if not combated early and strenuously, to lead to a fatal termination. If these symptoms were not checked at their very first appearance by hot applications to the surface of the abdomen, by blisters to the epigastrium, etc., it soon became necessary to produce the constitutional effects of mercury to the extent of affecting the mouth slightly, by whatever channel administered, and with a rapidity proportioned to the urgency of the symptoms; this was the only safeguard, and, as far as my experience goes, quite an effectual one. It was often no easy matter to effect this when there was much purging and the irritability had made some progress. In very urgent cases large doses of

calomel were given in combination with opium, to restrain its action on the bowels, and inunction with ungt. hydrarg. fort. employed at the same time. The quantity of the mineral required to produce mercurialism was often surprisingly large; often also the effect on the gums was not observed until the abdominal symptoms had declined for some days—until, in fact, the patient was convalescent; and not even in those who took the largest quantity was the salivation at all profuse.

In the same class of cases, also, the bowels were inclined to be inactive during convalescence; they would not act without the use of medicine; and, if allowed to pass two days or more without an evacuation, pain and tenderness of abdomen on pressure, frequent and ineffectual desire for stool, and at length diarrhœa, red tongue, etc., were apt to make their appearance. These symptoms were easily prevented by keeping the bowels moderately open by small doses of the neutral salts, given three times a day, in combination with a few drops of the diluted nitric acid, or by giving castor oil with a little tincture of opium, or of the sol. morph. hydrochlor. every second day. If the abdominal symptoms, however, had made much progress, mercurialism and counter-irritation, with hot applications to the surface of the abdomen, became necessary to remove them.

The question has occurred to me,—Whether this intestinal irritability might not depend for its cause upon the cathartics employed, or the irritation produced by them?—But, after giving the matter all the attention in my power, I have not been able to trace any necessary connection between them.

In those cases which showed every evening symptoms of determination of blood to the lungs, these symptoms always disappeared towards morning on diaphoresis taking place, and I endeavoured to induce this diaphoresis early in the paroxysm by putting the feet in hot water, giving mustard emetics, and then covering the person well with bed-clothes, and had every reason to be satisfied with the method, as it appeared to shorten the paroxysm generally, and often to prevent its recurrence.

In one case, which occurred on shore at Rio, in 1850, where, after the fever had subsided, pervigilium, from nervous irritability, remained, the exhibition of chloroform was followed by refreshing sleep, and a decided and permanent improvement in all the symptoms.

Homœopathy, if we believe those who advocate the system, was, as in almost all other diseases, generally successful in the treatment of this disease also.

The question of the contagious nature of this disease has been sometimes argued as a quarantine question. The two have no necessary connection; but, if the views expressed in the preceding remarks are correct, it will naturally follow that, as far at least as

ships are concerned, if it is wished to avoid the disease, it is of much greater importance to avoid ports where the disease is prevalent than to perform the strictest quarantine in those ports.

In 1851, all the cases of fever in the "Cormorant," in January and February, occurred at Bahia, or its neighbourhood; those after that period at Rio, or near it. The ship went into Rio harbour on the 11th March, and sailed again on the 17th of the month. From the 16th onwards cases of the disease became very frequent until the 25th; and, in that period of ten days, nineteen cases of fever occurred.

One case (No. 25 in 1850, and No. 23 in 1851, in the accompanying tables) deserves notice, inasmuch as he was of a very strumous habit. He had a large open scrofulous glandular swelling on each side of the neck, before the fever appeared on the coast; these subsided and healed under the use of cod-liver oil; they again enlarged and suppurated, after the smart attack of fever he had in 1850; and again disappeared under the same treatment. In the attack of fever in 1851, he was nearly carried off by the intestinal irritability so often noticed in these remarks; and I think his life was saved by mercury, a large quantity of which was exhibited by the mouth and by inunction before the gums became affected, and as soon as this was observed he began to improve; the mercurialisation did not even advance to salivation. He remained long weak, but was paid off in the ship, in February 1852, in perfect health.

I think we may conclude from the preceding remarks,—

1st, That a certain elevation above the sea level, as well as a certain distance from the equator, give a sure protection from this disease, if the poison is not received elsewhere. What precise degree of latitude, or number of feet of elevation, gives this protection, it would be premature, with our present amount of knowledge, to attempt to define; and it would not be unreasonable to imagine that the required degree of elevation depends partly on the latitude of the place (in the same way as the height of the snow line depends on the same circumstance), and on the degree of concentration of the poison.

2d, That the young robust natives of high latitudes (or cold countries), recently arrived from those places into an intertropical country where the disease is prevalent, not only get the disease earliest, get it when others escape, but also get it in the most severe and fatal form, and persist in getting it after it has ceased among more acclimated people.

3d, That acclimated people, natives of the same high latitudes, suffer in a milder degree.

4th, That natives of the place, and natives of the tropics generally, suffer in a milder degree still.



5th, That negroes suffer very generally, but not severely.<sup>1</sup>

6th, That from the age of fifteen to that of twenty-five is the most fatal age.

7th, That after the age of thirty, or thereabouts, the degree of susceptibility to this disease diminishes with advancing age.

8th, That females have no exemption from the disease.

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*Number of People on shore and afloat at Rio de Janeiro, of the different nations named, the Number of these who had Fever, when it could be ascertained, and the Number of Deaths from Fever, between 1st December 1849 and the 30th June 1850 :—*

Nations.	On shore, and afloat.	Number present at Rio de Janeiro.	Number who had fever.	Number who died.
British .....	On shore .....	400	360	69
" .....	In merchant ships .....	2150	—	153
Belgians .....	On shore .....	60	60	28
" .....	In merchant ships .....	52	43	14
Bremenese ...	On shore .....	50	25	4
" ...	In merchant ships .....	10	10	4
Danes, .....	On shore .....	200	—	2
" .....	In merchant ships .....	300	200	27
Russians .....	" .....	270	236	46
Swedes and )	On shore .....	1080	1000	126
Norwegians }	In merchant ships ... }			
Spanish .....	On shore .....	356	340	32
" .....	In merchant ships .....	138	26	5
Portuguese ...	On shore .....	22,000	17,500	1300
" ...	In merchant ships .....	552	500	27
" ...	In men-of-war .....	724	680	129
Sardinians ...	On shore .....	2000	600	200
" ...	In merchant ships .....	500	—	60

For the above numbers I am indebted to Dr Deas, lately surgeon of H.M.S. "Southampton," on the Brazil station, whose zeal in searching for information about this fever is beyond all praise of mine. It is to be hoped that he will give the result of his inquiries and observations to the public, as from such a source they must be very valuable.

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<sup>1</sup> The late able and experienced Dr William Fergusson, Inspector of Hospitals, always maintained that blacks never get this disease.

## CASES OF FEVER

As they occurred in the "Cormorant," the number of the case, date of attack, of discharge, ages as near as could be ascertained, the result, etc., from 1st February to 30th September 1850:—

No. of case.	When attacked.	Age.	When discharged.	No. of days on sick report.	Remarks, result, etc.
1	Feb. 10	25	Feb. 17	7	
2	... 14	46	... 22	8	
3	Mar. 4	19	Mar. 9	5	Died at 8 A.M., when away from the ship; no medical attendance, but sedulously cared for by his companions; had yellow skin, delirium, and black vomit.
4	... 19	32	April 4	16	
5	... 21	25	... 10	20	
6	... 22	35	Mar. 27	5	This was also the subject of Case No. 39.
7	... 22	25	May 13	52	Invalided for palpitation, and organic disease of the heart following fever; had an attack of fever in H.M.S. "Crescent," previous to this one.
8	... 22	26	... 13	52	
9	... 22	46	April 22	61	
10	... 22	16	... 1	10	Invalided for hepatitis, following fever; was said by the attendants to have had black vomit.
11	... 22	19	Mar. 27	5	
12	... 22	24	May 29	68	
13	... 23	34	April 7	15	Died at 11.25 A.M. Sent to hospital-ship "Crescent," for hepatitis, following fever, and returned to his own ship, and to his duty, on 6th July; was said by attendants to have black vomit.
14	... 23	21	Mar. 27	4	
15	... 23	23	... 26	3	
16	... 24	38	April 1	8	Died at 8.30 P.M. Also the subject of Case No. 42. Was in attendance on the commander, Case No. 9, who had fever, when attacked; he was also the subject of Case No. 40.
17	... 24	15	... 9	16	
18	... 24	25	... 1	8	
19	... 24	17	... 11	18	Had black vomit for two days. Died at 11 A.M.
20	... 25	18	... 6	12	
21	... 25	32	Mar. 28	3	
22	... 25	26	... 29	4	Died at 9.45 A.M.; sphacelus of right upper extremity the immediate cause of death.
23	... 26	23	April 12	17	
24	... 26	34	Mar. 30	4	
25	... 27	26	April 9	13	Died at 6 A.M.
26	... 28	35	... 7	10	

CASES OF FEVER, Etc.—(Continued.)

No. of case.	When attacked.	Age.	When discharged.	No. of days on sick report.	Remarks, result, etc.
27	Mar. 28	25	Mar. 31	6	Died at 11 P.M. ; said he had a severe attack of yellow fever in the West Indies, some years ago, in H.M.S. " Scourge."
28	... 28	36	April 6	9	
29	... 29	20	... 7	9	
30	... 29	24	... 5	7	Died at 2.50 P.M. The commander's steward, and in attendance on him when attacked by the fever.
31	... 30	20	April 7	9	
32	... 31	20	... 4	5	
33	April 20	17	... 30	10	Died at 11.30 A.M. ; this youth was of a phlegmatic, indolent, habit of body, subject to ulcers, and had two open when seized with fever, for which he had been on sick report since 1st February.
34	... 23	24	... 27	4	
35	... 26	34	May 9	13	
36	... 30	28	... 29	30	Sent to hospital-ship " Crescent," for hepatitis, after fever ; and was ultimately invalided from that ship. Sent to hospital-ship " Crescent," for hepatitis, after fever, and was ultimately invalided from her ; he had sewn up all the bodies of those who died previous to his own illness, before their interment.
37	May 1	37	... 29	28	
38	... 2	24	... 5	3	
39	... 18	25	... 25	7	Died at 6 A.M. ; three days and seventeen hours ill. Also the subject of Case No. 5. Also the subject of Case No. 16. Never entirely confined to bed. Also the subject of Case No. 15.
40	... 20	38	... 28	8	
41	July 20	20	Sept. 11	83	
42	... 20	23	Aug. 1	10	

All the cases not otherwise marked returned to duty, including Case No. 12. Twenty-eight out of forty-two recovered.

Average duration of the attack,	16.3	days.
Ditto do. of recoveries,	13.32	...
Ditto do. of fatal cases,	6.54	...
Ditto do. of invalided cases,	52.0	...
Ditto do. of hospital cases,	43.0	...

## CASES OF FEVER, Etc., FOR 1851.

No. of case.	When attacked.	Age.	When discharged.	No. of days on sick report.	Remarks, result, etc.
1	Jan. 10	18	Mar. 4	53	This youth had a narrow escape, having had much of the intestinal irritability referred to in the remarks.
2	... 11	21	Feb. 16	36	
3	... 19	34	Jan. 27	39	The subject of Case No. 35 in 1850.
4	... 19	18	Feb. 5	17	
5	... 19	16	... 9	21	
6	... 28	18	Mar. 23	54	
7	... 29	24	Feb. 5	7	The subject of Case No. 25 also, and of Nos. 15 and 42 last year. Died at 1.30 P.M.
8	Feb. 8	23	Mar. 9	29	
9	... 8	26	Feb. 14	34	This was a hospital case, No. 12 in 1850; the attack this year was a severe one. A severe attack.
10	... 19	31	... 28	37	
11	Mar. 16	25	April 17	32	
12	... 17	16	... 17	31	
13	... 17	27	Mar. 27	10	Had black vomit in 1850; this was also a severe attack. The subject of Case No. 17 in 1850. The attack of fever was followed by bronchitis and diarrhoea.
14	... 18	30	... 23	5	
15	... 18	50	... 23	5	
16	... 19	18	April 17	29	
17	... 19	16	... 17	29	The subject of Case No. 25 in 1850; on the present occasion he escaped narrowly with his life.
18	... 19	20	June 9	82	
19	... 19	25	April 14	26	
20	... 19	23	Mar. 26	7	
21	... 20	39	April 27	38	The subject of Case No. 7 also, and of Nos. 15 and 42 last year.
22	... 20	33	Mar. 28	8	
23	... 21	28	May 21	62	The subject of Case No. 4 in 1850, and of No. 38 in 1851.
24	... 21	43	Mar. 26	5	
25	... 21	24	... 29	8	The subject of Case No. 13 in 1850. In 1850 had acute pericarditis after acute rheumatism; at this time acute pericarditis supervened on second day of fever.
26	... 22	26	April 2	11	
27	... 23	33	Mar. 28	5	
28	... 23	23	... 30	7	
29	... 25	35	... 29	4	
30	... 31	22	April 24	24	
31	April 4	35	... 11	7	
32	... 18	25	... 24	6	

CASES OF FEVER, ETC., FOR 1851.—(Continued.)

No. of case.	When attacked.	Age.	When dis-charged.	No. of days on sick report.	Remarks, result, etc.
33	April 21	19	April 24	3	Had very threatening symptoms when first attacked ; but had no return of fever after the first remission.
34	... 25	42	... 29	4	
35	... 25	17	... 29	4	
36	... 25	49	... 30	5	
37	... 28	23	May 7	9	
38	May 1	33	... 14	13	The subject of Case No. 27, and of Case No. 4 in 1850.
39	... 2	20	... 28	26	
40	... 5	17	... 11	6	
41	... 24	18	July 4	41	Sent to hospital-ship "Crescent," and subsequently invalided, not having made a perfect recovery.
42	June 10	26	June 25	15	

Average duration of the attack 21·28 days.  
Do. Do. recoveries 20·6 ...

ARTICLE III.—*A Notice of an Epidemic Fever which occurred at Grey Town, Nicaragua, on board H.M.S. "Rosamond," formerly the "Eclair."* By J. WATSON, M.D., Surgeon, Royal Naval Hospital, Jamaica.

IN consideration of the interest that may be felt respecting an epidemic fever occurring on board the ship so unhappily notorious under her former name the "Eclair," and also because it involves some points of practical importance, I beg to communicate the following notice of the fever which recently broke out on board the "Rosamond" at Grey Town. I have been indebted to Mr Forbes, the surgeon, for the greater part of the facts which did not come under my own observation:—

The "Rosamond" arrived here from Europe in December 1851, and afterwards made several trips to the Spanish Main and among the islands, during which her people enjoyed good health. There were a few cases of mild seasoning fever, such as most new crews experience on first coming to the West Indies, but the general sanitary state on board was quite satisfactory. On the 4th June she took her departure from Port Royal, and anchored at Grey Town, Nicaragua, on the 9th of the same month, with no disease then on

board. The anchorage of Port Royal not being productive of marsh remittent fever, terminating in ague, there can be no doubt that the serious disease which soon after manifested itself in the "Rosamond," had its origin in the harbour of Grey Town.

That harbour is a circular basin at the mouth of the river St Juan, landlocked, and surrounded by a low swampy country, containing many lagoons, and covered with thick forests, and a dense undergrowth of tropical vegetation. The river opens into the basin by several mouths, with low alluvial islands between them; its bottom is a thick mud, and the beach is composed of a dirty muddy sand, with a nearly impenetrable *bush* towards the land. The vessel lay near the centre of the harbour, which is not less than half a mile from the shore, from the 9th June till the 4th July, when having, in the interval, had thirty-seven cases of fever, she proceeded to the Island of St Andrews, which is 150 miles to windward, far removed from the coast, and esteemed very healthy. She returned to Grey Town on the 11th July, and the day following, in consequence of the continued and increasing sickly condition of her crew, she put to sea, and made the best of her way to Jamaica, where she arrived, and disembarked her sick at this hospital, on the 17th July.

While the "Rosamond" remained at Grey Town, it rained almost incessantly the greater part of every day. The awnings were sloped to protect the people from the rain; but the protection so afforded was only partial, and the interior of the vessel, as well as the clothing and the bedding, was necessarily very damp. The temperature was comparatively low, being usually from 76° to 78° Fahrenheit in the shade. I believe it has been the practice to permit the crews of ships stationed at Grey Town to fish in the river with the seine. The "Rosamond's" people received this indulgence, and a portion of them used to fish in the river, when the weather permitted, from four to seven o'clock in the morning. The amusement was of course attended with much bodily exertion, and complete exposure to wet while it continued, at least as far upwards as the waist. Simple as this matter of fishing may seem, it will be seen that it was followed by the most pernicious results. It has been ascertained by lists which were kept, that sixty men went to fish from once to six times each, including officers, and that ninety never went on these fishing parties. Because some persons are so constituted as to be able to expose themselves with impunity, and are daily seen to do so, it is not uncommon to hear their *escapes* advanced as proofs of the innocence, or even healthfulness of the exposure, and its concomitants. But when, as in the present case, a ship's company become the subjects of the experiment, and when the results are stated numerically, as they will be found below, the fallacy of such notions must become immediately apparent. It will be seen that they who entertain them mistake the exception for the rule.

The daily progress of the disease is shown in the following abstract from the sick list:—



*Fever Cases put on Sick List on*

June 23, . . . . .	3	July 9, . . . . .	1
" 24, . . . . .	1	" 10, . . . . .	5
" 25, . . . . .	2	" 11, . . . . .	1
" 26, . . . . .	3	" 12, . . . . .	3
" 27, . . . . .	3	" 14, . . . . .	2
" 28, . . . . .	8	" 15, . . . . .	1
" 29, . . . . .	3	" 16, . . . . .	2
" 30, . . . . .	3	" 17, . . . . .	1
July 1, . . . . .	6	" 18, . . . . .	2
" 2, . . . . .	3	" 19, . . . . .	2
" 4, . . . . .	2	" 20, . . . . .	1
" 5, . . . . .	4	" 21, . . . . .	2
" 6, . . . . .	7		
" 7, . . . . .	5		
" 8, . . . . .	1		
		Total, . . . . .	77

This fever was of the remittent type, and precisely similar to those which, in the same locality, have affected almost every ship-of-war stationed there during the last four or five years. The cases which occurred at Grey Town, presented sharp pyrexial symptoms for the first three or four days, with headache, pains of back, dry skin, and thirst, becoming then remitting, and finally intermittent. Those which occurred at St Andrews after the "Rosamond" left the malarious region, are said to have been milder; and of the fifty-three which were sent to this hospital, the majority had assumed the form of ague at the time of admission. The few cases which appeared on board, after the ship returned to this place, and which were sent immediately to my care, very soon became convalescent, except one man, who had anomalous convulsive symptoms, and another, who nearly died of epistaxis.

Of the treatment in this place, little need be said. There was seldom need for depletion, and mercury was not exhibited, and those who survived, therefore, convalesced rapidly; but moderate antiphlogistic measures were employed in the pyrexial periods, and quinine formed the principal remedy in the apyrexial. As soon as intermissions occurred, wine, punch, and a generous diet, were found highly serviceable. A similar plan was adopted on board; but the Surgeon there generally commenced the treatment with an emetic, and afterwards administered some antimonial medicine. As there was scarcely any irritability of stomach in any of the cases, the emetic treatment answered well, although, on account of a traditional antipathy against it, we avoid it in this hospital, as likely to induce that most troublesome symptom.

From associations connected with the former history of the "Rosamond," there was a considerable panic, and a very general apprehension among her people, that the cause of this illness was something in the ship. It is quite clear that this could not have been the case. She arrived healthy at Nicaragua, and, after fourteen

days, her first case of fever showed itself. She left the place, the last time, on the 12th July, and nine days afterwards fresh cases ceased to occur, although only 51 per cent. of the people had gone through the disease when it stopped.

The salient points of interest connected with this sickness in the "Rosamond," are embodied in the following statements:—

"Rosamond" had on board 150 officers and men, of whom 77 or 51·3 per cent., took fever.

Of the above 150, 60 went from once to six times each man, on fishing parties.

Of the above 150, 90 did not go on fishing parties at all.

Of the 60 fishers, 39 took fever, or 65 per cent., and 35 per cent. escaped.

Of the 90 non-fishers, 38 took fever, or 42·2 per cent., and 57·7 per cent. escaped.

Died of fever or its consequences on board,	7
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Died of fever or its consequences in hospital,	2
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Invalided in consequence of fever,	6
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but generally with complication of other complaints of some standing. The commander and a stoker were also invalided, but they were not in the surgeon's fever list, and are therefore excluded here.

From the above contrasted statements, it is manifest that the liability to fever was increased nearly 23 per cent. by the practice of fishing, and its accompaniments. At least I cannot imagine any cause besides that, for the remarkably larger number of seizures among those who fished, than among those who did not. The increased danger among the men who went fishing, must be due to the bodily exertion, or to their being exposed to a more concentrated malarious poison, than their shipmates who remained on board half a mile distant, or to both causes conjoined. I am inclined to believe that any bad effects of the bodily exertions, in a temperature by no means high, were counterbalanced by the good effects of mental exhilaration in the enjoyment of a favourite amusement; and I attribute the unhappy results of the fishing to the fact of the people being brought into the heart of the malarious district in the river, where the poison was probably most energetic. However that may be, we are justified in concluding that, if the sixty men had remained on board, their proportion of attacks would have been twenty-five, and not thirty-nine, as it actually was, and that fourteen cases of fever were added to the list, which might have been prevented, had the commanding officer been aware of the additional source of danger; which, of course, he was not. I am certain that the practice of fishing with the seine in the Grey Town river, has been the rule and not the exception, among the ships stationed there, and all of them have been sickly. If a statement similar to this had been drawn up respecting the sickness on

board one of those ships, and that paper, with results corresponding with the above, had fallen under the observation of the captain of the "Rosamond," that officer would have probably not followed the practice of his predecessors.

It is in the hope of disseminating more correct notions of the danger attending the unnecessary exposure of men in unhealthy districts, that I have ventured to record the facts which form the subject of the preceding observations, and more especially that, if the Admiralty continue to keep a ship-of-war in that most fatal place, Grey Town, the danger of the special exposure herein reprobated may be fully appreciated. As a proof how little people are in the habit of recognising such causes of disease, even when they are in full operation, I may mention, that officers of intelligence were quite unaware that more cases of fever occurred among the fishing parties than among their other shipmates. Convinced, however, of the facts by the lists which were kept, the inference is admitted doubtingly and unwillingly, although any other conclusion from the premises seems impossible.

NAVAL HOSPITAL, JAMAICA,  
10th August 1852.

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ARTICLE IV.—*Cases of Induction of Premature Labour.* By  
GEORGE W. THOMSON, M.D., Licentiate of the Royal College  
of Surgeons of Edinburgh, Hawick.

THE induction of premature labour, while it is the simplest, and one of the most beneficial, of the operations in midwifery practice, is certainly at the same time the one of all others least frequently resorted to by the general practitioner, and consequently least known to the class of patients for whose benefit it has been proposed. A dislike to interfere with, or interrupt, the natural course and duration of pregnancy, and the dread that such interference, if attempted, might be followed by some disagreeable or dangerous consequence to the mother or the child, have gone so far in counterbalancing the advantages the practice otherwise possesses, that the result has been, in too many instances, that patients, the capacity and general formation of whose pelvis is such as to preclude the very possibility of their bearing a full-grown living child, are allowed to go forward to the completion of pregnancy, with the certain result awaiting them, that their labour, in addition to all its ordinary train of hazards, can only be completed by their being subjected, not certainly to a painful,—as, since the discovery and use of chloroform in midwifery practice, pain, in the great majority of cases, may be avoided,—but to an operation highly dangerous, and often hurtful to themselves, and whose sole aim and object is the mutilation and destruction of their

infant. The following narrative of cases, though containing nothing either novel or unusual in the practice of this operation, may, so far as they go, at least assist in strengthening the belief in its utility and safety.

Mrs H., aged 39, of remarkably short stature, but full and plethoric habit of body, florid complexion, and generally in the enjoyment of excellent health, was, on the 29th October 1848, confined of her first child. Her labour was long protracted, and very severe in its nature; the first stage was got over with tolerable ease, but no sooner was that completed, and the second stage of labour set in, than her sufferings became almost intolerable. The head of the child after a time became firmly impacted in the brim of the pelvis, where, though the uterine contractions were remarkably powerful, and scarcely ever absent, it remained for many hours; delivery by the forceps was found to be impossible, and at length, after the greatest amount of suffering, and when nearly exhausted, she was delivered of an ordinary sized child by the crotchet and perforator.

On the 12th November 1849, Mrs H. was again confined with her second child. On this, as on the former occasion, her sufferings were very great and long-continued. From the commencement of the second stage till its completion, nearly thirty hours elapsed, during which the expulsive pains were constant and very powerful; she was delivered, however, at last, after being nearly two days in labour, without the assistance of instruments, of a still-born and rather diminutive child, its head fearfully misshapen, and, to use her own expression, "its eyes nearly squeezed out" by the long-continued pressure to which it had been exposed.

These particulars of her former confinements were related to me by Mrs H. when nearly six months advanced in her third pregnancy. Suspecting that some very serious obstruction or deformity must exist in her pelvis, to account for these unfortunate results, I elicited, on inquiry, that in early youth, when only about three or four years of age, she had met with a severe fall from a considerable height, which had produced lameness in the left limb. This had, however, gradually worn off; and in fact, so little after-consequences had it to all appearance produced, that until recalled to her mother's recollection by my inquiries, it had been entirely forgotten. With this exception, she could assign no other cause for her difficulty in parturition, nor could I, by external examination, detect any. To satisfy myself, therefore, still farther on the subject, I proposed an examination of the pelvis, *per vaginam*, with the view of discovering, as nearly as possible, its capacity and dimensions. To this Mrs H. agreed; and I then felt convinced, from the extreme narrowness at the brim, and a remarkable flatness in the left side of its cavity, that it was impossible for her to bear a full-grown living child. Under these circumstances, and to give her the benefit of the only chance which seemed to exist of a fortunate termination to her pregnancy, I proposed the induction of

premature labour, the nature of which I explained to her, and to which, after some hesitation, she was persuaded to submit. I accordingly resolved to bring on labour between the seventh and eighth month, or about six weeks prior to the completion of her full time.

Early in the forenoon of the 24th October 1850, therefore, I introduced a sponge-tent, mounted on the firm, bent, steel probe recommended for this purpose by Dr Simpson, well up through the os uteri, which I found to be small but very dilateable, the cervix being in the undeveloped state proper to, and usually found at, this period of pregnancy. In passing up the sponge-tent into the interior of the uterine cavity, which was done with very little difficulty, its sharp conical point ruptured the membranes; but as its thick extremity completely filled up the os and cervix uteri, there was no escape whatever of the liquor amnii. The circumstance of the membranes having been ruptured in this way, though not usually advised, I certainly found to be of considerable advantage, as, by the sponge being brought into immediate contact with the liquor amnii, it expanded much more rapidly and completely than it would otherwise have done, and the first stage of labour was so much the more readily completed.

Within two hours from the time of the tent being introduced, Mrs H. felt distinct labour pains commence, which went on steadily increasing, till she sent for me about eight P.M., when I found her in smart labour, which, to all appearance, promised to be rapidly approaching its completion. On examination, the os uteri was fully dilated, the foetal head presenting in the first position, and nearly filling the cavity of the pelvis. In little more than an hour the head came to press upon the perineum; and before ten P.M., scarcely twelve hours after the operation for inducing premature labour had been performed, she was delivered, with great ease, of a very small, but sufficiently lively, female child, who, I am happy to say, is now a healthy and promising little girl, of nearly two years old, and not under the average size of children of a similar age.

About three weeks ago, I was again consulted by Mrs H. on the approach of her fourth confinement. On this occasion I had no need of persuasion to induce her to submit to the same operation she had formerly undergone; but regretted to find that, through miscalculation on her part, she had allowed herself to go fully a week farther on than the time at which she was last delivered. On making inquiry as to the last appearance of the catamenia and her time of quickening, I came to the conclusion that she wanted only five weeks to the completion of her full time of pregnancy, which I regretted, as giving the child less chance of living; and the more so from the fact, that owing to certain other circumstances, I found that it would be impossible for me to place her under treatment for still a few days longer.

On the 7th September 1852, the very earliest day that could be

embraced from the time of her application, I accordingly again introduced, in the same way as before, the sponge-tent; but on this occasion avoided rupturing the membranes. Towards the morning of the eighth, labour-pains came on, but of a languid kind, which continued at intervals during the day, but subsided in the evening, when she fell into a sound sleep, from which she was only awake at five next morning, by a renewal of the pains.

On making an examination during the forenoon of the 9th, I found the os uteri only partially dilated, the membranes entire, and presenting the foetal head still lying far up above the brim, and the sponge, tolerably well expanded, lying between the inner surface of the uterus and the amnion, which it had separated to a considerable distance anteriorly. With the view of hastening on delivery, I introduced a second tent between the membranes and the posterior aspect of the uterus; and on my return in the evening, found my patient now in active labour, the membranes ruptured, and the child's head presenting in the cavity of the pelvis. On this occasion, however, her labour, though greatly easier, was in many respects more like her two first confinements. The expulsive efforts were constant, continuous, and severe; and it was not until after the expiry of some hours that she was delivered of a living, but small female child, presenting the appearance of a foetus at the eighth month, its head much flattened, and the face and surface of the body pale and livid, from continued compression. In a short time this lividity wore off, the child began to cry freely, and otherwise evinced so much strength as to give great hopes of its surviving. In this, however, we were disappointed; about twelve hours after birth, efforts at retching came on, and a considerable quantity of frothy blood and mucus was discharged from the mouth and nostrils, which being soon succeeded by convulsions, the child died some hours after, and within twenty-four hours from its birth.

Before adopting the practice I followed in the two last confinements of Mrs H., that of turning (proposed and so ably advocated in a late Number of this Journal by Professor Simpson) was maturely considered, and the question, whether Mrs H. might not be allowed to go on to her full time of pregnancy, and an attempt made at that period to deliver her by version of the child, was thoroughly weighed. The amount of obstruction at the brim, and in the cavity of the pelvis, was, however, I considered, so great as to preclude the hope of saving the child by this operation, although it certainly might have, to a certain extent, shortened the period of suffering to the mother. And if proof were needed for the utility of, and urgent necessity for, the induction of premature labour in Mrs H.'s case, the history of her last confinement abundantly afforded it. Here, ten days or at most a fortnight, and that between the seventh and eighth month, made all the difference between an easy labour, followed by a living and healthy child, and a severe and tedious one, ending in the birth certainly of a living



child, but which died within twenty-four hours, evidently from the effects of compression on the brain. Her second delivery was certainly terminated by her own efforts, and without the use of instruments; but there was a circumstance during this pregnancy which differed from her former or succeeding ones, and which had a powerful influence over her delivery. This was the fact that, about three months previous to her confinement, and while cholera was very prevalent in her locality, Mrs H. had successive attacks of vomiting and severe diarrhoea, one of which confined her closely to bed for three weeks, and left her, till the period of her delivery, weak and feeble, her appetite almost gone, and her general health considerably shaken.

The natural consequence of this was, that the child, a female one, was small and diminutive, not much larger, it was stated to me, than the eighth month child of which I delivered her last. And even with this circumstance acting in her favour, it has been seen that her labour was severe, very protracted, and unfortunate.

From her latest confinement, Mrs H. has made a steady and excellent recovery, and has but one regret, namely, the circumstance of not having earlier applied to me to have labour brought on; and so fully satisfied is she of the propriety of this operation in her case, that, should she be placed in similar circumstances again, she will not only cheerfully submit to its performance a third time, but take care to give more timely warning when the period of its necessity approaches.

HAWICK, 23d September 1852.

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ARTICLE V.—*Case of Foreign Body lodged in the Air-Passages for Four Years and a Half; terminating Fatally by Gangrenous Abscess of the Right Lung.* By JAMES STRUTHERS, M.D., Leith.

IN October 1844, Thomas Neal, a footman, æt. 22, while eating part of a fowl, and laughing at the same time, was suddenly seized with a violent fit of coughing, and a feeling of suffocation; he became blue in the face, felt a sharp pain in the chest, and was sensible of part of his food having entered the windpipe. These symptoms subsided in about half an hour, and never returned. An emetic was administered, and acted freely; and both fluids and solids were swallowed without difficulty. From about an hour after the accident a tickling cough, with a wheeze in the throat, continued to trouble him occasionally, but gave little inconvenience; and he went about his work as usual, as if nothing had happened. He still, however, was impressed with the conviction that there was something in his

windpipe, and pointed to a spot a little to the right of the upper part of the sternum, saying that he felt it there.

About three months after the accident, the cough began to be accompanied by the expectoration of white frothy sputa, which, without any other change in the symptoms, gradually increased in quantity during the ensuing twelve months. At the end of that time he was seen on several occasions by Sir Benjamin Brodie, and entered St George's Hospital, London, where he remained for a fortnight, and was then advised to go to the country. About a month afterwards he, for the first time, observed the sputa to be tinged with blood, and to have a foetid odour. During the two following years the cough was more frequent, the expectoration very profuse, and the quantity of blood in the sputa gradually increased, as did also the foetor of the breath. This last symptom became so marked in 1848 as to oblige him to leave his situation, for which he was in every other respect fit. During the greater part of 1848, he had exacerbations of the cough every two or three weeks; at these periods there was increased foetor of the breath, the sputa contained a considerable quantity of florid blood, and he occasionally brought up as much as half a pint of pure blood at a time. In the beginning of November 1848, he had a rigor, followed by pain in lower right side, increase of cough, and shortness of breath, and the sputa became of a brown colour (pneumonia?); for this he was treated by a physician in Musselburgh. In the beginning of the following month, he entered the Royal Infirmary of Edinburgh, under the care of Dr Bennett. He was then pale, but by no means emaciated; he complained a good deal of cough, which occurred at frequent intervals, and was accompanied by profuse expectoration of viscid sputa, very foetid and stained with blood. On examining the chest, there was dulness on percussion over the inferior three-fourths of the right side, both in front and behind, but most decided a little below the nipple. The left side was resonant throughout.

On auscultation, the vocal resonance was found increased over the whole of the right side, particularly at a spot a little below the nipple. At the middle of the same side, posteriorly, a gurgling râle was heard over a space two inches square; in the other parts of this side, the respiratory murmur was very harsh, and much obscured by mucous and sibilant râles; it was least affected at the apex. On the left side the respiratory murmur was puerile throughout, and unaccompanied by any râle. The appetite was good; bowels regular; urine healthy; he slept well; was free from pain; and the voice was unaffected. During the next three months, there was but little change in his condition. For weeks at a time the sputa were free from blood; but every two or three weeks they became bloody for several days at a time, and then also the cough was more frequent, the expectoration more profuse, and the foetor of the breath and sputa greatly increased; while the urine deposited large quantities of pink urate of ammonia, and contained numerous crystals of the

oxalate of lime. After a residence of three months, he left the Infirmary in the beginning of February 1848. For the next six weeks he enjoyed tolerable health, being able to walk a considerable distance without inconvenience, experiencing shortness of breath only on walking fast; the cough continued pretty constant, with copious expectoration of whitish sputa, generally foetid, and only occasionally tinged with blood. Once or twice he expectorated several small masses of a brown colour and of some consistence; these he imagined to be portions of the foreign body.

Towards the middle of March his appetite and strength began to fail; he lost flesh and became feverish, thirsty, and restless; was obliged to confine himself to the house; and suffered from shortness of breath, even when at rest; the cough and expectoration continued much the same; he had no rigors, and was free from pain. On the morning of the 24th, he awoke suffering from great increase of cough and shortness of breath, and continued during the day to expectorate, at intervals of a few minutes, large quantities of frothy sputa, deeply tinged with blood, and much more foetid than usual. I was asked to visit him at his own house on the 25th, and found him much weaker than when I had last seen him, some weeks previously. The cough was constant, the expectoration profuse, the sputa frothy and mixed with florid blood; the breath and sputa had a gangrenous odour, which was very perceptible on approaching the bed. He had no pain, his chief complaint being of great weakness, dyspnoea, and occasional feeling of suffocation. The respirations were 45; the pulse 130, weak and wiry. On examining the chest, the right side was found scarcely to move on inspiration, and was universally dull on percussion; all natural respiratory sound was absent; gurgling was audible over the greater part, both in front and behind, with coarse mucous and subcrepitant râle, towards the upper and lower parts. Although the voice was weak, the vocal resonance was much increased, and there was very distinct bronchophony over the inferior two-thirds. There was no friction sound audible. The left side was very resonant, and, with the exception of puerile respiration, and some subcrepitous râle, inferiorly, presented nothing unusual. During the next three days, he became rapidly weaker; the cough and dyspnoea increased; he could speak only in monosyllables; the respirations rose to 68, and the pulse to 140; and he expectorated daily about two pints of thin bloody sputa, which had a strong gangrenous odour, and latterly flowed in an almost continuous stream from the mouth. On the 29th, he became typhoid, had hiccup and slight delirium, and died in the evening.

#### *Sectio Cadaveris.*

Along with Dr Alison, who had seen the case with me during the last two days, I made a post-mortem examination of the body 22 hours after death. The features were much collapsed; there was some yellowness of the skin; and a copious discharge of thin brown

fluid from the mouth and nostrils. Percussion of the chest elicited the same sounds as during the last days of life.

On opening the *thorax*, the right lung, with the exception of the lower part of the anterior border, was found firmly adherent to the walls. The adhesions were short, dense, and of a white colour. The lung was removed without laceration; it was somewhat diminished in bulk, of a dark red colour, and had a pulpy feel. The apex was occupied by a closed cavity, the size of a small orange, which was distended with a brown dirty-looking fluid of the consistence of cream, and having a most intense gangrenous odour. The wall of this cavity approached the pleura superiorly; its inner surface was very irregular, presenting numerous shreds of disorganised pulmonary tissue. At the middle of the lung posteriorly, and about half an inch from the surface, there was another cavity, the size of a walnut, lined with a dense gray-coloured membrane, one line in thickness, and broken up in several places; it was partially filled with a dirty-coloured fluid, and opened directly into a bronchial tube, the size of a crow quill, at the other extremity of which the foreign body was found at a future stage of the dissection. In the neighbourhood of this cavity, and throughout the whole of the inferior and posterior parts, the lung was riddled with numerous small cavities, varying in size from that of a hazel nut to that of a pea. Some of these were closed and filled with a fluid similar to that found in the one at the apex; others were nearly empty, more or less anfractuous, and communicated freely with the bronchial tubes; the walls of some were formed of a thick dense membrane, those of others were soft and ragged. The middle part of the anterior, and a small portion of the inferior, border were in a state of gray hepatisation, and were the only parts free from cavities. On laying open the *right* bronchus, a small piece of bone was found at the bifurcation of the middle primary division; it was lying almost loose, and came away without any force being used; it was quite clean, and bore a strong resemblance to part of a vertebra of a small animal, being of an irregular elongated form, and presenting several sharp spicula (Fig. A). The mucous membrane at the part was thickened, but quite free from ulceration, and not more vascular than that of the other bronchi. The *trachea* and the *bronchi* of both lungs were stained of a dark gray colour, but otherwise presented nothing abnormal. In the left *pleura* there were three or four ounces of clear serum. The lung was healthy, except a small portion at the inferior border which was hepatised, and studded with small, gray, indurated nodules, the size of corn-pickles. These consisted, as ascertained by the microscope, of accumulations of altered epithelium, with much granular fatty matter. The apex of the lung was free of deposit, and there was no tubercle in any part. The *bronchial glands*, especially those on the right side, were greatly hypertrophied, several of them being as large as pigeons' eggs; they contained no foreign matter. The *heart* was of the normal size; its muscular and valvular structures were healthy;

and all the cavities contained both firm decolorised and dark loose clots. The *blood*, examined under the microscope, presented the red and white corpuscles in the usual proportions. The *abdominal viscera* were in all respects normal.

The fluid from the abscess at the apex of the right lung, on being examined under the microscope, was found to contain small shreds of fibrous tissue, broken down pus globules, and a large number of crystals of the triple phosphate and of the urate of ammonia, Fig. B.



A, Piece of bone, natural size.

B, Fluid from abscess, magnified 350 diameters, containing shreds of tissue (a); broken down pus globules (b); crystals of triple phosphate and urate of ammonia.

*Remarks*—This case presents a very good illustration of the length of time during which a foreign body may remain in the air-passages without giving rise to any urgent symptoms. There can be no doubt that the freedom from distress was due to the bone having, almost from the first, become fixed in the bronchus, and having remained in the same position till death; had it either been fixed in the larynx or trachea, or remained loose in the passages, it must have given rise to a train of symptoms quite different from that manifested throughout the case. For three months, the only symptom of there being anything amiss in the chest, was the occasional occurrence of slight cough and wheezing, resulting, most probably, simply from irritation, and not from the body becoming loose in the passages.

At the distance even of fifteen months, so slight was the disturbance of the respiration, and so doubtful the evidence of the existence of a foreign body, that Sir B. Brodie—the patient informed me—expressed his opinion that there was nothing in the air-passages, and considered the case one of chronic cough, from which recovery might soon take place. A similar opinion seems to have been entertained of the case when under treatment in St George's Hospital, as no proposal was ever made to the patient to have an operation performed; nor, indeed, would such have been justifiable unless, on physical examination, undoubted evidence of obstruction in the chest had been discovered. It was not till a month

after he had left London, and sixteen months after the accident, that the sputa became bloody and foetid; and this seems to have been the period at which the disorganising process first commenced in the lung. The importance, in such cases, of having recourse to the stethoscope, and of not trusting to the disappearance even of every *symptom* of the presence of a foreign body soon after its supposed entrance into the air-passages, is still better illustrated by a case mentioned by M. Louis, in which, after the first few minutes, the patient for a whole twelvemonth had not a single bad symptom; at the end of that time the foreign body—a cherry stone—was expectorated; a copious purulent expectoration followed; and the patient died exhausted in three days.

The first opportunity I had of examining Neal, was in the latter end of 1848, four years after the accident. The history of the case then was such as to excite a suspicion that a foreign body had entered the air-passages at the time supposed by the patient, and had remained there ever since; while the physical examination of the chest established the existence of an open cavity of some extent near the middle of the lung. Such being the state of matters, all operative interference seemed to be contra-indicated; as, even had all doubts of the presence of a foreign body been removed, the probability was, that it would be lying in the cavity, and therefore beyond the reach of instruments. I am not aware of any case of recovery, whether spontaneous or by operation, being on record in which more than two years had elapsed from the date of the accident. In those which have lasted longer, death took place sooner or later from disorganisation of one or both lungs, either from tuberculosis or gangrene; and the fatal issue would seem to have been equally certain, whether the foreign body remained in the lung or was expelled by the efforts of nature. When the latter event has taken place, as it has done ten or even seventeen years after the accident, the case has either terminated suddenly, or the morbid process has gone on unchecked by the removal of its original cause, the disorganisation which had taken place before the expulsion having been so great as to prevent recovery. Had the bone been removed in this case, even four years after the accident, there is good reason to believe that recovery would have taken place, as only a small portion of the lung was seriously implicated, the general symptoms mild, and the constitution of the patient but little affected.

A point of considerable pathological interest in the case, is the absence of all ulceration at the part where the foreign body was impacted; the only change discovered, after careful examination, being thickening of the mucous membrane. That the bone must have remained all along in the place where it was found, is almost certain; otherwise, from its comparatively small size and its spongy texture, it would have been frequently projected up to the larynx, giving rise to paroxysms of cough, none of which ever occurred; besides, there was no thickening of the mucous membrane in any other part; and



the cavity which was first formed opened directly into the tube at the mouth of which the bone lay. Another point of pathological interest, was the existence in the fluid from the gangrenous abscess at the apex of the lung of large quantities of the crystals of the triple phosphate and urate of ammonia, formed, no doubt, on the spot by the destruction and decomposition of the surrounding tissue. Their form was exactly the same as that in which they exist in the urine.

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ARTICLE VI.—*On a New Mode of Remedying Cartilaginous Bodies in the Knee-Joint.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

THE risk of serious and even fatal consequences which attends the removal of cartilaginous bodies from the knee-joint by external incision, led me to think of another method by which the object might be more safely accomplished. This was making a subcutaneous opening in the synovial membrane, through which the body having been passed might be either allowed to remain embedded in the cellular texture, or removed at a subsequent period without disturbance of the articular textures. On the 1st of February 1841, this proposal was carried into effect on a patient, who had been admitted into the hospital under my care, for relief from a cartilage of considerable size, and with complete success, as will appear from the account published in this Journal for the following month (March). The same idea had occurred to a French provincial surgeon, M. Goyrand, who published an instance of its application in practice. The first notice of this case, which was quite unknown to me, did not appear in the English journals until April.

Although, as frequently happens with procedures not destined to maintain a permanent reputation, this operation succeeded perfectly on the first occasion of its performance, it was soon found to labour under a very serious objection. This was from the difficulty, or rather impracticability, frequently experienced in effecting displacement of the cartilage through a subcutaneous aperture of the synovial membrane. In order to remedy this defect, Mr Liston proposed to introduce the knife twice, and make a double incision. But, while the difficulty was thus very imperfectly removed, the advantage of the operation was in a great measure sacrificed by the complication of procedure.

Having become satisfied that displacement by subcutaneous incision could not be depended on for the remedy of this disease, I endeavoured to think of some more effectual mode of affording relief, and soon afterwards met with a case which has, I hope, ultimately led to the accomplishment of this object. The patient came from Yorkshire, to get a cartilage removed from the knee-joint. Upon

examination, I found that the body, which was of more than average size, lay over the external condyle, and learnt that, although previously very troublesome, it had occasioned no uneasiness since occupying this position, which it had done without any change since a short while before he left home. I was still urged to perform the operation, but declined, in the hope that there would be no further occasion for it, and therefore ordered merely a blister to be applied from time to time. Some months afterwards the patient informed me that the cartilage kept its place, had decreased in size, and given him no uneasiness whatever.

In the next case that presented itself, I endeavoured to produce the effect of this natural process, by thrusting pins and needles through the cartilaginous body, together with the superjacent textures, so as to prevent any motion for days and weeks; but without success, as the restraint was no sooner withdrawn, than mobility, with its attendant consequences, returned. I then tried to effect displacement by subcutaneous incision, also without success; and, finally, in compliance with the urgent desire of the patient, removed the body by external incision. As inflammation and suppuration of the joint followed, notwithstanding every practicable precaution, I felt more than ever the importance of discovering a more safe substitute for this procedure; and upon the next occasion adopted the following method.

The cartilaginous body having been urged to the outer side of the joint, and pressed down over the external condyle of the thigh bones as close as possible to the head of the fibula, and kept in this position by the finger of the left hand, I introduced a tenotomy knife obliquely through the integuments and subjacent textures, until it reached the cartilage, into the substance of which I cut freely, so as to make sure of fully dividing the synovial membrane. A compress and bandage were then applied, so as to maintain the body in its place, and leave room for the application of a blister on the surface. Adhesion was thus accomplished, and in the course of a short time followed by a distinct diminution in the size of the cartilage, which occasioned no further trouble. Two other bodies, which existed in the same joint, were treated after the same fashion, and with the same result. My colleague in the hospital, Dr Mackenzie, has employed this method in one case with complete success; and I may here give the particulars of another, which lately occurred in my own practice.

“Alexander M'Pherson, æt. 27, shepherd, Glenorchy, admitted September 1st, 1852. About five weeks before admission, he felt himself suddenly ‘crippled’ as he was walking; this occurred a second and a third time, and was on each occasion attended with intense pain in the knee-joint. He examined the joint carefully after one of these sudden fits of lameness, and at length discovered a moveable body. He was advised to go to Edinburgh, to obtain the advice of Professor Syme.

“On admission, the loose cartilage can be easily felt, and appears

to be about the size of a large garden bean. After he has walked a few steps, it usually takes up its position between the inner condyle of the thigh bone and the internal tuberosity of the tibia. When he lies down, the moveable body slips up the inner side of the synovial membrane, crosses the joint, and may then be obscurely felt about an inch and a half above the outer edge of the patella.

"September 13th.—Mr Syme having felt the cartilage in the position last described, pushed it down the external side of the joint, and pressed it firmly against the synovial membrane, close to the head of the fibula. The knee being bent, Mr Syme entered a straight tenotomy knife through the integuments, and made an incision into the surface of the cartilage, which was immediately afterwards fixed by a strip of adhesive plaster. A blister, about the size of a crown piece, was then applied over the seat of the cartilage.

"September 14th.—The blister has risen; there is not the slightest degree of constitutional disturbance.

"September 16th.—Patient continues well.

"September 23d.—Cartilage seems fixed; he now moves his leg a little.

"September 29th.—The moveable body cannot be felt.

"Discharged cured, October 18th, 1852."

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ARTICLE VII.—*Urethrotomy: A Page in the History of Surgery.*  
By CRITO HYPERCRITICUS, M.D.

WHEN I was in Paris many years ago, at a time when the medical world rung with the discoveries of Magendie relative to venous absorption and the functions of the cerebral nerves, I had the luck to be present at a dialogue, when two lively Frenchmen thus settled the merits of their now illustrious countryman:—

A. Que pensez-vous, Monsieur B., de toutes ces recherches curieuses?

B. Monsieur! je n'en pense rien du tout. Je ne me fie ni à ce Magendie, ni à ses prétendues découvertes sur l'absorption par les veines.

A. Ne croyez-vous pas qu'il y ait de belles choses dans ses travaux?

B. Du tout! Du tout, Monsieur! C'est un menteur, cet homme-là!

A. Après tout, je crains que vous n'ayez raison? Et pourtant on trouve quelques petits grains de vérité dans ses faits.

B. C'est un exécrationnable gueux.

A. D'accord. Mais néanmoins, les veines absorbent,—n'est-ce pas?

B. Eh, bien! soit, si vous le voulez, Monsieur! D'ailleurs, tout cela était bien connu d'Hippocrate.

Since that occasion I have witnessed so many similar incidents in medical history, that I begin to think there can be no such thing as a real discovery in medicine, unless it shall be proved to the general satisfaction, first, that the discoverer has no respect for truth, and next, that he has none for his neighbour's property.

More profound philosophers, advancing a step beyond this, have come to consider it quite enough to set up for ever any *ism* in medical science or practice, however improbable, if they can point to a strong and well-grounded conviction among professional men, that it is simply a lie. My own contemplations, however, have not yet raised me to this pitch of transcendentalism. I still hold by the indefeasible necessity of the second condition also. But, to my judgment, that clenches the argument. Accordingly, I do think one might now almost believe in Homœopathy, or Sumbul, or Pulvermacher's chain, if they could only be so much as suspected to have been stolen from Hippocrates or Avicenna.

I have been lately led to revive these ruminations by the history of the new invention of urethrotomy for the cure of stricture;—in which I find a singular illustration of this my favourite theory.

The facts relative to this important operation were pretty well known at one time. But they have lately been turned topsy-turvy in a court of law in the shape of a jury trial; and every one knows what that means in Scotland; nor will any body wonder that the facts are now not very easy to come at.

Some people have been dissatisfied even with the verdict given on the occasion. Now really this does seem surprising. Those who have got to pay for so costly an entertainment, may well be mortified, that the ingenuity, so successfully exercised to vulgarise the jury, and suppress the facts, was followed by so incommensurate a result. I may say I was myself surprised at that. For I confess my humble admiration no less of the tact displayed to entrap twelve simple-minded citizens, than of the shrewdness, or good-luck, which enabled such men to shun the snare laid for them. They gave their verdict for the defender. For they unaccountably discovered, through the mists summoned around them by legal necromancy, that there was an offender as well as a defender; that the defender was not the offender; and that he was a defender *de facto* long before the pursuer made him so *in foro*.—"Voilà tout!"

But the facts, I say, have been woefully suppressed and twisted in consequence. Now, this is a pity in the circumstances. It cannot be doubted that sundry very likely young men will write for the next Jacksonian prize of the English College of Surgeons, who, it is well known, have announced "The Pathology and Treatment of Stricture of the Urethra," for next year's subject. The natural impulse of pugnacious youth will turn the attention of these ingenuous inquirers to the historical prolegomena. And, not to disguise the truth, there seems little room for discussion now on any other branch of their theme. But my

fear is, that if, according to the most approved construction of such historical edifices, they be contented with consulting indexes for information, they will search in vain for what they are in want of, and thus surgery may lose an important page of its history. And if, on the other hand, they honestly read up for the purpose, they will find more than they want, and much more than they can digest, or than the medical world will tolerate. It may, therefore, save them some trouble, and their readers too, if one, who really knows the state of the facts, shall now present them categorically.

But there is another reason for bestirring myself in this matter. It has been whispered, that a certain coroner has signified his pleasure, that he shall sit on the body of any man within his jurisdiction, who may dare to submit to the operation, and be so inconsiderate as to die of it. I have my suspicions, however, that the medical profession in general will be disposed to save the worthy gentleman all that trouble. If they have no great faith in the capability of a Scotch jury-court to approfond a question of this kind,—they have not much more in an English coroner's inquest,—they would probably have still less in an Irish jury, whether coronary or assizal,—but they would have least of all in a court of twelve cockney tradesmen, presided over by his coronership, after previously intimating his sovereign will to hold one on the first favourable opportunity. In short, surgeons at large would, with great submission, judge rather for themselves, without his compassionate and dispassionate assistance.

As it is of great moment to separate carefully the facts from the philosophy of this subject, and indeed my sole anxiety is about the former, and I make the reader freely welcome to put the latter to any noble or ignoble use which may best suit his taste or his conscience,—let us make a fresh start, with a proper interval.

The operation of urethrotomy, or cutting into the urethra for stricture, had been long practised, to relieve a class of cases of no infrequent occurrence, and a species of bodily and mental distress seldom surpassed for severity, continuity, and endurance. But with dubious results. For it was often unsuccessful in effecting relief; and when successful, this was too often attained only at the cost of relief from all sublunary suffering. The cause of failure was, that, as the surgeon believed there was no practicable passage through the urethra, he dug for one,—sometimes hitting it, sometimes making it, sometimes neither hitting nor making it, and in most cases so dabbling and scratching a rather ticklish organ, that no consequences, however serious, could excite any wonder.

At last it occurred to a member of the profession to reflect, that, as there was no urethral stricture in this world, however curiously constructed by nature and art together, that had not a passage for urine,—the case of obliteration from violence being of course excluded,

—the operation might be much improved by the simple device of passing into the bladder a grooved staff, and entering and slitting the urethra with its help, instead of plunging at random as formerly among metamorphosed and undistinguishable tissues. A very simple matter to constitute a discovery, and make all the stir which followed! But it seems to be the lot of all discoveries in medicine, which are good for anything, that they shall seem to be simple enough to find out, when they are once found; and they must make a stir, if they stand good, because they can scarce fail to run counter in that case to some strong mortal prejudice, or some mere human self-interest.

During the eight years which have elapsed since the operation was first practised, the proposer himself has applied it in nearly seventy cases,—a pretty broad basis of induction, one would think. In not a single instance is it alleged, or known, that the patient has perished, either from the disease or the doctor. And it may be said, with very little fear of contradiction, that a tolerably sharp look-out has been kept on all hands by some folks for any such personage.

A large proportion have been radically and permanently cured. What that proportion may be, I grieve, for the sake of medical statistics,—for which I have a profound respect,—that I cannot make out. If people, after recovering from a state of bed-ridden helplessness, will be so confident and overlusty as to spread themselves over India, and America, and Caffreland, and the North-West Passage, and where not,—there is of course no help for the statistician: an exact ratio becomes an impossible quantity.

It has been averred, that one or two have suffered after the operation from “shock,” or from hemorrhage. It has also been alleged, that one or two others are not only no better, but have been made for life miserable by perineal abscesses and compound perineal fistulæ,—all proved to have been the result of the operation by strong asseveration, and some very necessary, elegant, and modest engravings. But hereby hangs a tale, to be told hereafter.

It has been also insinuated, that some have relapsed. Cases of the kind have even been reported to myself, and I was therefore impatient to come at the truth respecting them. For this end, I first thought of writing to the proposer of the operation in the guise of a friend, colouring to my own liking what might be thus extracted, and using the information to suit my own views, and my estimate of the public taste at the moment. Then it occurred to me, that I might advance the cause of truth with more certainty, and treat the case with more unction, were I to publish a pamphlet full of impertinence, and illustrated by abominable pictures. But at last, being unable to overcome a weakness which grows with my years, and that leads me to look in preference at the fair side of nature, and to place some faith in human virtue, I suddenly recollected I had been informed somewhere, that Mr Syme had enjoyed a character for unsullied truth from boyhood upwards,—until at all



events he, in an evil hour, as it seems, for himself, devised this operation; and so I thought the most feasible measure would be to address him just as one honest man might another, and ask him what had been his own experience. I received a courteous answer, in which, to my great relief, the writer states that there is some ground for the report.

"In a case," says he, "which was sent to me in June 1849 by Mr Courtenay from London, and which has been made the most of by him and other opponents of the new operation, the patient described himself, in a letter to me in August, as already so well that his usual medical attendant declared he was ready to recommend his life for assurance if required. The improvement, however, did not prove altogether permanent. Before the end of the year the patient complained of some threatenings, attributable to nervous anxiety. In the following February he had an attack of retention similar to those he had previously experienced. Still, so far as could be gathered from himself, his general health has continued better; and the disease, though not removed, has been less troublesome than before the operation. 'Most thankful,' writes he the last time I heard from him in April 1851, 'am I to be able to say, I certainly am better than before I went to Edinburgh.'

"I think the imperfect result of this case, and some others in the early part of my practice, may be attributed to one or both of the following circumstances,—the importance of which I had not ascertained at that time from experience: viz., *first*, that the strictured part of the urethra may not have been divided freely enough; and *secondly*, that the precaution of occasionally passing a bougie, was not observed long enough. When I introduced an instrument some days after the operation, the patient remarked, that it 'jumped' at the seat of contraction, instead of passing smoothly along the whole urethra, as I now always find it do; so that it must have encountered an appreciable, though slight resistance, which a more free division of the urethra might have removed. I supposed at the time that the incision would be sufficient to obviate any tendency to contraction in future; and I therefore advised the bougie to be laid aside so soon as the wound of the mucous membrane could be considered as fairly healed. I have been long aware, however, of the possibility of unfavourable results from these two errors; but, among the cases I have operated on during the last two years, I am not acquainted with a single instance of relapse."

I have said that I was much relieved to learn from the best of all authority, that the operation had not succeeded from the very beginning perfectly and permanently in every instance. For in truth I had scarcely ever heard of any good invention that was perfect from the first; and, consequently, had this one been considered such, I should have been sorely puzzled what to think. But here we have clear proof that it possesses one other element of excellence: it was capable of improvement, and has been already improved.

The condition of safety in the operation is that a staff shall be first passed into the bladder. But it has been maintained by many, that sometimes a staff cannot be passed; and they naturally ask, what is to become of the operation then? The answer is that a surgeon, who makes this objection from personal experience, had better not attempt the operation. Mr Syme has given his assurance, that he has never met with an impassable stricture since he became persuaded that all are permeable; and every one ought to know the significance of that fact, who is acquainted with the opportunities of that gentleman's professional life. He does not maintain, however, that every stricture is patent to every surgeon; but merely, that there is no such thing as a stricture "*absoluté et naturaliter*" impermeable; that, though possibly impermeable "*quoad chirurgum*," it never is so "*quoad chirurgiam*." Neither does he say, as some foolishly pretend, that it is always easy to pass an instrument through a stricture: on the contrary, he acknowledges that it is often difficult, and sometimes foils for a time the most skilful. But he maintains that with patience, caution, due skill, and frequent trial, the worst stricture will be overcome at last. His opponents reply:—let him wait a little, and he will assuredly encounter an impermeable stricture. How long, then, must he wait? Seventy of the worst possible cases, many of which had previously brought the staffs of other surgeons to a dead halt, constitute, as I have said, a reasonably broad basis of induction. Mr Syme, too, has invited the sceptical, but hitherto in vain, to send him a stricture which they think he cannot surmount. Is the decision of the practical question to be put off until that gentleman shall be baffled? If it must be so, then so be it. But in the mean time, if there be a doubt whether Mr Syme ought to speak with confidence on this point until he shall acquire more experience, there can be no doubt that his opponents had better not speak at all about it till they acquire greater skill. They have yet a lesson or two to take in the art of handling their staff.

A corollary from this conclusion supplies the only acceptable interpretation of a startling statement made by some, probably in the excitement and turmoil of controversy. "We," say they, "cure all permeable strictures by dilatation: then why slit them?" There is but one way of understanding, without disparagement to the veracity of those parties, a proposition so directly opposed to all sober surgical experience. To them all strictures are dilatable which they find permeable,—merely because they find impermeable all strictures which are not dilatable.

No fact, connected with the operation, has occasioned me more perplexity than the circumstance, that, while not a single fatal case, or even any seriously inconvenient consequence, seems to have arisen from it in the experience of its proposer, both the one and the other are acknowledged to have occurred in the hands of some of those who have adopted it. Here, again, I thought it best to

apply to Mr Syme himself for an explanation ; who has thus replied :—

“ Having now employed the operation in nearly seventy cases, without a single instance of hemorrhage or death, I feel entitled to say, that the procedure is free from danger when properly executed. I am quite aware that the groove of the conductor may be missed ; and that the knife, if pushed beyond it, may readily cut the artery of the bulb, so as to occasion serious hemorrhage. I know, too, that if random incisions are made deeply into the perineum, while the stricture is not at all or imperfectly divided, the same disastrous consequences may ensue as after the old operation. But it is nowise necessary, that any man should undertake the operation who is apt to commit such awkward mistakes. It is true also, that, if the after-treatment be improperly conducted, other obstacles may thus oppose success. For instance, there was lately, and there may be still, in one of the London hospitals, a poor creature disturbing the ward with his moans, and poisoning it with ammoniacal effluvium, in consequence of a flexible catheter being retained for weeks after the operation, except when it was changed at distant intervals, with difficulty, from the thick encrustation of phosphatic deposit. But is the operation itself to be held accountable for the consequences of such defiance of the dictates of common sense and surgical principles ? ”

These are the professional facts of my subject. And now for personal matters.

The world is not always at first sufficiently grateful for a useful invention. This is apt to happen, if the invention necessarily wounds people's self-complacency. Such has been the fate of Mr Syme's operation. Its initial step requires a degree of skill, which some who call themselves surgeons have suddenly discovered that they do not possess. There is no other way of accounting rationally for its unwelcome reception.

For more than twenty years past Mr Syme has enjoyed an unusual amount of public confidence as a surgeon, and equally uncommon opportunities of self-instruction in the chair of Clinical Surgery in the University of Edinburgh, and in the office of acting surgeon of the Edinburgh Infirmary. During that period he has contributed many acknowledged improvements to operative and medical surgery. It might have been expected then, that any further addition from the same source would be received by the surgical profession, perhaps with some favour, but surely with common civility. But what has been in this respect the fate of his proposal for the radical cure of rebellious strictures.

In the *Monthly Medical Journal* for 1844, and again in 1847, in a collection re-published by him of the more important of his “ contributions to the Pathology and Practice of Surgery,” he gave an account of a single case in which his new operation was performed for the first time, and with complete success, though in somewhat

unpromising circumstances; and after farther experience, he gave a fuller account of it and other cases in 1849, in a pamphlet "On Stricture of the Urethra and Fistula in Perineo."

It is particularly necessary to observe that, neither in the matter nor in the manner of these publications, is there the slightest visible ground of offence given to any individual man or to mankind in general. The older operation by external incision, without first passing an instrument through the stricture, is indeed condemned, but by pure surgical argument alone. Nevertheless, the treatise was not long published before it was assailed on all sides with unmeasured bitterness.

The subject was taken up in the medical societies of London, where "every puny whipster" drew his sword in the argument, and thrust with right good will at the operation and the author of it. Then a Mr Courtenay, somewhat noted for his advertisements in the newspapers and wrappers of periodicals, appeared in the field, first sending a plausible letter, as well as one of his own incurable patients, to Mr Syme, and then turning his back on that gentleman, abusing his operation, and dragging his name with his own through the mire of one of his pamphlets. Next up starts Mr Gay, the colleague of Mr Wakley, junior; and in the paternal journal he is allowed to charge Mr Syme with bad faith towards the medical profession and the public, by wilfully misrepresenting and concealing the true results of his cases. As I could not, with my utmost exertion of ingenuity, understand how this very unusual charge was made out, I ventured again to apply to Mr Syme for the true state of the facts;—which will appear from the following reply:—

"The patient sent by Mr Courtenay was operated on in Edinburgh in the end of June 1849. Before the middle of July he was so well as to return home to the south-west of England, with every apparent prospect of thorough recovery. Immediately after this long journey he suffered from irritation in one of his testicles; on account of which I sent him before the close of July a comforting note, with the assurance that he would nevertheless be soon well again. Accordingly, on August 1st, the patient wrote to me in glowing language how prosperously his case went on, and mentioned, among other things, that 'a few days since a medical referee of an assurance society, Dr ———, formerly one of your pupils, voluntarily remarked that he should not now have the slightest hesitation in recommending my life for assurance, when in May last he did not consider it worth a year's purchase.' In November of the same year, in my pamphlet on 'Stricture of the Urethra and Fistula in Perineo,' I described this case, among others, as having been cured; which it is evident I had good reason at that time for doing. Before the end of the year, however, the cure proved not to be permanent or complete. This I have adverted to in a previous communication to you, in which I have likewise mentioned the probable cause, and suggested a precaution for avoiding such imperfect success in future. When I last

heard of this patient in April 1851, he continued to be in a materially improved state,—not quite well, but in comfort, when he passed the catheter every five or six weeks.”

Now this was the case which Mr Gay, in 1851, alleges that Mr Syme falsely represented, in his pamphlet of 1849, as having been cured; and he grounds the charge on that gentleman's letter of comfort to the patient in July,—taking no account of the patient's subsequent condition at the time when Mr Syme's statement was made.

By-and-by Mr Gay and Mr Courtenay were joined in the onset by others; but especially by one, without whom the *mêlée* would not have been complete, and who finally took the whole burden of it on his own shoulders.

Mr ex-professor Lizars had hung upon the professional path of Mr Syme throughout his whole progress in life; seldom missing an opportunity of snarling and snapping at his heels; but seldom escaping with a sound skin; and on two occasions suffering severely from the administrators of the law being attracted to the spot by his noise. For some time after this sharp lesson, Mr Lizars left off his old courses. But at length, lured in an evil hour by the treatise on Stricture of the Urethra and Perineal Fistula, he again rushed furiously in pursuit of its author; and being soon joined in the chase by Mr Courtenay, Mr Gay, and others from the London pack, he followed it up in full cry, forgetful of the past, regardless of the consequences, and with no farther precaution than the aid of a helpmate to support him in the worry. In the last three months of 1850, Mr Lizars and Dr Müller, reported to be at the time his assistant in practice, published sundry statements imputing bad faith to Mr Syme, and denying the success of his cases, and the utility, and even safety of his operation. Mr Syme in reply referred simply to the fact, that Mr Lizars had previously been convicted in a law court of libelling him by calumnious professional charges, and had anticipated a second similar conviction by an ample retraction and apology; and he begged the profession, therefore, to judge as to the respective credibility of the parties in the new controversy. Afterwards, in February and March 1851, Mr Lizars published two editions—the latter an octavo of 130 pages—of a work of his own on stricture and perineal fistula,—the evident and main, if not sole, object of which was to insult and injure Mr Syme. This work consists of 3 pages of dedication, 12 of preface, 41 of text, 88 of appendix of documents, and 11 engravings. In every one of these quarters, even in his plates, the intention of the author is transparent. It has been my lot to peruse more controversial pamphlets in my time than it is agreeable to reckon up. But I can safely say, that one so overcharged with a plethora of personalities and unprofessional ethics, it has not been my lot to read for many a day.

In this remarkable production, the writer avers that the perineal section has “totally failed to accomplish a cure,” (xi.); that “every



case is a failure," (77); that "every case is, and must be, a failure," (19); that "it is a painful, hazardous, and therefore unnecessary substitute" for other methods, (20, 48, 113); "an appalling operation," (19); "an awful operation," (26); "a dangerous experiment," (xv.); and one which may be "justly stigmatised as a great surgical blunder," (xi.) He maintains he has proof that some of Mr Syme's patients, contrary to his assurances, have narrowly escaped death from excessive hemorrhage,—describes cases of his own, which, if treated by the perineal section, must have fallen victims to hemorrhage,—and in noticing the cases of others, said by him to have actually died, writes so that one not well informed might suppose these had occurred to Mr Syme. He alleges that sundry other terrible consequences, which it is superfluous to specify here, have resulted from the operation, both in Mr Syme's hands and in those of other surgeons; and he contrives that the reader shall confound these under one category, unless he have the penetration to discover the incomprehensible joke, that all other surgeons who practise the operation are "Perineal Sectionists," but Mr Syme—a "Pure Perineal Sectionist." He perpetually sneers at that gentleman as a "Pure Perineal Sectionist" [*passim*],—as one of the "sanguinary surgeons" (125),—as having been guilty of the "reckless use of the knife,"—of "self-conceit" (111),—of an "unparalleled puff" (93),—and of "boastful triumph," (21); of taking, "with his usual candour, no notice of the event" (103),—of having, "in relating the particulars of a case, given so much of its history as appeared to confirm his assertions, and suppressed parts which went to show that . . . his conclusions and opinions were erroneous" (125),—of practising "evasion, and temporary suppression of facts" (xiii.),—and "a suppression of truth which every honest man would condemn" (61). Having thus exhausted his own stock of hard words, and finding Shakspeare a convenient help to his natural backwardness in railing, he tells of Mr Syme, that "conceit in weakest bodies strongest works" (93),—that he has erected a "baseless fabric of visionary cures" (119),—that he has committed "the lie with circumstance" (117),—that "he's a great quarreller, and but that he hath the gift of a coward, . . . would quickly have the gift of a grave" (117); and in conclusion, he proclaims that, "from this day forth, I'll use you for my mirth, yea, for my laughter, when you are waspish" (119).

It is true that much of this savoury mess of spicy sayings is not the produce of his own brain or pen, but consists of quotations in prose and verse from Shakspeare, Courtenay, and Gay; and it is possible that he may have comforted himself with the notion, that it is safer to speak evil of one's neighbour in that shape. But if so, Mr Lizars has yet to learn the meaning of "a distinction without a difference;" and that written language, whether poetical or prosaic, is, according to the usage of gentlemen, and in the eye of the law, homologated by him who republishes it with approbation, whether expressed or implied.



Then Mr Lizars, aware that pictorial illustrations are a great help to the uninstructed mind, appends eleven engravings, nine of them old, two of them new, and all of them superfluous, — representing chiefly entire figures of entirely nude gentlemen, in postures for which there is no authority in classic marble or bronze. As I belong to the family of the Hypercritici, I may possibly be a trifle too fastidious; but I do confess I would much rather not be immortalised in connection with these unmitigated likenesses of things unmentionable. I suspect it is well for the originals, Mr Francis Rodger, and Mr Joseph Antonio—if they really did ever expose themselves in the attitudes here represented—that the police did not surprise them in the fact. As for Mr Lizars, so far from having any tender scruples on the subject, he delights in his pictures. That one, says he, is a “clever sketch;” “this is a beautiful drawing.” And the two gems among them are thus titled, in his own joyous language:—“This plate represents the deplorable condition of Joseph Antonio after he had undergone the perineal section.” “Fig. 2 of this plate” [an entire male, exhibiting with a cheerful countenance seven bloody scores on his perineum and buttocks] “represents the lamentable state of Francis Rodger, who had undergone the operation of perineal section in the Royal Infirmary.” Two circumstances about these *gravures de luxe* deserve attention. In the first place, one of them represents a case that never was under Mr Syme’s care; the other, a man who never had stricture at all. And, secondly, it is impossible to discover for what scientific or practical purpose they have been introduced. But one purpose they may serve, and they have served, if I am not misinformed: a single glance of them will go far to cure any unfortunate with impermeable stricture of all ambition to get into the hands of any “Perineal Sectionist, Pure” or Impure.

What was Mr Syme to do in this storm of defamatory criticism? Controversy was out of the question; was he therefore to bend to the blast? The Jury Court was evidently again open to him; but, having already been twice reduced to protect himself in this way against the same individual, was he called on to indulge that gentleman with a third correction of the same kind? Silence, however, was liable to misconstruction, and was misconstrued by many. He therefore determined simply to explain the cause of his silence,—which was, that the statements of “Mr Lizars, and his assistant, Dr Mullar,” relative to the alleged bad effects of his operation, had been “declared by him to be all utterly devoid of truth;” and that he had regarded Mr Lizars “as long placed beyond the pale of professional respect and courtesy.” This intimation was intended to appear in July 1851, as a letter in the “London Medical Gazette.” But it fell unluckily into the hands of an editor, who was so little conversant with his functions as to print the letter in a garbled form, without the writer’s consent or knowledge,—publishing along with it, as his reason, that he thought the suppressed passages libel-

ious and actionable. Mr Syme, convinced that they were not so, and that his letter was more than ever called for by the circumstances in which this blundering editor had placed him, had it printed in the "Monthly Medical Journal" for August following.

Mr Lizars and Dr Mullar thereupon each raised an action against him: the former, because Mr Syme had stated in print that he regarded Mr Lizars as beyond the pale of professional respect and courtesy; the latter, because he had been designated as the assistant of one who was so regarded, and his statements had been described as being utterly devoid of truth. The Court, however, found, that Dr Mullar had no ground of action; and the action of Mr Lizars being sent to trial, the Jury returned a verdict for the defender, Mr Syme. At the trial, Mr Lizars' case was so skilfully managed by his counsel, as to exclude from the evidence both the fact of that gentleman's prior attacks on Mr Syme's character, for which he had been twice compelled, by legal proceedings, to grant redress, and likewise even the pamphlet on "Stricture and Perineal Fistula," which was the cause of Mr Syme's statement being made public. Nevertheless, with even these serious defects in Mr Syme's case, the Jury decided in his favour, on the obvious ground, that Mr Syme's statement, which was called in question, stated merely the professional relation in which he stood to Mr Lizars, and simply as his reason for not entering into controversy with him.

It was surely to be expected by the members of the medical profession, that the whole personal part of the conflict concerning the new operation of urethrotomy for stricture was to be now at an end. But on the contrary, it has been pursued on Mr Lizars' behalf with more virulence and pertinacity than ever.

The numerous Edinburgh newspapers reported the trial fairly and without comment,—except one, which is understood to be under the influence of the law-agent on the losing side of the action. In this paper a violent leading article, barely, indeed, on the safe side of the law of libel, full of abuse of the defender's witnesses, and more especially of the jury,—which the pursuer so far chose for himself by refusing a Special one,—was followed by a letter of the same kidney from one who assumes the *nom de guerre* of M.D. At the same time the trial was published, and everywhere advertised, with extracts from this independent and most impartial journal. Then, a meeting was summoned in Edinburgh, at which Mr Lizars' friends were invited to consider the propriety of presenting him with a testimonial of condolence,—possibly, it was suggested, in the form of hard cash to defray his law expenses. Next, the resolution of this mummerly of a meeting, to get up some such testimonial, was circulated far and wide among the medical profession in town and country. After this, an obscure country newspaper was prevailed on somehow to add its howl to the very limited lamentation. Then of course ensued fresh advertisements of the trial, with extracts from the sentiments of this extraordinary champion. And at every step special care has

been taken to create a prejudice on the part of the medical profession and the public against Mr Syme's operation, and against Mr Syme himself;—which, indeed, seems to have been the only object really contemplated by these measures.

It is sufficiently obvious that some worthy people have thus laboured with a zeal which it would not be easy to surpass, in order to put down Mr Syme's operation for stricture as being useless and dangerous, and the proposer of it as a man of bad faith. This direct mode of attack not being thought enough, an effort has been finally made to turn the flank of the controversy, by maintaining that, after all, the operation is not his own, but was proposed long ago by the French surgeon Desault.—“D'ailleurs, tout cela était bien connu d'Hippocrate.”

This device is easily disposed of. The claim made for Mr Syme is the discovery, that there is no such thing as an impermeable stricture; that it thus becomes possible in all cases called impermeable,—as well as in all cases of ordinary retractile stricture which cannot be permanently cured by dilatation alone,—to lay open the urethra from the perineum by cutting upon a grooved staff passed through the stricture; that, when so performed, the operation is perfectly safe; and that it effects a complete and permanent cure. It surely cannot be pretended that the operation was ever performed in this shape, and with such assurances, before it was proposed by Mr Syme.

To conclude,—the practicability of the operation has been denied: its utility has been denied: its safety has been denied: the truthfulness of its proposer has been denied: its originality has been denied. It has undergone persecution, if the verb *To persecute* has been rightly defined by the great lexicographer,—“to pursue with malignity,—to pursue with repeated acts of vengeance or enmity.” Therefore, according to the theorem with which I set out, it is a new and important discovery.

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## Part Second.

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### REVIEWS.

*Great Artists and Great Anatomists: a Biographical and Philosophical Study.* By R. KNOX, M.D., F.R.S.E. Lond., Van Voorst, 1852.

THE object of this work, Dr Knox tells us in his preface, is threefold:—“1st, To establish the exact relation of descriptive anatomy

to the science of the animal organic world, as it now is, and as it once existed. In the life and labours of George Cuvier, as he views them, the author finds this relation fully made out. Before Cuvier appeared, geology was a farce—a subject of ridicule, cosmogony a myth, the history of creation a tissue of error and absurdities. 2d, To trace transcendental anatomy to its essence; and to show, in the life and labours of Geoffroy (St Hilaire), that the philosophy of the creation of animals is explicable only by descriptive anatomy. 3d, To discover, if possible, in the life and labours of the immortal artist who painted the ‘Cena,’ and of his great rivals, Angelo and Raphael, the true relation of descriptive anatomy to art.”

That the doctor has settled these, or any other question, no one who reads this book will have the hardihood to assert. It is hardly possible, from such a jumbled, confused, unconsecutive string of sentences, to gather what Dr Knox really would be at. We hardly ever encountered a book so entirely devoid of all method or arrangement.

The “Great Anatomists,” constituting more than one-half of it, though coming last in the title, come first in the text. He begins with a life of Cuvier, which contains more about Geoffroy than about Cuvier himself. He goes on to the life of Geoffroy, and that is as much about Cuvier and Napoleon as about Geoffroy. At p. 74, we have a section headed “Garden of Plants;” but it contains chiefly some adventures of Dr Knox in 1845-46, when “business of a scientific nature called him to Paris,” and the history of the Garden, so far as given at all, is thrust into the chapter headed “Life” (of Geoffroy). The doctor does not seem, when he began to write this book, to have determined upon his opinions any more definitely than on his arrangement. Thus, at p. 104, we have the expression, “Vertebrate animals,” as expressing animals having a cranium and vertebral column, denounced as “a most incorrect expression, calculated only to mislead;” whilst at p. 36 we are told, that to “Lamarck we owe the highly philosophical terms of Vertebrate and Avertebrate.”

We have in vain sought, in the biography of Geoffroy, for any elucidation of the philosophical part of the question. The chapter on the doctrine of the French and German transcendental anatomists, and many detached passages in other portions of the book, proclaim undoubtedly that the doctor is a partisan of the school of Lamarck, and of the author of the “Vestiges of Creation;” that he sneers, or rather laughs outright, at such a vulgar idea as a First Great Cause having anything to do with the existence of the living beings which have peopled or do people the world; that secondary causes alone would ever enter into a reasonable mind as giving rise to the diverse forms and characters of animate beings;—in short, the whole book, from beginning to end, is a tissue of rampant heterodoxy. It is, however, entirely devoid of argument; it is

little better than a self-conceited declaration of the doctor's sentiments. He seems to have out-Heroded Herod in putting forth his scepticism, apparently in the desire to make people stare at him, like the foolish folks who are occasionally seen parading themselves in kilts, where all persons of common sense have their *nates* covered with breeches, but who, provided they are only stared at, care little about making improper exposures.

It is but justice to the author, however, to say that he (p. 106) declines, as being inconsistent with a short biographical notice, the discussion at great length, of that theory with which his obnoxious tenets are more especially connected; nevertheless, when an author puts forth sentiments which he must well know are abhorrent to the greater proportion of his fellow-men, it is at least to be expected that he should show some argument in favour of his opinions, especially when they have been over and over again contested, and as often, we are persuaded, thoroughly refuted.

That we are justified in this denunciation of the style and tendency of Dr Knox's "Great Anatomists," we shall allow him to show in his own words:—

"Whoever looks attentively at the structure of man and animals, especially if aided by anatomical research, may readily observe that, generally speaking, all vertebrate animals are formed of organs strongly resembling each other, however remote from each other in the zoological scale the species or genera may be; that, in point of fact, it is obvious that all have been formed on one plan or scheme. This plan or scheme must have existed at their creation; the scheme or plan must be regulated by secondary laws, such as those of attraction, for none else are intelligible to the human mind. Aristotle, Leibnitz, Pascal, Newton, Harvey, all thinking men of all ages, will admit this statement to be essentially true."—P. 104.

"We know not, then, the causes of the specific and generic differences in animals, nor why such differences continue fixed for a period—the historic period, for example; they depend, no doubt, on secondary laws, which some future Newton may discover."—P. 109.

"These mysterious varieties [malformations in human structure] taught no lesson to our predecessors, wrapt up in practical utilitarianism; mythical cosmogonies; a contempt for truth; fatalists; admirers and believers in 'the best of all possible worlds.' *Lusus naturæ* was still the phrase: Bismillah! ejaculated the orientalist; wonderful are all thy works! responded the western fanatic. But the torch of science, quenched deeply in the mire of horrid ignorance, and, what is still worse, a learned pedantry, since the time of Aristotle, still glimmered—was not extinguished: It burst forth in the German school."—P. 59.

Again, at p. 55, we are told that Leibnitz, Newton, Pascal, and Bacon, "those lights of the earth," desiring to know whence and how originate the various forms which life assumes on this globe, "thought of none else but physical causes," "considering the question, as it really is, a natural, a physical question, a question of secondary laws," and to this we have appended the following highly characteristic note:—

"It was excellently observed by the talented MacCulloch (author of the 'Remarks on the Islands and Highlands of Scotland'), that the uneducated solve

every difficult moral, physical, or metaphysical problem by an appeal at once to a First Cause. The practice prevails also with the very learned—in words, as at Oxford and Cambridge, and thus extremes meet.”—P. 209.

It is hardly possible for any one who does not read Dr Knox's book, to form a just estimate of the value of the great transcendento-anatomical doctrine—the unity of organisation—which is the main subject of his laudations. It has “developed the great plan of the creation of living forms—the scheme of nature” (p. 12). “When sufficiently supported by facts and further researches, and philosophically formulated, it will take its place with the discovery of the laws of gravitation” (p. 17). It is “a theory the greatest ever offered to the consideration of man (p. 53). It was to revolutionise human knowledge.” Dr Knox gives us his creed on the subject in the following short sentence:—

“It was the opinion of Geoffroy, as we have seen it was that of Cuvier, that there had been but one creation. This, also, is my own opinion. I believe all animals to be descended from primitive forms of life, forming an integral part of the globe itself; and that the successive varieties of animals and plants, which the dissection of the strata of the earth clearly sets forth, is due to the occurrence of geological epochs, of the power of which we cannot form any true conception.”—P. 109.

How it should happen that “geological epochs, of the power of which we cannot form any true conception,” and which, if we follow Dr Knox, we are to suppose to be independent of that First Cause to whom “the uneducated refer their problems,” should have uniformly preserved one plan or scheme “in originating the various forms which life assumes on this globe,” does not, we must own, appear very clear to us. Whatever share these secondary causes may, as agencies, have in the great work, we should humbly conceive that their effects being on a uniform plan, rather would lead us to refer the whole result to that First Great Cause, to whom more than western fanatics will respond, “Wonderful are all thy works;” but whose working and power Dr Knox and his co-transcendentalists wish to ignore altogether.

Our space does not permit us to comment at length upon the biographies of Cuvier and Geoffroy. One thing, however, is clear, that—upon his own showing at least—no one should be more qualified for the task of writing them than Dr Knox, if intimacy with the subject of a biography be a good qualification in a biographer. “Cuvier is his “illustrious friend” (p. 19). Cuvier, Geoffroy and De Blanville are my “three friends,” with whom I had “been intimately acquainted” (p. 77). De Blanville is “my esteemed friend” (p. 82, etc.). Our readers will easily, therefore, imagine, what pain it must have cost our author to be forced by a dread regard for historic truth, to pen the following sentences as to his illustrious friend, “the immortal Cuvier.” They contain the doctor's explanation of a fact which seems to have gruelled him sorely amidst a superlative admiration for Cuvier, that the illustrious friend would have



nothing to do with the transcendentalists, and utterly repudiated their theories.

"It was this great law of transition, of metamorphosis, which alarmed Cuvier in his later years, although it ought not to have done so—Nature's transitions of organic life in time and circumstance ; the formation of all *living forms* from one living essence. His dislike to see in the living world, past and present, one animal instead of many, was caused simply by a dread of its touching that reputation which he knew the world based on his having proved the contrary."—P. 27.

"All honour, notwithstanding, be to his great name ; his dislike to transcendentalism was forced upon him. What passes for the views and theories of Cuvier in England do not belong to him. They emanate from a school with whom truth in science is of no moment. They emanate from men who are not strictly scientific, but who, like Philo-Judæus, Derham, and Paley, look into works of science, not with any view to extract truth therefrom, but to find happy applications in support of errors in human history, and a cosmogony to which antiquity has lent a sort of reverential awe."—P. 29.

"The history of the remarkable contest which followed I shall give in my life of Geoffroy. Cuvier ought to have avoided its discussion. In fact it did not in any way interfere with his great determinations—demonstrations I ought to call them. But he thought so ; and the world, which is worse, also thought so ; and thus forced on him the invidious task of assailing a theory, the correctness of which he had fully admitted in his youth."—P. 45.

In short, Cuvier was weak enough to allow a regard for the opinion of the world, to lead him to oppose what he ought to have supported—silly enough not to know, so well as Dr Knox, that his "great demonstrations" would stand as his *monumentum aere perennius*, whatever theories were founded upon them—and base enough to shut his eyes to the truth, from a dread of its touching his reputation. Call you this backing of your illustrious and immortal friends? We humbly conceive that a much more feasible and truthful explanation of Cuvier's objections to transcendentalism, especially to that most objectionable tenet—the progressive development theory—is that as an observer of nature, he found that the facts of comparative anatomy, and especially of palæontology (abundantly confirmed since his day), were inconsistent with a theory, which to him, as a Christian man, was additionally repugnant as subversive of his faith.

We must, however, spare a few lines for the Great Artists to whom Dr Knox starts *per saltum* from the Anatomists. Whatever amount of "unity of organisation" there may be in the doctor's theories, there is assuredly none in his book ; for there is not the slightest connection, that we can trace, between the one part of it and the other, nor any reason why the two disquisitions should be in one volume, except to save expense with the bookbinder.

"The following inquiry into the true relation which anatomical science bears to art, was undertaken chiefly with a view to terminate a controversy which has prevailed for at least some hundred years. The matter in dispute was, 'the importance of a knowledge of anatomy to the artist : ' the relation, in fact, of anatomy to art."—P. 135.

We are not prepared to maintain that there has been no difference of opinion as to the value of anatomy to the artist; but the existence of centuries of controversy on this point appears to us to be a pure effort of the doctor's fervent imagination; and if controversy there be, sorry we are to say that we do not see that Dr Knox has done anything towards its settlement. In fact, in reading this part of his book, we have been strongly reminded of some of the late despatches from Caffreland. There is a prodigious amount of words, and huge promises that all is to be settled forthwith, but for all that we can see of results, the controversy *labitur et labetur in omne, etc.*

So far as we can make out the doctor's opinions at all, they appear to be neither very new nor very wonderful. He holds that the ancient artists, having invented the "Greek canon of beauty," having discovered the perfect and the absolutely beautiful, embodied it in their matchless statues; and these having been disinterred at or about the time of Da Vinci, became in great measure the producing cause of the excellence of Leonardo and his great contemporaries and followers. "The grosser minds of modern men, at least of the European mind of that period, . . . could not at first discover nature in antique art. This was natural enough; but the great Italian masters made the discovery at once." "The effect which the sight of these antique marbles produced on these great minds was such as to enable them to soar at once to the highest style of art; to look for, and to discover and pourtray, the beautiful; to paint and to draw the objects of the material world as they exist in nature, and not as seen in the coarse minds of the mass," etc.

Having fired off his Greek canon, with which he makes as much noise as if it were its namesake with the double n, he comes to the controversy as to the value of anatomy to artists, which, though it has lasted centuries, he undertakes to settle. The first item of pacification is, that these same ancient Greeks had never studied anatomy. This the doctor says he has made out solely from examining their works. It thus appears that anatomy is not *essential* to the formation of a first-rate artist. We shall not, however, found much on this, because these same inventors of the Greek canon may have been exceptional personages. We, therefore, follow the doctor to the further settlement of the question, and, *Teucro duce*, take up the lives of Leonardo da Vinci, Michael Angelo Buonarotti, and Raffaele Sanzio, to see what light their biographies will throw upon the question. We accordingly find that Leonardo was a great anatomist, and made a good use of his anatomy; Michael Angelo was a superficial anatomist, and allowed it sometimes to mislead him; and Raphael was no anatomist at all, so that it neither helped him nor set him wrong. Yet all three were first-rate artists, each having his own peculiar excellences; and the result of the whole of this wordy biography is, that their knowledge or want of knowledge of anatomy had nothing to do

with these excellences at all. To this conclusion the doctor seems himself to have come, for he plainly tells—"Each walked majestically in his own great and original path in life, guided by an inward light, and regardless of all around. Thus they each attained, though by different paths, a reputation destined to be immortal" (p. 172).

We have said that, amidst the mass of words which we have before us, it is no easy matter to find out what the author means to express. You never know where you have him. You think that you have got at the nucleus of the subject, because he is soberly discussing anatomy and art, when lo! he is, in the next line, denouncing Dibdin's sea-songs as the vulgar trash of a Saxon boor; or lauding Napoleon as everything that is great; or vilifying a Prince of Orange for doing what his pet Napoleon was not averse to—"commanding French troops merely, I suppose, by way of amusement, with a chance of plunder, and keeping his hand in use at these sort of things." As was to be expected in loose writing of this kind, we have a variety of inaccuracies, some of which we are surprised to find in any book professing to be written upon art. Thus, at p. 137, he says, that "of the history of ancient Greece at the period when the finest marbles of antiquity were sculptured, little or nothing is known;" and, in the next page, that "these immortal works were not carved during the historic period, but prior to it." Where he got this information, so dogmatically asserted, defies conjecture. He seems, however, not to be very sure of his position, for he wriggles out of it in the next page, where he says, "The age of Phidias, it is true, is tolerably well known; and the Elgin marbles are *supposed or generally* understood to have been chiselled by this great master and his school;" but he returns to his first statement a few pages on, and says of the men who carved the Elgin marbles, that little or nothing is known of their persons; they belonged, perhaps, to the age of Homer." We certainly do not ask Dr Knox to believe that it was Phidias or his immediate pupils who carved these great works; but that it was at or about their time, and not in the prehistoric period, will, we think, be maintained by few but Dr Knox. As to assigning them to the age of Homer, he might as well have gone back to Tubal-Cain.

In the mere detail of biography, too, the author is wofully ill-informed. Da Vinci's death, which, according to his own peculiar fashion, is noticed at the beginning of his life, is stated to have occurred at the court of Francis the First (a story commonly improved by making him die in the arms of the king), in May 1519; but he says that there are doubts about this, and recommends to the Italians to settle this point, as an important one in the history of art. The thing is settled already. Da Vinci was born in 1452. He died 2d May 1519 at Cloux, near Amboise, when the king and his court were at St Germain, and only heard of his death by a letter

from Melzi to Leonardo's relatives. But there are good bits, as well as inaccuracies, in the book. The account which Dr Knox gives of the sketch-book of Leonardo, in the private library at Windsor, which the Doctor was permitted to examine, is highly interesting. Certainly not the least curious sketch there, is one of the valves of the aorta, which he vouches to be anatomically correct. It is to be regretted that the doctor could not decipher whether the writing in the same volume is explanatory of the drawings or not. If it is, it would not surprise us to learn that Leonardo, who was "an engineer, a mathematician, and a machinist," as well as anatomist and artist, had, as Dr Knox hints, known something of the circulation of the blood. Surely somebody must know what this writing is about.

But our readers will be curious to learn how the Doctor ends the great controversy as to the value of anatomy to an artist; and as the whole decision of the pacificator is contained at the end, we allow the Doctor to speak for himself in his peroration.

"There is but one school of art—nature. But, to read her volume profitably, artists must study profoundly the antique Greek, and ancient Italian school, formed by the era of Leonardo, Angelo, and Raphael.

"It may precede or follow or coincide with the study of the living figure; still these immortal works must be your guide. For whether it be composition, or colouring, or design, you are likely to find that these masters read Nature more clearly than you ever can. But do not copy nor imitate them further than as objects of study.

"Learn anatomy by all means, but do not forget its object. When you draw a dissected limb be sure to sketch the living one beside it, that you may at once contrast them and note the differences. In drawing from the nude figure, contrast your sketch with the antique; you will find in it many defects. Never forget that perfection, the result of a high specialisation of nature's law of individuality, is rare; the opposite, that is, imperfection, the result of a tendency to unity of organisation, is by far the more common. You will be chiefly called on to draw the draped figure: see that you place your drapery not on a machine but on a person of *fine feeling*. Fashion in dress is the trick of society, to substitute a conventionalism for beauty and fine forms; never sacrifice art at its shrine, but paint the person in what *becomes* him or her, regardless of the existing mode.

"The relation anatomy holds to art is to explain—first, how far the shapes and figures of the inward structures modify the external forms of man and woman;—second, it informs the artist of the meaning of such forms;—third, it explains to him the laws of deformation; that is, of variety in external forms; the causes of these varieties, and the tendency to which they lead. As an artist he must represent them, no doubt; but in doing so, let him wisely follow nature rather in her intentions than her forthcomings, and return to the perfect or to its approximation, whenever time and circumstances permit him to do so."—Pp. 202-204.

So that the settlement of the controversy amounts to this, Don't draw a dead limb when you want to express a living one; pray, Dick Tinto, don't carve a Fighting Gladiator from a body in the dissecting room; don't paint a Dead Christ from the Tipton Slasher, stripped and in a boxing attitude. So ends our controversy. *Requiescat in pace.*

We have said that the first half of the book is mischievous, and the last superficial,—a sort of muscicular abortion from a parturient mountain. We by no means, however, wish to prevent any one from reading it; we have no fear of its either undermining his orthodoxy or corrupting his taste. There is no argument to support the offensive tenets of the first portion, and what little is in the last portion at all is harmlessly feeble. The chief characteristic in the style of the work is its being rambling and disjointed, but pervaded throughout with a self-complacent thrusting forward of the author, which will make any one, on a first perusal—and it does not merit a second—laugh at it rather than condemn it, and vote it to be as we did, *Knox et preterea nihil*.

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## COLLOQUIA DE OMNIBUS REBUS.

### COLL. V.—VISIONS ON MOUNTAINS—OBSERVATIONS ON PLAINS —STRICTURES ON FLATS AND SHARPS.

*Editor* [to *Medicus*]. Did your expedition make out the ascent of the Cobbler?

*Medicus*. *Chirurgus* and I did; but *Obstetricus* was prevented by another laborious labour.

*Editor*. And were you rewarded for your pains?

*Medicus*. We were indeed. We have had a clearer view than before of two or three things at a distance.

*Editor*. We shall be glad to hear your adventures. I have always understood that the Cobbler is a remarkable mountain. Where is it exactly?

*Medicus*. It is situated at the head of Lochlong, opposite Arrochar, and forms one side of the entrance into Glencroe. It is a mica-slate mountain, towards 3000 feet in height. Thousands of tourists annually skirt its base on the way to and from the head of Lochfine; but, being unaware of its magnificence, scarcely any one ascends it. Viewed from the east, even at a distance of fifty miles, its top evidently forms a cleft crest of precipices; and nearer, but especially from the slope of the road above Arrochar towards Loch-Lomond, there is seen, perched on its crest, a rock singularly like a little cobbler seated at work, in the act of stooping to draw his waxed “ends:” Hence the name of the mountain. The summit has the structure of a semi-crater. The east half of the crater has vanished, giving place to an elevated valley about two miles long, deep, rugged, and gently inclined towards Lochlong. The west half, which remains, consists of a chain of precipices, often mural or overhanging, and at some points about four hundred feet in height.

The ascent, till you reach the foot of the precipices, is a mere affair of legs and wind, labour and love together. With half a mountaineer's eye you may also perceive your way round the precipices by the south-east; and then, twisting, climbing, and scrambling among enormous piles of fallen mica-slate rocks, you rest at length on the Cobbler's knees. But to reach his crown is quite another thing. Looking him in the face, you see his head is a great block about ten feet wide, twice as high, and thrice as long, with overhanging sides continuous with the primary precipices below, and apparently impracticable without mechanical aid.

Seated here, without a living creature in view, and wrapped in silence as well as solitude, now gazing along the river-like Loch, or at the green oasis of Arddarroch on its dark shore in the distance, now down upon the cheerful white cottages of Arrochar scattered under our feet opposite, and now up again at the ridges, peaks, and rounded heads of mountain rising behind mountain on every side—we felt, like other feeble mortals in similar situations, the vanity of human things in general,—and naturally most of all the vanity of Medical Journals,—but especially of the London Hebdomadaries.

*Editor.* Had you any fault to find with the Monthly?

*Medicus.* Fault? No, indeed! But we thought what would become of it, when we all grow old, and lazy, and witless; and what might become of it, if all capable practitioners in Scotland, or even only some fifty I could mention among the children of our own medical school, were each to contribute from their stores of practical knowledge but one twentieth part of that sustentation, which for two years past has been supplied by us all,—with, heaven knows, too scanty leisure for the purpose, and with no other object than the good of the profession, and the credit of old Scotland and its academic metropolis.

*Chemicus.* That was no mere baseless vision of the mountain-mist. Why should we not live to see it realised?

*Chirurgus.* Then we thought of the London Hebdomadaries:—what good they had done in their generation; and how much evil—how one had thrust itself into notoriety and success by stabbing and throat-cutting—that another, after struggling many years for lingering existence in a virtuous way, had sunk at last into the embraces of a third, the meanest of all—that a fourth had just begun to lower upon the horizon of medicine on a humble scale—that the coming of a fifth had been portended—how they have gradually swallowed up the honest old English Monthlies, left only one languishing Quarterly, and succeeded in so lowering the public taste down to their own level, that in periodical literature there is no longer any profitable investment for capital, whether in brains or gold, except the fleeting leaves of a weekly print—and most of all, how long it will be before the medical profession of England shall be so imbued with the dignity and purity of true learning, as to starve



into surrender those who win their weekly bread by devouring men's characters, and pandering to the insatiable human appetite for scandal, strife, and spoliation.

*Editor.* The power now possessed by the weekly medical press has really become a very serious matter, considering by whom it is exercised. Their depressing effect on medical literature and medical ethics raises grave doubts, whether the good they have done is not outweighed by the concomitant evil.

*Ostetricus.* They claim to be the redressers of medical grievances, the prop and stay of professional independence, reformers in medical legislation, and a great encouragement to medical literature. What have you to say to these pretensions?

*Editor.* The profession in England are undoubtedly indebted to them for undertaking a deal of disagreeable and dirty work for the redress of professional grievances. They deserve credit for their promptitude and vigour in assailing,—though not always indeed in the best taste,—arrogant navy-boards, miserly poor-law guardians, selfish assurance companies, greedy medical partners in druggists' profits, impudent quacks of all kinds, obscene advertisers, and the like. Students in London, too, have enjoyed singular facilities for denouncing the poorness of the dinners at St Bartholomew's College, the opacity of the dressers' heads that encircle the operating table at Guy's, the slothful progress of Todd and Bowman's Physiological Anatomy, and other annoyances of similar magnitude. At the same time it may be feared that they have often got very bad advice as to their behaviour at school, from such arbiters of manners and morals. As to the mightier grievances of the profession, such as the miserable state of medical legislation, the close system of medical appointments in the great London hospitals, the patronage of quackery by the public and the legislature, the discouragement of medical science and of science in general by our government,—the weekly journals may have done their best; but as yet they have effected nothing; and they have done some mischief too, inasmuch as the insolence of tone and personal abuse, essential to their system of tactics, have steeled against conviction men of weight whose minds might have been open to argument and persuasion. In the department of medical legislation, I am afraid they have obstructed the course of justice by the impracticable nature of the measures they have pertinaciously advocated, and the influence they have brought to bear against every rational scheme of adjustment.

*Obstetricus.* We may dispose easily enough of the boast of their partizans that they have been a great support to professional independence. I cannot, for my own part, positively state that I ever knew a man in our profession destitute of his own share of independence; and it may be safely said, that on the contrary we see many, who, were they to think and act less for themselves, and more by the advice of their betters in judgment and information,

would do themselves much good, and the world no harm. And if the facility of publication has taught the members of our profession more promptly to resist evil influence and injustice, it cannot be denied that they have been too often led by the same cause to commit themselves both to unnecessary defence and wanton attack; that these contributors to weekly literature have been too often betrayed into a low level of controversy by editorial example, so that the personal and the insolent have become the fashion in controversial medicine; and that the tone of medical ethics has consequently been lowered, thus grievously endangering the favourable position which our profession has hitherto occupied in society in Britain comparatively with other countries. Those who perpetually sneer at the combativeness of medical men, have no idea of the force of temptation in the ready opportunities of indulgence presented by the weekly medical press.

*Chemicus.* I take it that, as human nature now stands, the several professions are all much alike in regard to the natural gift of pugnacity, and the tendency to gratify it. But the varying circumstances of their lives yield various modes of gratification. The soldier alone is trained by a course of severe discipline to store his for use on legitimate occasions. Lawyers expend theirs in sham fight; expectorating their bile on the clients, or sometimes on one another in shape of their clients, and with the singular privilege of having an umpire on the bench to look on and stop the combat when it becomes unsafe. Divines have periodical opportunities of exhausting their animosity legally, usefully, and uncontradicted, upon sin and sinners in general. But medical men have no artificial outlet for their share of combativeness. Their professional relations supply frequent cause of quarrel; but they have nobody to assail except one another; they have no other way of doing so but in downright earnest; they have no authoritative umpire to regulate the lists for the duello; and instead of this, they have three or four hebdomadaries, sitting like so many "succubi" at their elbows, tempting them to commit the sin of wrath, recording its first overboilings, and thus binding them down to the consequences for ever.

It may be asked,—Why do the editors of the weekly journals admit into their pages so much personal contention, which in their hearts they must condemn? Listen to the answer of one of them who puts to himself the question, why he does not reject what he disapproves of. "If such were to be the rule of our policy, the 'Lancet' would soon become an insipid, worthless production,—the mere vehicle of puffing and vapid twaddle" (1851, p. 184.) In short, by the contrary course, the sheet is enlivened, and pays.

*Medicus.* Of all the pretensions put forth on behalf of the weekly medical journals, the most groundless is, that they encourage medical literature. On the contrary, they have degraded it. Their main peculiarity, besides the promotion of strife, scandal,

and personality, is the countenance they have given to a third-class literature, composed of cases, lectures, and hospital reports.

*Editor.* The mania they have created for this description of authorship is inconceivable. In the "Lancet" for March 22, 1851, a list is given of communications, which want of space compelled the editor to postpone; and it amounts to the appalling multitude of 118. The list consists of 7 lectures, 34 hospital reports, 30 cases, and 44 dissertations. It is very plain, however, that all England could not supply authors able and willing to furnish the fourth part of that amount of materials worthy of being rescued from oblivion.

*Medicus.* There is in the first place no greater load on medical literature than the undigested heaps of solitary cases, to which these periodicals have given birth. No one can over-rate the value of well-described Cases, when they present any novelty, or illustrate any point in practice not yet sufficiently established and elucidated. But the great majority of the detached cases in the weekly press are without any point to illustrate; most of them exhibit an utter ignorance or disregard of similar facts which had appeared again and again before; and not a few have no other merit than that they superfluously illustrate well-established generalisations which may be found in any good systematic work. If the authors of such trivialities would read before they write, they might save their pen and ink.—The so-called Hospital Reports of the weeklies are even worse. There is not perhaps a more difficult branch of medical authorship, or one requiring greater opportunities of study and more varied qualifications, than a well-digested hospital report. But forsooth, the editor of the "Lancet" on the 22d of March 1851 had no fewer than four-and-thirty more than he knew what to do with! Were hospital physicians and surgeons to make it a habit themselves to study, methodise, and comment upon the cases of a season, and favour the world with reports framed on such a basis, their disinterested exertions would be everywhere acknowledged with gratitude, and would doubtless raise the standard of medical literature and learning. But it is a farce to entitle as hospital reports the productions, which appear under that name in the weekly periodicals, and which are seldom anything else than solitary cases, often too without point, without novelty, and even without any better authority than that of the young penny-a-liner who composed them.—As for Lectures, we had occasion to digest the Clinical ones to some purpose not long ago, and nothing more need be said of them. The lectures of a systematic kind have been now pretty well exhausted; and, looking back to them, it is not easy to see how they differ either in nature or utility from well-known pre-existing independent works on the same subjects, or how they have any pretensions in point of quality to equal consideration.

*Obstetricus.* The department of original dissertations, the most important branch of journalism for the literature of medicine, must, of course, present inequalities in all periodical works. But it is very plain that a great proportion of those in the weekly journals exhibit signs of hasty publication; and few will stand as works of reference after the lapse of two or three years. They seem in general intended for temporary, and not for permanent monuments of the industry of their authors; and it is greatly to be feared that their object has been not so much to advance medical learning and practice, as to serve for an advertisement to bring the authors' names before the public.

*Medicus.* It is a remarkable coincidence that, contemporaneously with the spread of weekly journalism, there has been in this country a great decrease in standard works of original research. I fear that matters have reached this point in London,—that a man can scarcely now advance into professional practice, unless he advertise himself by entering into competition with the crowd who scribble for the weeklies,—that, when he does so succeed in attaining a respectable position in his profession, it is not until he has thus naturally enough acquired a distaste for the toil of medical authorship; and that consequently he ceases to write when most competent to instruct by his writings. You may trace more than one biography through these successive phases.

*Chirurgus.* Add to all this the intolerance of the weekly tyrants for every thing better than themselves and their partisans, their base usage of the heads of the profession for at least a quarter of a century; their furious and unreasonable antipathies, their uncompromising advocacy of their own favourites for the time being; and no wonder can arise that in the higher walks of our profession there shall now exist a disinclination to take the same active share as formerly in contributing to the literature of medicine.

In the days of buccaneering, when some band of pirates had boarded a vessel down on the Spanish main, they either made the crew walk the plank, or clapped them in irons under the hatches, and then, dressing themselves in the cocked hats and laced coats of the officers, they strutted on the deck with all the air of legitimate possession. Just so have our brethren of the southern metropolis been treated by the weekly periodicals, which, during the last thirty years, have gradually acquired an ascendancy no less complete, than despotic and injurious. They have silenced the good men of standing and reputation, knocked down and abused any man who ventured to question their authority, and thrown a shield over the folly and presumption of their own creatures.

*Editor.* This is a sad view of the case. Do you see no chance of escape from such miserable thralldom?

*Chirurgus.* Yes, I do. And what is more, I believe it is from us the poor sufferers are most likely to get assistance for their

emancipation. The piratical crew constantly endeavour to keep their prisoners in the dark as to what is going on in the world around them, and to make them believe that the power of their master is proof against all attack. But if we can manage to let a little light shine down into the gloom of their dungeon, we may awake some spark of hope in their disconsolate breasts; and, reminding them that where there is a will there is a way, we may perhaps rouse them to make the effort requisite for setting themselves free. They will then make a rush to the arm-chest, shoot the man at the wheel, hoist the old standard of independence, and, giving three cheers for the good old cause of truth all over the world, again pursue the course which has been so long and disgracefully interrupted.

*Medicus sings.*

“ Thus said the captain  
To his gallant crew;  
Down with the black flag!  
Up with the blue!

Fire on the maintop!  
Fire on the bow!  
Fire on the gun-deck!  
Fire down below!”

*Omnes in chorus.*

Down with the black flag, etc.

*Medicus.* I espy another blue flag bearing down to our assistance.

*Reliqui Omnes.* Where?

*Medicus.* There! In the south-east! The Provincial. She comes out as a *rasé* in the weekly form, purposely to meet the rogues in their own waters.

*Chirurgus.* She is said to be chartered by respectable owners, and has plenty of ammunition, so that she only needs a well-appointed crew and a judicious captain,—above all things true to the old flag,—in order to blow the whole nest of pirates out of the water. We shall watch her manœuvres with some anxiety. Let us drink to her success and good behaviour in the meantime.

*Editor.* We left you sitting on the Cobbler's knees. Did you reach the crown of his head? Or did you make any other discoveries from your giddy eminence?

*Medicus.* Looking him full in the face, we thought he had a most impracticable and forbidding appearance. But on narrow inspection, a hole, or eye, is descried in his right cheek, on creeping through which you see on his left jaw your way to his hindhead by an ascending ledge, two feet wide, inclined uncomfortably outwards a little, and separated by an ugly crevasse from the main rock,—at this spot an overhanging precipice, towards fifty feet in height. On creeping through the Cobbler's Eye, and coming suddenly in sight of what remained of the ascent, I had a vision in mid air below of two or three familiar faces, anxiously upturned and watching for a

vacancy in sundry offices; and at the same time I experienced an extraordinary sensation, as to which I thought it prudent to consult *Chirurgus* before venturing further. So I retreated, and *Chirurgus* in turn made his way through the sinus with his usual caution and aptitude—

*Chirurgus*. —and back again,—satisfied that the ascent is practicable; that necessity or duty, but no other incentive, should induce him to climb it; and that he had discovered the true seat of bodily fear, which it seems had been also just discovered by *Medicus*, and as to which the world in general is strangely in the dark.

*Medicus*. The seat of sudden moral fear, so to speak,—for example, the sudden fear of a wound to the affections,—seems certainly to be in the heart; and no one who has felt its cutting pungency can wonder that in an acute degree it should prove sometimes instantly fatal, as both fact and fiction say that it may. But *Chirurgus* and I each found, while in the Cobbler's Eye,—meditating further progress, but dubious about the stability of the ledge, and calculating the effect of last winter's frosts on the crevasse,—that the seat of anxiety for the person, or bodily fear, is in a very different part of the body. Many years ago, on leaping from a rock on Arthur Seat to save a fall, and while descending through eighteen feet of air, to alight unhurt in a bed of tall nettles, I felt in this quarter a momentary cutting sensation so acute that I never have forgotten it. I experienced it again in the Cobbler's Eye, and so did *Chirurgus*.

*Chirurgus*. We accordingly re-seated ourselves on the Cobbler's knees, and wondered why the sensation should be felt in a part of the body, the least likely perhaps of all to be maltreated in a tumble;—by what physiological mechanism it is produced there, of all possible places;—how the fact has never been noticed before by either physiologists or mental philosophers;—and whence it is that “language has been made to conceal the thoughts” so completely in this case, that it is a daily habit to speak of the heart failing in sight of sudden danger, whereas every one personally cognisant must be aware that it is quite another organ. Those, indeed, who speak of being unmanned by fear, may be thought to have had a clearer conception of the truth. But yet it is probable that they speak figuratively.

*Medicus*. Having ruminated over these things, and being unable to come to a right understanding of them, we determined to carry the subject down with us for the consideration of *Physiologus Londinensis*, who was waiting for us at the bottom. But he could give us no farther satisfaction than that the phenomenon must be a reflex operation, and referred us to Dr Marshall Hall for ——— [Enter *Famulus*, with a Card.]

*Medicus*. “*Dr Nix!*” We shall be all delighted to see him. Desire the gentleman to walk in. [Enter the doctor, very tired and



*very cold, having just arrived from London by train, with the wind at N.N.W.]* Doctor! you are welcome. Before you tell us how we happen to have the pleasure of this visit, let me recommend to you a tumbler of this indigenous cordial. We call it whisky-toddy. It is an excellent restorative; and in your case will, doubtless, have all the charm of novelty.

*Dr Nix.* [*Having complied with this advice, and wonderfully comforted in consequence.*] You are probably aware, gentlemen, that I have just had a case of death from the administration of chloroform, notwithstanding the use of my inhaler, which has hitherto been regarded by me as an infallible preventive of bad consequences. As you still boast, I understand, of invariable success with the handkerchief, and had the civility to give me an invitation to Edinburgh not long ago, I have come to get some information on the subject.

*Chirurgus.* Dr Nix may be assured that we have no intention of boasting. We have too much reason to be humble and thankful, considering what has happened in other quarters, for us to make our immunity a subject of boast. It is true, nevertheless, that in the metropolitan hospital of Scotland, there has hardly been any operation, great or small, performed during the last five years, except under the influence of chloroform,—and that not a single instance of fatal effect has occurred. This fact, we think, is sufficient to show, that the administration of chloroform, when properly conducted, ought not to be regarded in a general way as dangerous,—and that the handkerchief, always employed in conformity with the original recommendation of Dr Simpson, does not deserve the condemnation it has received from Dr Snow and others in the south. But we would regard it as no less rash than foolish, to deny the possibility of death resulting from some accident or unfortunate combination of adverse influences,—such, for instance, as would have happened, if chloroform had been administered in one of the very first cases in which its employment was proposed. This was an operation for hernia, in which one of the ordinary surgeons of the Infirmary at that time, had scarcely made his first incision, when the patient gasped and died,—without any good reason that could be discovered, then or subsequently. Now, had chloroform been used in this case,—and the merest chance prevented it from being so,—the only death on the operating table, that has happened during all my acquaintance with the hospital for a period of more than thirty years, would most unquestionably have been ascribed to the anæsthetic agent, and would perhaps have opposed an obstacle to its adoption, no less formidable than the death of the London guinea-pig.

*Chemicus.* A most merciful deliverance, truly! There would have been an end put to chloroform for ever had that patient taken it. But, indeed, might it not have saved the man's life, by annihilating the nervous disturbance which, in all probability, occasioned his death? What guinea-pig do you refer to?



as well as those who have taken this up at second hand, even although they make a distinct profession of it.

*Dr Nix.* I receive your advice with the courtesy with which it has been expressed, and will give it my serious consideration. I have the honour, gentlemen, to wish you a pleasant evening.—

*Exit.*

*Chirurgus.* What news from Holland, Mr Editor?

*Editor.* I wish you would all go over and look at the new hospital at Rotterdam, before the Managers of our Infirmary proceed any farther with their building plans. I confess myself to be by nature a domestic sort of animal, not quite free of prejudice in favour of Edinburgh, and much inclined to the existing order of things medical; and, consequently, during my late trip to Holland, I was in a fit state for viewing novelties, without being led away by excessive admiration, or seduced into admitting foreign superiority. But the new Gasthuis of Rotterdam is a sore trial to any one who wishes to keep the tenth commandment.

It stands on the Coolsingel, near the Amsterdam Railway terminus, at the outskirts of the town, and separated from the canal by a handsome railing and shrubbery. It is four stories high, and, of course, being in Holland, has no sunk floor. The front is very handsome, and apparently of freestone; but as to this point I do not speak with confidence; for its lustre, like that of a new London Club-house, is suggestive of brick, mortar, and cement.

Dr Molewater, the chief medical director, who resides in the building, did me the honour of conducting me over the whole establishment. On entering the centre door, I found myself in a spacious lobby, and fronting a grand staircase, from the landing-places of which other staircases, of ample dimensions, lead upwards to the different corridors of the wards. The walls of lobby, staircases, corridors, and wards are of dazzling whiteness; and there is an extraordinary display of luxury in mahogany stairs, bannisters, and doors. The corridors, which are probably twenty feet high and of the same width, open by mahogany double-doors, each into four wards. Every ward contains ten green painted iron bedsteads, covered with white counterpanes, and surrounded by striped blue and white curtains, which are hung about ten feet above the floor, by means of an iron rod around the ward, bearing above each bed a semicircle of thick iron wire, on which the curtains play. When the curtains are close-drawn, free ventilation is permitted through the semicircular space above; and a touch of the hand suffices to draw back the curtains to the wall, and afford uninterrupted access to the patient.

There is no wooden skirting in the wards; in fact, no wood-work except the floors, which are of broad white deals, as clean as daily washing and scrubbing can make them; nor is there any of that detestable waxing of floors, so trying to British equilibrium in some of the French hospitals; here honest soap and water are

alone in favour, and woe to the spider or bug who dares to defile these sanctuaries with his presence. The four wards of each gallery, which inter-communicate by side doors, constitute a division, under the care of a well-qualified attendant, with four nurses, one for each ward. Adjoining the wards there are rooms for the attendants, and hot, cold, vapour, and douche baths on every floor. The lower panes of the windows in the wards and corridors open on hinges; but the sashes are fixed. The total number of beds in the hospital is 240.

The ground-floor contains, with other accommodations, the laboratory, a model in point of arrangement and cleanliness, and presided over by Mr Robertson, whose great-grandfather emigrated from Scotland; and there is also a steam-engine, which, with a very moderate consumption of fuel, executes a great variety and amount of work. It pumps an enormous supply of water to every part of the building; moves the lifts in two shafts by which the food, medicines, and other articles of consumption are raised to the several floors; cooks the meat; warms the baths; and sending a steam-pipe into every closet in the house, enables any nurse to prepare poultices, fomentations, or five or six gallons of boiling water in five minutes, by simply turning a stop-cock to admit the steam round a proper vessel.

The heating and ventilation of the whole establishment are admirably managed. There are neither open fires nor stoves in the wards. These are heated, as well as the corridors, by warm air, which is conveyed in iron-pipes lined outside with fire-brick, and charged with due moisture by being passed over water. Between every two beds, about eight feet from the floor, heated-air enters by a finely-perforated plate. The amount of moisture is regulated by observations on the wet and dry bulb-thermometer, and by attending to the surface and temperature of the water which supplies the vapour. The temperature of the wards is kept as nearly as possible about 68° F. There is no unpleasant odour in them; and the appearance of cleanliness everywhere is most refreshing.

Nothing can be imagined better adapted to the treatment of pulmonary diseases. It is possible, however, that patients, after a long residence amidst all these comforts, may become more susceptible to the influence of cold and moisture on returning to their homes. And although there are many points in the arrangement of this Dutch hospital which we might gladly see imitated in our own part of the world, it must not be forgotten, that a due regard to economy may prohibit their introduction here. The machinery for heating, ventilation, and bathing, would be very expensive in a British hospital, both in construction and maintenance, and would require the constant superintendence of an experienced engineer, whose services would in themselves constitute a considerable item in the annual outlay. But the same amount of cleanliness might

surely be attained in a land where water is plentiful; and pure air and soap not unreasonably dear.

Dr Molewater directed my attention particularly to the operating-table, a very curious piece of mechanism, which fairly puts to shame the old butcher's block in our Surgical Hospital. It is a stout steady mahogany table, which can be lengthened or shortened, and turned or inclined, in whole or in part, in every direction, by machinery not unworthy of Lord Rosse's Telescope,—and which can be raised or depressed at will by hydrostatic pressure from a column of water sixty feet high leading from a cistern at the top of the house.

*Medicus.* Did you see much of Dutch Fever in the wards?

*Editor.* I saw only some cases of Simple Typhus, exactly resembling those with which we are familiar in the Infirmary; and Dr Molewater assured me that Dothineritis is hardly ever seen at Rotterdam, except in cases imported from France or Germany. I was surprised too when he told me that Agues are not common. But perhaps they are too much so, and also too much the subject of domestic treatment, to be often admitted into hospitals. Phthisis, rheumatism, organic diseases of the heart, and granular disease of the kidneys, are there, as with us, the most common of chronic diseases. Molewater speaks highly of the efficacy of the vapour-bath in the treatment of Bright's disease.

*Obstetricus.* You were at Utrecht: did you encounter Professor Harting or Schroeder van der Kolk?

*Editor.* I had the pleasure of making the acquaintance of both, and of visiting their museums with them; and I had the great good luck to see most of the Savans of the Netherlands at a quarterly meeting of the Association for the cultivation of natural science and medicine in Holland. Although I had thought myself tolerably familiar with continental customs, I confess I was not prepared for the form of procedure by which Professor Vrolik of Amsterdam constituted the meeting as its president. As soon as he was seated, he deliberately drew forth a box of lucifers, made a short speech, lighted his cigar, and smoked it "ex cathedrâ;" an example which was immediately followed by the secretaries and almost every man in the room,—who from twelve o'clock till four maintained the sacred fire with a solicitude worthy of vestal virgins.

*Chemicus.* Had the philosophers any beer with their tobacco, more *Germanorum*?

*Editor.* No. Apparently Mr Allsopp's medico-chemical puff-circular had not then reached Utrecht.

*Chemicus.* If Mr Allsopp has half the skill in brewing as in puffing his beer, it may be worth their attention. He has actually had the ingenuity to extract a testimonial to its quality from the notice we took of his precious certificates.

*Editor.* We have no reason to complain of the position which

we hold in his estimation. He seems to have felt that doubts might be entertained by the public of the honesty of his cloud of witnesses; and he therefore favours the world with our opinion, that "Mr Allsopp's advertisements contain medical names which are a guarantee against suspicion in their instances." *Quis custodiet ipsos custodes?* Who will certify the certifiers? We ought to be highly gratified with the compliment implied in being selected for this purpose; and therefore let us make him welcome to the "incidental testimonial,"—protesting, however, that we have neither in recompense received a sample of his beer, like Baron Liebig, nor even pocketed a hundred guineas, like one at least of his puffers metropolitan.

But as to your inquiry about the philosophers of Utrecht,—I had much professional conversation with Schroeder van der Kolk, who is looked up to by his countrymen as the patriarch of the Utrecht School, and is esteemed by all that know him as a man of the most kindly disposition and unwearied industry. He is stout, active, and young for a patriarch, being a hale gentleman of fifty-five or thereby. His labours are well known in this country; but few know how enthusiastically he works, except those who have seen him at home, in the midst of the splendid museum collected by himself. Every preparation has its little history, and serves a special purpose in the collection. For it is not sufficient that a specimen should be rare, or a fine injection; it must illustrate some physiological or pathological fact or theory, otherwise it is held in small respect by the professor.

He showed me part of a large series of beautifully injected specimens of the placenta and uterus. You are aware he has recently published an elaborate memoir on this subject; of which, however, no notice has hitherto been taken in our medical journals. He kindly spent several hours in showing these and other medical treasures,—such as preparations of the kidney of man and all classes of animals, exquisitely injected,—specimens of cancer in various organs,—specimens of diseases of the heart and vascular system, including a double aorta,—and above all a human allantois, regarding which he had an amusing story to tell.

The philosopher had not long bottled up his allantois, and was still enjoying the great satisfaction of being the only possessor in the world of such a treasure, when one day Bischoff, the embryologist, called to inspect the museum. Of course the allantois was produced. But Bischoff, *zweifelte daran*,—was sceptical about the allantois,—to the grievous disappointment of its owner. It might be a diseased ovum! Impossible, replied Schroeder;—when, horror upon horror, the sceptic craved permission to dissect the specimen, asserting that he could then only pronounce decidedly upon the subject. It was a trying conjuncture for poor Schroeder! To be asked to entrust his allantois, the very flower and pet of his whole collection, to the ruthless hands of an incredulous and perhaps



envious anatomist! So he expressed his fears. "Let me take it out of the bottle," said Bischoff, "and examine it with the microscope, as I propose: It shall be restored in safety,—*dafür bürg ich mit meinem Kopf*, I pledge you my head it will." The tempter prevailed. "I commit it to your tender mercies," replied Schroeder, "my preparation is *unicum*; but you are *unicus*!" The anatomist retired with the allantois into a closet. In a few hours Schroeder looked in to learn the result of the *tête à tête*, and was rapturously saluted by the embryologist: "*Da haben Sie, Herr Professor, was noch kein menschliches Auge nicht gesehen!*"—Professor, you have got here what no human eye has ever seen before!" The allantois was safe, and its authenticity recognised by the first authority in Europe.

I likewise made the acquaintance of Donders, and had a long colloquium with him, *De rebus omnibus, et præsertim nobismet ipsis, et Scholâ Medicâ Edinburgensi*. He is perfectly read in English medical literature, has visited London, and purposes to come among us one of these days. We conversed for mutual convenience in German; and I may remark, that I never had occasion to use the French language with the Dutch, for whenever our respective mother tongues were insufficient, German was sure to carry us through. Donders is an energetic and rising man, who observes and experiments much, and writes a good deal in the "*Nederlandsh Lancet*," which he edits. He is now completing his "*Handleiding tot der Natuurkunde van den gezonden Mensch*," or System of Physiology, which promises to be an important work.

I met Professor Harting in the museum of the Anatomical School in Jans Kirk Hof, and found him measuring skulls in the collection. He took a great deal of pains to make my stay in Utrecht agreeable.

The most curious sight in the School is the old-fashioned anatomical theatre, which reminded me of the chambers represented in the old prints. It is far more antique in appearance than even the "*Snijkamer*" of old Tulpius in Rembrandt's celebrated picture. It consists of a funnel-shaped box, about twelve feet wide at the bottom, and thirty feet at top, supported by wooden props on the floor of a moderately-sized room. In its interior the students seats are disposed as in an amphitheatre; the lecturer and his table stand in the throat of the funnel; and immediately above them a gigantic white reflector, like an inverted kail-pot, throws light from an oil or gas lamp on the subject of demonstration.

I afterwards visited Harting at his *Laboratorium*, and spent some hours in examining his great microscopical collection. Of instruments I saw a sufficient variety, from the simple lens of Leeuwenhoek to the most recent models of Oberhäueser, Nachet, and Amici. Harting uses with his microscope a kind of ocular, made at Wetzlar by Kellner, and called the Orthoscopic Eye-piece, which was quite new to me, and is, I think, unknown in this country. It has

the advantage of giving a very wide and perfectly flat field. Kellner asserts it is made of three lenses, but makes a secret of the construction. The most instructive specimens perhaps in Harting's collection, are the injected preparations illustrative of Bowman's views as to the intimate structure of the kidney,—in which the tubes are filled with blue and the arteries with red injection. There are also admirable injections of the liver,—the pupillary membrane of a four months' foetus injected from the umbilical vein, with chromate of lead and size,—the villi of the intestines filled with two colours,—and above all, the capillary system of the human lung, the specimens of which are the finest microscopic objects I have ever seen. The material used for injecting them was Prussian-blue suspended in oxalic acid and size (See "Monthly Medical Journal," March 1852); and they were perfectly preserved in a weak solution of arsenious acid, in which they could be examined by transmitted light, with a power of 250 diameters. Harting and Schroeder told me that the blue injection, though apt to lose its colour when long excluded from the light, soon regains it on exposure. The specimens I saw, which were five or six years old, were in excellent condition.

I had also the satisfaction of hearing a paper read by Harting, at the meeting of the Association. The subjects brought before the meeting were sufficiently diversified,—viz., A mathematical investigation of Foucault's experiment on the Pendulum; Observations on Uterine Hydatids, and the Pathology of the Chorion and Amnion;—A description of an apparatus for measuring Electro-Magnetic intensity;—on the Glands of the Stomach, etc., by Donders;—on the Blood-vessels of the Brain, by Schroeder van der Kolk;—and on the Island of Urk in the Zuyder Zee, by Harting. Urk is an island not much bigger than Inchkeith, and is supposed to contain in its population, at least of its cemeteries, some precious relics of primæval Batavians. In the course of his communication, Harting produced the skull of one of these ancient gentry, which greatly edified the ethnologists of the meeting. There was a narrowness of brow and breadth of hindhead, phrenologically indicative of a character far from amiable, and a singular want of hollow-ness below the malar protuberances, which would have interested Lavater. There was much in Harting's communication relative to the Geology, Botany, Climate, etc., of this little island,—enough to show the variety of the author's acquirements, and his zeal in prosecuting any scientific inquiry to which he applies his powers of observation and description.

*Medicus.* Anything equally good, *Chemicus*, among your incidents of travel on Spey-side and Brae-Riach?

*Chemicus.* Nothing half so good. But on my way home I met with a curiosity which you will oblige me by explaining. Driven south by the first snows of approaching winter, I encountered at Perth a consignment of ice from London for a cockney sportsman

on the Inverness-shire mountains. It would be instructive to learn through what conglomeration of ideas such a commodity was destined thither at such a season.

*Editor.* Various theories suggest themselves in explanation. But I am afraid we must postpone this matter. It is too serious a question to take up so late.

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## Part Third.

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### CLINICAL REPORTS, LECTURES, ETC.

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#### MEDICAL CLINICAL WARDS.—AUTUMN 1852.

CASES ILLUSTRATING THE ASTHENIC FORM OF INTERNAL INFLAMMATIONS NOW COMMON IN THIS COUNTRY. BY DR ALISON.

I recur to this subject—of which I offered a few illustrations last year—because it seems to me of such importance as to make it highly desirable that we should possess as precise information in regard to it as its nature will admit.

It is important with a view to theory, because we cannot have correct notions of the nature of inflammation, or its effects on the living system, without being assured whether or not it is the fact, that the same local changes, to which we give that name, in the same textures and parts of the body, leading to the same effusions and consequent lesions of texture, may, in different persons, and in the same persons in different seasons (independently of any complication with other diseases), go on in connection with, and may even prove the immediate cause of, nearly opposite forms of constitutional disorder, and may therefore be fatal in very different ways. In particular, this observation has always appeared to me to be very important in reference to the question, whether the state of inflammation (denoted with most accuracy by the precise nature of the effusion and consequent lesion, in any individual texture of the living body), can be reasonably ascribed to any conceivable modification of the natural power of contraction, with which the vessels supplying that part are endowed. If we find that inflammation, from first to last, or during its most active stage, is not only compatible with, but is apparently supported by, nearly opposite conditions of the flow of blood through those vessels,—and accordingly, that it appears to be moderated and restrained in different cases, by means the effects of which on the contractile power of vessels are quite opposed to one another,—we have surely, in those facts, a strong ground for the opinion, that neither the “solus spasmus” nor “simplex atonia” of Hoffman or Cullen or Hunter—indeed, that no modification of the vital power of vessels—can be regarded as the main cause of inflammation.

Again, it is equally important in a practical view, to understand that it is not the circumstance of any part of the body being affected with inflammation, or even with inflammation in any particular stage of its progress, which demands any particular remedy, either of the sedative or stimulating class; but that remedies of different character, and even modes of treatment apparently opposite, may not only be adopted with general success, but, on careful examination of individual cases, may appear decidedly useful; i.e., useful in sustaining and promoting the different natural processes, which are the chief agents in conducting all inflammations to a favourable termination.

This assertion is, however, only extending to the vital actions of nutrition and exhalation concerned in that process, a principle which is now generally admitted as to the vital actions of the nervous system. The inquiries and observations of Dr Abercrombie, Dr Marshall Hall, Dr Gooch, and many others, have unequivocally shown, as to many cases of threatening of apoplexy, and of symptoms resembling phrenitis or hydrocephalus—what perhaps was better understood previously in regard to epilepsy, and to delirium tremens—that the morbid actions of the nervous matter indicated by these diseases, may not only take place under nearly opposite conditions of the central function of circulation, but be obviously aggravated and prolonged, if not excited, by deviation in the way either of increased or deficient supply of blood from the healthy state; and, therefore, that according to the existing state of the circulation, symptoms almost exactly of the same kind, affecting the functions of the brain, pervigilium, delirium, spasms of different kinds, even stupor, may be obviously benefited in some cases by depleting, and in others by stimulating remedies; or that opiates and other narcotics, which exert a certain degree of peculiar or specific influence on different kinds of delirium or spasms, may appear on the most careful examination of individual cases, manifestly aided in their effects, sometimes by a sedative and sometimes by a stimulant influence exerted on the circulation,—according as the state of the circulation, going along with that morbid action of the brain, deviates in one mode or in another from the healthy condition.

In like manner, we now understand, certainly better than formerly, the true pathology of many cases of various modes of affection of the brain, which depend on the retention of urea in the blood; and we know, that the action of this retained poison on the nervous matter is in some cases (and those, if seen in time, the most manageable), connected with a rapidly excited and vigorous state of the heart's action, maintaining a congested if not inflammatory condition of the kidneys, and that in some such cases, the effect of general blood-letting on delirium, spasm, or coma, is as rapidly and decisively beneficial, as in any case of acute inflammation or hemorrhage; but that in other cases the disease of the kidneys, although indicated by the same condition of the urine, and leading to the same diseased actions in the brain, proceeds slowly and insidiously, and is neither attended by any such excitement of the circulation, nor capable of being benefited by any such remedy.

But these things being so, there is nothing anomalous or perplexing in the farther statement—at least equally important, if true—that the process of Inflammation itself, consisting essentially in a peculiar perversion of the fundamental vital action of nutrition, is in like manner by no means uniformly attended by the diathesis phlogistica of Cullen and the older authors, which is “*always* characterised by a hardness of the pulse, is increased by all tonic and stimulant powers applied to the body, and is most effectually taken off by the relaxing power of blood-letting” (First Lines, § 247); but that it is sometimes attended by an increase, and at other times by a rapid diminution, of the powers moving the blood through the body; and that according to these varieties of the circulation, the natural process by which this morbid action is brought to a favourable termination, may be obviously aided in some cases by remedies which have the very opposite effect in others. We may state farther, that the coincidence of inflammation, even of such inflammation as causes very rapid and extensive, although peculiar effusions, with a depressed state of the circulation forbidding full or general depletion, when that inflammation has been of what we call a specific character (diffuse, or erythematic) and arisen from a specific cause, has been long known. What we now consider is the asthenic or typhoid form of fever, going along with local inflammation of healthy character, proceeding from cold, and tending to the usual inflammatory effusions of lymph and pus.

How it should happen that these different forms of inflammatory disease should occur, and so different remedies be found useful, at different times and

in different places, we must profess our entire ignorance. It is by experience only that we are assured that such differences exist,—that they exist too generally and too permanently to be ascribed only to occasional differences of temperature, or weather, or diet, or the constitutions of individuals; and farther, that they occur under the observation of the same persons,—as indicated by phenomena which are not matters of opinion, and under circumstances which require the alteration, if not absolute abandonment, of opinions which had been thought fairly founded on experience. If these observations are correct, therefore, they not only constitute a most important fact in the history of inflammatory diseases, tending remarkably to explain the otherwise unsatisfactory and perplexing variety of opinions which we find to have existed, among men of acknowledged discernment and judgment, as to the most successful treatment of such cases, but they open to us a new line of inquiry, the results of which no one can yet anticipate, into the nature of certain hitherto unperceived influences, pervading the surface of the globe, and known to us as yet only by the modifications of vital action which they produce.

I can truly say, not only that it is no theoretical change of opinion which has convinced me, that many internal inflammations, resulting from the usual exciting causes, are now attended by a much more asthenic form of constitutional disorder than thirty years ago,—that they *may be fatal in a different way*,—and often demand a different treatment; but that this belief has been only gradually established, and, as I think, only as justified by facts frequently presenting themselves; and further, that it is still only avowed, in so far as facts, which can be easily described, seem to require; my belief remaining unshaken, that cases of internal inflammation have often occurred, which were restrained and conducted to a favourable termination by full and repeated bleedings, but which, under any other treatment, would have gone on to rapid disorganisation of the affected texture, and consequent death; but such cases appearing to me to have been sufficiently indicated by a peculiarity of the symptoms, which is seldom ever observed; and the inflammations of the same parts, now more common, showing as distinctly, by other marks, that they neither involve exclusively the same danger, nor admit of the depleting treatment to the same extent, or an extent approaching to what was formerly common; without undeniable risk of consequences, which did not result from the large and repeated bleedings of former days.

In considering this question, it is first necessary to remember that we have now the means of recognising internal inflammations, particularly in the thorax, which we did not possess formerly. The notion attached to the term pneumonia, when applied to a case in progress, in the time of Cullen or of Gregory, was merely *nosological*. It expressed a case of which the leading symptoms were—*inflammatory fever, dyspnoea, and pain in some part of the chest*; it was regarded as intense in proportion as the course of the symptoms thus observed in combination was rapid and urgent, and if not checked and altered in its progress by early and active treatment, was expected to go on to death by asphyxia—the danger being indicated simply by the degree of dyspnoea. Dr Cullen was at pains to explain, that he distrusted any diagnosis of the texture within the chest which was specially affected; that he was aware of the pain being often slight or absent; but that the pulse, in cases which he called pneumonia, seldom failed to be “*frequent, full, hard, firm, and quick*” (i.e., sharp); and when deviating from these characters, it was usually in the advanced stage, and was often irregular (which may frequently have been from complication with diseased heart). It was of this combination of symptoms, making rapid progress within a few days, after the usual kind of febrile accession, that Cullen and Gregory asserted so confidently that they implied great danger, indicated chiefly by the urgency of the dyspnoea, often amounting to orthopnoea, with flushing and turgescence of the neck and face, and that the only remedy to be relied on was full and repeated bleeding, used early, when effusion had hardly if at all begun, and while the pulse retained



the characters stated above, and repeated as long as, in connection with that state of the circulation, the dyspnoea continued or recurred. It was allowed that the above description did not apply to the state of the pulse in all cases,—particularly that cases occurred in which the pulse, in the early stage, felt weaker than might have been expected from the other symptoms; and on cautious trial of blood-letting, and careful observation of its effects, it was noticed in some such cases that the pulse improved in strength after the first bleeding, so as to suggest the idea that it had been rendered small and apparently feeble by the difficulty of transmission of blood through the lungs in sufficient quantity to excite strong action in the left side of the heart, and that this difficulty as to the use of the remedy therefore disappeared as these cases went on. I still maintain that, in these circumstances, full and repeated bleeding is the proper remedy, and every thing else is trifling; but I maintain further, first, that the pleuro-peripneumonia, as made known to us by auscultation and percussion, may, and at present often does, exist and extend itself rapidly over the lungs, without any such urgent dyspnoea, and certainly without any such inflammatory fever, or such characters of the pulse as I have quoted,—sometimes with slight fever, sometimes with fever assuming rapidly the typhoid type,—and that in such cases the full and repeated bleedings are either unnecessary or unsafe; and, secondly, that cases of pneumonia answering to the description of Cullen and others, particularly as to the urgency of dyspnoea within the first few days, and the strength and *endurance* of the pulse attending that dyspnoea, are of rare occurrence at the present day.

Such cases as are here given show unequivocally, that we may have all the local indications of extensive and rapid pneumonia with very slight fever, and no urgent dyspnoea, and that in such cases *no active treatment is required*. The patients should, of course, be watched from day to day, and the usual remedies—local bleedings, blisters, antimonials, laxatives, opiates, etc.—in some of them may be very properly used, to palliate symptoms and appease anxiety, or to guard against aggravations; but the essentials of practice are merely *rest* and *low diet*; under these, cases showing that combination of symptoms may confidently be expected to get well by the salutary provisions of nature herself. If the frequency and firmness of pulse and dyspnoea become aggravated, more powerful antiphlogistics are demanded; if the fever becomes typhoid, or the pulse feeble and the expectoration difficult, wine and stimulating expectorants may be equally useful; but many such cases really require neither the one nor the other;—they may be withdrawn, as in the present cases, on careful observation of the symptoms, without inconvenience; and they ought not to be urged, in any such case, to such a degree as to affect the circulation materially, unless we see distinctly, by cautious trial, that the state of the circulation improves under their use.

If we are satisfied, as I think we may be, that in these circumstances, *when inflammatory effusion extends rapidly over the pleura and lungs, without any or but slight affection of the pulse or breathing*, whether this is a state of matters (as in these cases) which has spontaneously arisen,—or whether, as in cases to which I previously alluded, it is consequent on moderate bleeding and other antiphlogistic means,—the resources of nature for its abatement and rapid absorption may be safely trusted,—we may, of course, be very sceptical as to the alleged powers of any remedies, especially such as produce no sensible effect, in promoting that healing process. But all this teaches us nothing as to the real efficiency or importance of full blood-letting in pneumonia, as inculcated by Cullen and Gregory; simply for this reason, that those cases could not by possibility have been called pneumonia by these authors. They would have been called, no doubt, cases of catarrh, or at most threatenings of pneumonia, subsiding under an antiphlogistic regimen,—as it is admitted on all hands that all inflammatory diseases very frequently do, when within a certain grade of intensity.

These observations may be applied to the treatment of pneumonia by va-



rious means, which have little or no sensible effect on the body,—*inter alia*, by inhalation of chloroform, as lately recommended in Germany, and stated to have been employed in above 200 cases of acute pneumonia by Waucherer and Baumgartner, and by Varrentrapp,—simply by inhalation, repeated at intervals of from two to four hours, with remarkable success; although it is fairly stated by Varrentrapp, that two of his twenty-three cases, fully detailed, were bled, and two others treated by blisters and mercury, and that although he had only one fatal case under the use of the chloroform, he admitted during that time three others which were fatal, and in which it was not given on account of their severity or complications. See "Medical Times," new series, vol. iii. p. 414. The true mortality in the cases admitted under his care, therefore, was four in twenty-six. From what I have seen of the use of this remedy in some cases of severe bronchitis and asthma, I believe it to be one possessed of real efficacy in checking, at least, one form of inflammation, and promoting expectoration from the extremities of the bronchia; although its effect seemed to me, in these cases, to be very transient. But when it is stated that this is a remedy of such power that cases of pneumonia, trusted entirely to it, recover, on an average, as satisfactorily as under any other treatment,—and when statistical evidence in proof of this assertion is brought forward, investing the subject with the authority of the exact sciences, it becomes necessary to examine the grounds of these assertions somewhat more closely; and in this, as in all other cases where statistics are applied to the elucidation of our views in regard to the treatment of any given disease, the first question is, Are we sure that the terms employed denote the same things? In the present case, admitting, as I have no doubt we ought to admit, that all the cases termed pneumonia by existing authors are correctly named now, it is still quite possible that very few of them would have had that name in former times, and extremely probable that hardly any of those which were thus treated in 1851, would have been regarded by the best authorities in 1780, or even in 1810, as cases exemplifying the circumstances of pneumonic inflammation, which were then stated to involve much danger, or to demand full bleeding. Any comparison, therefore, between the treatment of the nosological pneumonia of former days and the pathological pneumonia of the present, is vitiated by the uncertainty whether we are comparing similar cases. All the statistical information that we have does not inform us whether, in any one of the cases there treated, there were the "frequent, full, strong, hard, and quick pulse," and other marks of the phlogistic diathesis. The only information as to the pulse is, that on an average of all the cases it was at or below 80 on the fifth day after admission—i. e., the ninth day of the disease; and as the mean age of the patients was not more than thirty-one, this is not a frequency of pulse which denotes any great intensity of fever. Neither are we told whether in any one case the patient could "lie on one side only, or only on his back, or could breathe only in the erect position, or whether, even in this posture, his breathing was very difficult, and attended with turgescence and flushing of the face, with partial sweats about the head and neck,"—which constitute the combination and rapid succession of symptoms, regarded by Cullen as denoting the danger of pneumonia. (First Lines, § 352.) If such cases occurred in the practice of the German authors who recommend the chloroform, and were witnessed in their early stage, making rapid progress, and the remedy withheld which, since the time of Hippocrates, has been regarded as our *summum remedium* in such cases, in order to make trial of the efficacy of a remedy not known till 1851, I can only say that the ideas of those gentlemen as to professional morality are widely different from mine,—so different that I cannot help feeling some distrust of their records of cases. But if, as seems to me much more likely, their cases of pneumonia were in general more analogous to those described in this paper,—if the pulse at the period of greatest intensity of the disease, when the inflammatory effusions were

rapidly extending, was generally soft, the face pale, the skin cool, the circulation easily depressed, as by assuming the erect posture, the breathing not urgently difficult, or rather hurried than laboured (on all which points the statistics tell us nothing),—it is in the highest degree probable that those cases would have gradually got well, as the cases here recorded did, without the use of any remedy beyond the rest, regulated temperature, and low diet, common to all febrile diseases treated in hospitals,—to which the result could reasonably be ascribed ; and the chloroform may be safely regarded as having been only a palliative.

This is, in fact, only one out of many cases which might be adduced to show, that the difficulty as to the real power of alleged remedies over disease is seldom of a kind to be surmounted by the help of statistics,—certainly not by striking averages of patients treated by one method or another ; simply because, in mathematical phrase, the *value of unity* is not known to be the same in the cases compared ; they may have varied in many other particulars besides the use of the remedy under trial in one set of them and not in another.

This observation extends to many other statistical statements which have been brought forward in proof of the efficacy of the homœopathic or of various other forms of expectant treatment in inflammatory complaints. It is not sufficient to know that two sets of cases which we compare, are, on an average, seen from the same period of their disease, that their average age is the same, that the average known lesion of structure which they involve or threaten is the same, and its average extent the same, even that the average frequency of pulse and frequency of inspiration is the same, and that the average amount of complication of the proper symptoms of the disease, with other diseases, or with affections of other parts, takes place. All these things may be, and yet these cases may have differed from one another most materially—the value of unity in comparing them may have been very different, independently of the use or disuse of any particular remedy. In particular, we must always remember, that it is only in particular circumstances of individual cases that any powerful remedy is thought, by those who are most satisfied of its efficacy, to be useful ; and the more powerful it is in those, the more chance there is that, in other circumstances of the same cases, its effect may be injurious or fatal. The judgment of the practitioner is an element of the highest importance in the history of any case in which a remedy of alleged efficacy is tried ; and this judgment is often determined by observations which he makes on the progress of the case, but which cannot be expressed in figures—*e. g.*, by what he can observe of the degree of strength and *endurance* of the pulse, the condition of the skin as to endurance as well as degree of temperature, the marks of muscular strength in the movements, even postures, of his patient, in the tone of his voice, the expression of his countenance, and other indications of the state of his sensations and thoughts.

Such cases as those here recorded show that inflammatory lesions in internal parts, of the very same kind, and, as far as any means of judging that we possess inform us, of the very same extent, which in some cases are attended by the urgent symptoms of inflammatory fever, pain, and dyspnoea, quoted from Cullen, may take place in others, with a very different set of constitutional symptoms, sometimes almost without fever, at other times with adynamic or typhoid fever ; certainly without the symptoms either of inflammatory or hectic fever ; and a little further observation will show, that the *mode of fatal termination* in these different cases of the same disease (as indicated on dissection) may be very considerably different. And if it be true, as now maintained, that these differences exist, not only in individual cases, but in the usual course of the same disease,—*i. e.*, of the diseased action which leads to the same lesion of structure,—at different times and places,—it is plain that very numerous and careful observations, supposed to be made on subjects exactly similar, may have really related to very different kinds of diseased action.

The general inference which may be drawn from these considerations is one

which might be easily supported by other arguments ; that the question of the efficacy of any powerful remedy over the progress of any case of disease is usually a much more complex one than most questions in etiology, as to the power of a given cause to excite disease ; or than some questions in pathology, as to the frequency or extent of certain lesions, or complications of lesions ; which may be satisfactorily solved by statistics. In judging of the power of a remedy, we may be aided by other means,—by observing its effect on the healthy body,—by reflecting on the pathology and mode of dangerous influence of the disease to be combated ; above all, by watching the change of symptoms in individual cases under its use ;—but remembering the sources of fallacy depending on the spontaneous decline of diseases, the influence of various external causes besides the remedy in question, the varying judgment of practitioners, the varying nomenclature of diseases, and especially their varying type at different times and places, of which I am now recording examples,—we can easily perceive that it is very seldom that, in such inquiries, we can have the value of unity so defined as to enable us to draw any inferences on that subject, susceptible of useful practical application in individual cases, from multiplying numbers or striking averages.

In the two first of the following cases we had all the local, or what are now usually called the physical, symptoms which could have presented themselves if three-fourths or more of one of the lungs had been disqualified for its office by rapid inflammatory effusion, both on its surface and in its substance. In the first of these, which I did not myself see till she was convalescent, but which was carefully and daily examined by my predecessor in the ward, Dr Bennett, the effusion evidently commenced while she was in the ward, although at a somewhat advanced period of the disease, and made rapid progress while she was under daily observation (particularly between the 28th July and 1st August). In the second, there was good evidence that it had taken place within the week preceding admission (*i.e.*, between the 12th and 19th September). In both, all the indications of the inflammatory effusions in the sputa, and in the sounds of percussion and auscultation, subsided rapidly within a fortnight after they were at their height ; and certainly in neither of these cases were the indications in the state of the pulse and dyspnoea, described by Cullen, as the indications of dangerous pneumonia requiring full and repeated bleeding, ever present. Indeed both seemed to me good examples of the principle which I have long laid down, in treating of the practical applications of the local symptoms, in the state of the sputa and in the sounds of the chest, indicating inflammation of the pleura or lungs, which have been so fully and satisfactorily made out of late years, especially by the French pathologists,—*viz.*, that we may place great reliance on these symptoms, as indicating the presence, and in many cases the extension or abatement, of inflammation of these textures ; but that we cannot trust to them at all, as indicating the amount, or even the proper time, of depletion which the cases require ; and that, while we regard those symptoms as the most decisive that we possess, for marking the nature and extent of the disease (considered pathologically), we must still look to the marks laid down by Cullen and others of the older authors, in the state of the pulse and the breathing, and to the endurance of these symptoms under the antiphlogistic regimen and remedies, as our warrants for using or withholding active remedies.

CASE I.—Mary Hodges, æt. 38, admitted evening of 24th July 1852, of moderately robust habit, although having nearly the colour of a mulatto. States that she has generally enjoyed good health till the 17th, when, after exposure to cold and wet, she was attacked with rigors, pain in the head, limbs, and abdomen, cough, and expectoration of a tenacious character, and of a reddish and green colour. These symptoms continued slightly up to the date of admission, when she had no dyspnoea and no marked pain in the chest. On percussion and auscultation, sounds appeared healthy. She complained of pain in the head, back, and extremities ; tongue loaded with a dry brown fur ; appetite

impaired ; bowels not opened for the last six days ; urine turbid, spec. grav. 1020, acid, not coagulable ; she sleeps badly ; skin dry and harsh, but without appearance of any eruption ; pulse 80, of good strength ; heart's sound and cardiac dulness natural. To have purgative enema and sinapism to the epigastrium. *26th July.*—Bowels freely moved this morning ; pains in head, back, and abdomen much relieved. *28th July.*—Cough has been worse for the last two days ; there is now some dyspnoea and pain in the left side posteriorly ; the expectoration is more profuse, quite viscid, and of a dirty green colour. On percussion, there is now well-marked dulness over lower third of left lung posteriorly. There is distinct crepitation in the lower half of same side posteriorly, fine at the base of the lung, but becoming subcrepitous towards the angle of scapula ; the vocal resonance is increased in the same part ; the respiration of right side is free, and approaching the puerile character ; pulse 80, of good strength ; more appearance of debility. R. Sol. Antimonial, ʒj. ; Aquæ, ʒiij. M. St., ʒss. q. q. 4ta hora. *Vespere.*—Vin. Rub. ʒiv. *30th July.*—There is now complete dulness over the left side posteriorly, as high as the scapula ; vocal fremitus very distinct, and resonance of voice increased. There is distinct crepitation to be heard as high as the supra-scapular region ; dyspnoea increased ; expectoration unchanged ; pulse full, 88. Appl. vesicat. 4 × 5 lateri sinist. Omit the wine ; low diet ; antimony continued, with gt. vj. of solution of morphia in each dose. *31st July.*—Slight sickness from medicine ; breathing much easier ; bowels open ; skin of natural temperature—moist ; pulse 70, of moderate strength ; expectoration still viscid, and tinged red, but diminished in quantity. *1st August.*—Slight nausea from medicine, but she feels much better ; there is no change on the left side, but there is distinct crepitation and partial dulness in the lower part of the right side ; pulse 64, soft, and rather weak ; urine clear ; bowels open. Had ʒiij. of wine daily from this time. *3d August.*—Feels greatly better ; the right lung is quite free, and no crepitation is to be heard above the sixth rib on the left side ; but there is still dulness and increased resonance of voice in the lower third ; still some viscid expectoration, but free from blood ; the tongue is clean ; appetite good ; sleeps well ; pulse 64, of good strength ; bowels open ; urine contains a copious lateritious sediment. Omitantur medicamenta. From this date she continued gradually improving in general health, although no change could be perceived in the lungs for several days. Both crepitation and dulness on percussion *had, however, disappeared* by the 13th, and she was dismissed cured 17th August 1852.

CASE II.—Mary Ann M'Donald, æt. 20, wife of a soldier, admitted September 20th 1852. States that, previous to the present attack, she has never been confined to bed with sickness, but has suffered occasionally from cough and slight pains in the chest, being frequently exposed to cold and wet feet. This cough was not affected by the state of the atmosphere. On the 11th September 1852, after a heavy washing the previous day (when she had been exposed to cold while perspiring), she began to complain of general uneasiness and nausea, but no cough or pain in the chest. On the following day the restlessness and uneasiness continued, and in the evening she had repeated rigors, with severe headache and nausea. These were followed by pain in the chest, dyspnoea, and a constant harsh cough, accompanied by thick viscid expectoration, so completely stained with blood, as to be mistaken by the patient for hæmoptysis, but there were no coagula. She applied for medical assistance, and thought herself considerably relieved, but was not bled either generally or locally. Had a sinapism applied to the chest, but no further counter-irritation. On the night of the 18th (the seventh after the rigors), she states that she had profuse diaphoresis, and awoke in the morning feeling much better, and the breathing easier. Since then the symptoms have continued to improve. On admission, appears of a moderately strong constitution, but at present rather exhausted ; no marked dyspnoea ; no lividity of face ; general surface cool and moist, but not clammy ; no eruption visible. She complains of headache ; general soreness of trunk and extremities, and exhaustion ; no pain in back, but dull aching pain

in the chest, increased by cough, which is still frequent, but not so harsh as before admission, and accompanied with expectoration, chiefly viscid, gelatinous, slightly tinged with blood. On external examination, the chest seems well-formed, and both sides expand equally in upper and fore part, but in lateral and posterior regions of right side the expansion is much impaired. On examination, cardiac impulse feeble; apex of heart beats in natural position; dulness and sounds normal; pulse 66, rather weak, but regular. Chest, anteriorly, normal on percussion, with the exception of slight dulness at lower part of right lung; respiratory sounds on right side rather feeble; a friction murmur is to be heard with expiration at the lower and lateral part of same side. On left side, anteriorly, respiratory sounds rather puerile; vocal resonance natural. Posteriorly, left side normal on auscultation and percussion; upper part of right side also natural; but there is on this side *dulness from about the fourth rib to the base of the lung*, with increased vocal resonance in some points, approaching in character to oëgophony; and about the lower angle of the scapula, a distinct double friction murmur is to be heard; respiratory murmurs in these parts generally inaudible, but there are no crepitous râles to be heard; there is cough, with slight expectoration of a tenacious viscid mucus, slightly tinged with blood, but no dyspnoea; complains of severe pain at the parts where friction murmurs are heard on full inspiration; tongue florid, slightly furred, dry and rough in the centre, tip and edges moist; great thirst; complete anorexia; no pain or difficulty in deglutition; bowels at present regular, but they were rather constipated previous to her admission. Catamenia have always appeared quite regularly; urine passed naturally and in normal quantity, spec. grav. 1021, not coagulable either by heat or nitric acid. Nervous and integumentary systems quite normal.—[Thos. Russell, *clinical clerk*.]

This patient had a blister on the right side on the night of admission, and took on that night and the next day five or six doses of  $\frac{1}{4}$  gr. of tartar-emetic. The pain having abated, and the pulse being weak, not frequent, she had *℥ij.* of wine daily from the morning of the 21st, and no other treatment. The expansion of the right side of the chest quickly improved, and the sounds gradually became more natural, so that, on the 2d October, the sound on percussion, in the back part of that side, was hardly different from that on the other; the resonance of voice nearly gone: and the respiratory murmur, although faint, was distinct, and without râle, even to the lowest part.

The following case was seen only at a more advanced period of the disease, so that we cannot speak with certainty of the nature of the symptoms during the early stage of the inflammation.

#### *Abcesses of Liver.*

CASE III.—Mary Turnbull, æt. 45, labourer's wife, admitted 20th August 1852. States that this, her first serious illness, commenced about a fortnight ago, and is attributed by her to exposure to cold during menstruation. The usual duration of the discharge is one week; but on this occasion it became suddenly suppressed on the fourth day, accompanied with rigors, pain in the back, headache, and general uneasiness and debility. Three days afterwards she suffered from severe pain in umbilical region, where, in a few days (eight days before admission), she felt a small tumour, not larger than a pigeon's egg, but which has rapidly increased since that time. For the last fourteen days she has vomited everything taken into the stomach, and has suffered from constant painful diarrhoea, previous to which the bowels had not been open for eight days. The vomiting and diarrhoea have continued without abating; she has been under medical treatment, and leeches have been applied to the epigastric and left hypochondriac regions. On admission, she appears naturally of a rather feeble constitution, and is considerably emaciated, with a sallow cachectic expression, but no yellowness of the face or eyes. She complains principally of acute pain in the epigastric and left hypochondriac regions;



constant vomiting, with severe diarrhoea ; stools dark coloured and watery, with some mucus ; the tongue is moist, but coated with a thick brown fur in the centre ; great thirst ; no appetite. There is a distinct tumour, about the size of a large orange, in the lower part of the left side of the epigastric region, extending to the left hypochondriac and umbilical regions. The surface over the tumour is exquisitely tender, and she complains of great pain in the swelling, so that it cannot be percussed ; right hypochondrium distended and dull on percussion, but not tender ; the catamenia have always been regular up to the present time ; there is some difficulty in micturition and dysuria, but quantity quite normal ; urine rather dark in colour, with copious mucous sediment, spec. grav. 1018, not coagulable ; pulse 104, weak and thready, but regular ; slight palpitation, but cardiac sounds normal ; surface rather chilly and damp ; no eruption visible. She complains of no cough, but there is some subcrepitus under the left clavicle, with slightly prolonged expiration and increased resonance of voice. *R.* Calomelanos, gr. j. ; *Opii*, gr. j. ; *St. Pil.* tal. j. ; 8va. q. q. hora. *Hab.* Supposit. *M. Morph.*  $\frac{1}{2}$  gr. ; *Vini Rubri*, ʒij. *21st August.*—Tumour fully as prominent, rather diffused, dull on percussion, but not so tender ; more pain in epigastric region ; she slept better last night ; still has vomiting ; ingesta returned unaltered ; diarrhoea much less ; expression of countenance improved ; pulse 108, firmer ; skin pretty warm. *Appl.* hirudines, viij. *Epigastri* Injicietur Enema domest. *22d August.*—Had a stool after the enema yesterday, partly fæculent, with shreds of mucus ; at the same time, the tumour became much diminished in size, but has since enlarged again, and is fully as tender as formerly, but pain in epigastrium is relieved ; tongue moist ; pulse 108, small and sharp. *R.* Calomel, gr. vj. ; *Opii*, gr. iv. ; *Conserv. Ros.* q. s. ut fiant *Pil.* iv. ; *St. j.* 6ta q. q. hora. *Appl.* hirudines, vj. tumori. *Vespere.*—Injicietur Enema. *23d August.*—Had a stool last night, watery, but more natural than before ; pulse 96, small ; vomiting still continues ; felt very faint last night after the leeches ; less pain in epigastrium to-day, but tumour is larger, and feels more prominent and elastic ; she had some sleep during the night ; complains of pain from enema. *Appl.* Cataplas. Emollient. Tumori, et hab. Enema Vespere. Cont. alia. *24th August.*—Had a fluid bilious stool after the enema ; still tenderness in the epigastrium, and vomiting unabated ; the tumour is still tender, more diffuse, less dull on percussion, fluctuation perceptible in its centre ; less pain on breathing or coughing ; pulse 90, rather firmer ; tongue furred ; feels relief from camomile fomentations. *Appl.* statim hirudines vj. abdomini, repetatur Enema vespere. *25th August.*—Had two stools after the enema ; slept a good deal ; still vomits mouthfuls of bilious matter without any effort ; fluctuation of tumour somewhat extended ; whole of upper part of abdomen distended, fluid perceived in the lower part ; owing to the weakness of the patient, the leeches were not applied ; pulse 90, small. *Repetat.* Enema, Vespere. *Vespere.*—*R.* Aq. Ammon. Ac., ʒj. ; *Sol. M. Morph.*, ʒiiss. ; *Aquæ*, ʒviij. ; *St.* ʒss. 3ta q. q. hora. *Hab.* Enema ē. *Sol. M. Morph. M.* xx. *26th August.*—The tumour was punctured to-day with a small trochar, and about ʒij. of serous fluid, with a slight appearance of pus, drawn off. She feels rather weaker to-day ; pulse 100, thready ; vomiting still continues, accompanied with pain in the epigastrium. *Rep. Pil.* Calomel. et *Opii* ; et repetatur Enema hora Somni. *27th August.*—Had a dark stool since yesterday ; no further discharge from the puncture ; pulse very feeble ; surface chilly, and covered with clammy perspiration ; vomiting still returns on making the least exertion. *Hab.* Sp. Vin. Gal., ʒij. She died at five p.m., 27th August 1852.

*Sectio Cadaveris, 28th August, nineteen hours after Death.*—Surface of body pale ; considerable lividity ; in the left hypochondriac region a globular swelling is seen, soft, elastic, and evidently containing fluid, and about the size of a cocoa nut

*Abdomen.*—Liver much enlarged, extending from the fifth rib to midway between the crest of the ilium and the last rib—laterally, the *left lobe* extends



into the left hypochondriac region, where it corresponds with the tumour already mentioned; it is firmly adherent to the peritoneum of the abdominal wall; posteriorly, it is attached by similar adhesions to the anterior and upper surface of the stomach; on separating this attachment, the cavity of the tumour was opened, and a large quantity of yellow, partly fluid partly curd-like, purulent matter immediately escaped. On separating the posterior attachment of the right lobe, another large cavity was opened, containing matter exactly similar to the other. On removing the organ, another smaller abscess was found in the upper and anterior surface of the right lobe. The substance of the liver was pale, but healthy. The abscess in the left lobe occupied nearly its whole extent; that in the right, of a similar large size, was completely surrounded by the substance of the liver.

*Kidneys* and other abdominal viscera healthy.

*Thorax*.—Slight adhesions in right pleura at its upper and back part; along the greater part of the posterior aspect of the left, much more strong adhesions are found.

*Lungs* slightly emphysematous, otherwise healthy.

*Heart*.—Aortic valves considerably thickened; the interior of the aorta roughened by atheromatous deposit; other valves, etc., healthy.—[From the Journal of No. XI.—Mr Russell, *clinical clerk*.]

The absence of any of the usual marks of liver disease, the appearance of pretty healthy bile in full quantity in the stools, and the almost complete limitation of the acute pain and tenderness to the *left* hypochondrium, where the tumour, and afterwards the fluctuation, were perceived, together with the small feeble pulse, the cold damp skin, and the incessant retching, so far deceived me, as to lead me to suppose the case one of peritoneal inflammation, supervening on chronic enlargement of the liver, and causing adhesion of several of the viscera, and an effusion of pus circumscribed by these adhesions; but on dissection it appeared that, although all the peritoneal surfaces in the left hypochondrium were adherent, the collection of purulent matter (containing many irregular fragments of curdy matter) was entirely within the substance of the liver, and was only one of three large abscesses which it contained. I consider the case important chiefly on this account, that the rapid enlargement of the fluid effusion, constituting a large abscess, which occupied nearly the whole of the left lobe of the liver, and most probably the formation of two other abscesses likewise, occupying together fully one-third of the substance of the right lobe, were distinctly perceived to take place during the time of the patient's stay in the hospital, between the 23d and 27th August; and coincided with such a state of the circulation as usually attends peritoneal rather than hepatic inflammation; the pulse being from 90 to 100, always small, generally so feeble, as I thought, to demand stimuli and opiates, the skin pale, cool, usually damp and clammy, and, in fact, the faintness and sickness such as were expected to prove fatal, and I have no doubt but for the stimuli and opiates given, would have proved fatal, as early as the time when the fluid effusion was commencing; so that there could be no doubt, that this rapid and extensive inflammatory action co-existed with a most depressed state of the general circulation.

Still it may be said, that all this is no proof of any material *change* having taken place in the constitutional affection usually attending internal inflammation. It may be said, that we have evidence of such disease taking place now with slight or with typhoid fever,—not of its having really required so active treatment formerly; that the mild expectant treatment, which was successful in the two first of these cases, was really all that was required in the cases alleged to have been attended with the more sthenic forms of fever, and to have demanded much more active remedies, in former times; and that it is only a *conjecture* to assert, that the disease has undergone such varieties in the course of time as to be now usually managed under a different course of treatment. In answer to this, I would observe, that the description given by Cullen, of the state of the pulse and breathing, demanding full and repeated bleeding,—his ge-

neral description of the phlogistic diathesis "attending any considerable inflammations;" and especially the earnest warning given by him and by Gregory, that the condition of those functions, although relieved for a time, *usually recurred* after one or more bleedings, and only finally subsided after several (from six to eight, was by no means an unusual number), are facts about which a practitioner of any experience and discernment could not be deceived; and I am only one of many practitioners, now living in this country, who are convinced that such cases frequently occurred from twenty to forty years ago;—more frequently in dispensary and private practice, than in hospitals,—because the patients were seen earlier,—and that they hardly ever occur at the present day.

For example, many pupils of the late Dr Gregory will remember his describing the case of a gentleman, then practising medicine in Edinburgh, who was under his care for pneumonia in the winter 1810-11, who had been bled, before Dr G. saw him, by his own brother, and at his own urgent request, three times within six hours, to the amount in all of 102 ounces of blood—i.e., there had been three bleedings, averaging 34 ounces each. Now in this case there could be no doubt as to the facts, that the patient—a man of intelligence and observation—expressed extreme anxiety, not only for the first, but for the last of these bleedings, and experienced signal relief from each, being certain, to use his own expression, that but for each of these he would have choked in a very few hours. It is equally certain, that when Dr Gregory saw this patient, after these large bleedings, the state of his pulse and breathing were such that, after a short delay, he thought himself justified in directing a fourth bleeding, to 25 ounces, within less than twenty-four hours after the first of the former bleedings, making in all 127 ounces taken within twenty-four hours; and I am happy to add, that this gentleman was not only immediately relieved, and in his own opinion and that of his medical friends, saved at the time, but so little affected by these bleedings, that he resumed his ordinary occupations within a few weeks, and has been engaged in the active duties of his profession from that time to this—more than forty years.

Now I would ask any practitioner, who has attended carefully to the effects of blood-letting practised within the last ten or fifteen years, three times within a few hours, to half the extent above mentioned, on account of inflammation in the lungs,—Whether he has seen any patient in these circumstances whose breathing has appeared to demand, or whose pulse has so rallied as to justify, further bleeding within twenty-four hours from the first? Or whether he has not observed, after such bleedings, positive indications of enfeebled circulation, in softness of the pulse, cool damp state of the skin, faintness on sitting up, etc., sufficient to warn him that this remedy had been carried, at least for the time, as far as was safe?

Another remarkable case was frequently quoted by Dr Gregory, in which the symptoms of pneumonia, always relieved by bleeding, returned so frequently, and always with such strength of the circulation, that the patient lost in all 392 ounces, at sixteen bleedings, during three months that he was confined, and made likewise a complete recovery. The general observation with which Dr Gregory used to conclude the recital of these and a few other cases of enormous loss of blood successfully directed in inflammatory disease, illustrates what I have stated, as to a form of such disease at least frequently met with in those days. "Remember, I do not quote such cases as models for imitation; but you must expect that, in most cases of well-marked pneumonia, the symptoms will recur, after the first and second bleedings; and when they recur more violently or more obstinately than usual, it is of great importance that you should know, how far the bleedings have been carried, and how often they have been repeated, in some such cases with perfect success."

Farther, I can say, not for myself only, but for many other practitioners with whom I have conversed on this subject, and whose experience stretches back from 30 to 40 years, that the facts which we have observed, and which have limited the use of this remedy in our practice of late years, are such as could

not have escaped observation, if they had occurred frequently in connection with inflammations within the chest, at the same period of the disease in former times.

I may mention, in particular, two cases on which, from particular circumstances, my attention was specially directed, where we had the symptoms of pneumonic inflammation, attended with a feeble state of the circulation, almost from the first; the amount of blood taken from them did not exceed 3xij. in either case, after which, although the breathing continued short and somewhat laboured, the pulse was so soft, and the febrile symptoms quickly became so distinctly typhoid, with muttering delirium and stupor, that, in anxious consultation of different medical friends, the only practice which could be farther attempted included the use of stimuli, as in typhoid fevers. Both these cases were fatal, but fatal by coma, with feeble circulation, as typhus fever usually is, rather than by asphyxia; and both showed, on dissection, the unequivocal appearances of hepatisation (not merely condensation or "carnification"); but occupying so small an extent of lung, in comparison with what I have seen in many other pneumonic cases, that we could not suppose that the whole cause of death lay there, nor regret that the loss of blood had been to so small an extent.

On the other hand, in watching the progress of cases of pneumonia that I have seen of late years, since my attention has been particularly drawn to this subject, and which have done well, with what I should formerly have regarded as inadequate depletion, often followed by the prompt use of stimuli,—I have been equally certain, that the state of the circulation and the breathing would have deterred from full and repeated bleedings any practitioner who had duly reflected on one of the favourite maxims of Dr Gregory, that "*the vital indication*," by which he understood the use of stimuli, when the *vis vitæ* in the circulation was failing, "*supersedes all others*." In one case, in a young, strong, healthy man, seen last winter, although the symptoms were unequivocally marked, the local indications extending to the greater part of one lung, and one bleeding to about sixteen ounces gave immediate relief, the pulse rallied so little after it, that I hesitated about a second bleeding,—and should certainly, at any time of my experience, have thought a third bleeding inadmissible; but after the faintness from the second bleeding had abated, the breathing was permanently relieved, and the convalescence went on perfectly well, the quantity of blood lost having been less than thirty ounces.

In another case, of a middle aged lady, of spare habit, but previously healthy, in which the local symptoms, by percussion, auscultation, and the state of the sputa, indicated affection of at least one half of the right lung, and which had been seen by a medical man from the second day, there was so little strength of circulation, that we had some difficulty in ascertaining the state of the sounds in the back part of the chest, simply from the inability of the patient to sit up, so that two applications of leeches, which took in all not more than twelve ounces of blood, one blister, and small doses of tartar-emetic, continued for three or four days, but not urged so as to cause vomiting,—were the whole extent of the antiphlogistic remedies employed; these were alternated, after the fifth or sixth day, with small doses of wine; and although the febrile symptoms never showed the typhoid affection of the nervous system, they had so distinctly the adynamic form of affection of the circulation, and so distinctly improved under these small doses of wine, that no one who attended closely to the progress of the case, could doubt that the convalescence was accelerated, if not secured, by their use.

I have lately watched the progress of another case, in a young man of delicate health, but previously healthy, who had been treated up to the fifth day, of a well-marked pneumonic attack, by a homœopathist, and in whom we had all the indications which we can expect when at least half of one of the lungs has become impervious to air from inflammatory effusion. He had urgent

dyspnœa on the fifth, and again on the eighth day, which was quickly relieved, on the first day, by bleeding to about fourteen ounces,—on the second, by ten leeches; he had likewise a blister, and nauseating doses of antimony repeatedly for several days; but his pulse, after these moderate evacuations of blood, was always soft, his skin always pale, cool, and moist; and a small allowance of wine (not above three ounces daily) during the second and third weeks of the disease, while the natural sounds were very gradually returning to the affected lung, certainly caused no unpleasant symptom, relieved his uneasy feelings, and appeared distinctly to promote his recovery.

The greatest practical difficulty is in the treatment of cases of pneumonia where, although there is no evidence of contagion, nor any appearance of typhoid eruption, nor deafness, the pulmonic symptoms are attended, not only with a soft frequent pulse, but with typhoid affection of the nervous system. Such cases may sometimes be observed, as has been noticed in France, to affect the upper lobe of the lungs more than the lower; some of them graduate insensibly into the cases, somewhat slower and less regular in their progress, of acute tuberculosis affecting the lungs; some of them may be ascribed to the previous action of intoxicating liquors, or may be said to be complications of pneumonia with delirium tremens; but I have seen many within the last ten or twelve years, where the fever attending pneumonia took this form, without any such peculiar cause showing itself. I think I can add, that most of these have recovered; in one of them, still under observation,—a girl, aged twelve,—we had clear indication of extensive condensation of the back part of both lungs, and of much of the upper part of one; but from the gradual abatement of all the symptoms which has taken place during the use of small quantities of wine in the third and fourth week of the disease, I have good hopes that by the end of the sixth the natural sounds will be restored in all those parts of the chest; and the only practical observation I can make on such cases is, that they must be treated as we would treat unequivocal typhus fever, complicated with pneumonic symptoms;—by careful observation of their progress, more as regards the state of the pulse and breathing than as regards the extension of the disease, indicated by the sounds in the chest; sometimes by cautious blood-letting, more frequently local than general; sometimes chiefly by blisters, expectorants—nauseating or stimulating according to the state of the pulse, and anodynes; often by wine and ammonia, carefully alternated with, or succeeding to, these remedies; but always with the caution that, under rest, and low diet, and regulated temperature, the disease has a more or less definite course to a favourable spontaneous termination; and that while manifest contra-indications exist in the state of the symptoms, the smallest amount of interference, either in the way of stimulation or depression of the circulation, under which that progress can be seen to go on favourably, is the best that can be adopted.

The most important additional practical observation which results from the view of the subject which I have attempted is merely this,—that in all cases of this kind which present any difficulty, it is incumbent on us, just as in practising for contagious diseases, to attend to the “nature of the prevailing cases of the disease,”—i. e., to regard it as an established though unexplained fact in the history of diseases, that not only the febrile disorder excited by a morbid poison, but that also which results from a local inflammation, excited by cold or other sporadic causes, is liable to great varieties at different times and in different places; insomuch that it becomes dangerous to life in different ways, and may be most effectually counteracted by different means. Such an observation has, indeed, often been made before by Sydenham and other practical authors; but the distinctions which they have drawn have been too minute and desultory, and probably often too fanciful, to obtain very general assent; whereas I think we may regard it as certain, that the connection of the asthenic or even typhoid form of fever, with sporadic internal inflammations, and the rarity of the indications of a phlogistic diathesis attending those

inflammations,—or of the advantage or safety of full and repeated blood-letting,—is a general fact observed over the greater part of Britain, and now of many years' duration.

When I first became convinced of this fact, from my own observation as well as from the reports of others, particularly of physicians visiting different parts of the Continent, witnessing the practice of homœopathic hospitals, of Fleischmann's hospital at Vienna, etc., it seemed to me probable that the internal inflammations, treated with so little evacuation, would be very apt to leave permanent lesions of the parts affected, often scrofulous, sometimes malignant; and the part of the statements of those witnessing such practice which I was most inclined to distrust was the assertion, that the convalescence of the patients thus treated was usually more rapid, than that of patients with inflammatory complaints treated by fuller evacuation. But, on watching the progress of cases of the kind, such, *e. g.*, as the two first of those given above, I have been satisfied that the observation is correct,—the absorption of the inflammatory effusion in such cases, even when very extensive, as particularly in the first of those given above, having often been effected with remarkable rapidity, and the subsequent rapid recovery of strength having indicated that the blood, although it must have undergone a change in the course of the inflammation, had quickly recovered its natural properties. This may be connected with the observation which I have repeatedly made, that in such asthenic cases blood, taken from the arm, although showing the buffy coat, has neither shown it of the thickness nor of the degree of tenacity which, in former times, we should have regarded as characteristic of acute inflammation. From the rapid and complete convalescence of some patients, likewise, whom I should have thought predisposed to tubercles, from such inflammatory attacks, I am aware that those who deny all connection between inflammatory action and deposition of tubercle derive their best argument; and I would certainly yield to it so far as to allow, that when inflammation leads, as I believe it often does, to deposition of tubercles, this peculiarity of its effect must be ascribed rather to a morbid condition of the blood previously existing, than to any effect produced on the blood by peculiarity in the inflammatory process itself.

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## Part Fifth.

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### MEDICAL NEWS.

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#### EDINBURGH PHYSIOLOGICAL SOCIETY.

MEETING XVI.—*June 29, 1852.*—Dr BENNETT, President, in the Chair.

1. *Dr Bennett* demonstrated under the microscope, the pathological appearances in a case of softening of the brain. They did not differ from those which he had several years ago described, as constituting inflammatory softening; but Dr B. directed attention to the fact, that the vessels in the diseased parts did not present any granular degeneration of their coats, and that the granules and molecules were altogether outside the vascular walls. He was well aware that the larger vessels, and even such as were composed of two or three layers, might



undergo fatty degeneration; but he had never observed any facts which confirmed the doctrine, that the ultimate capillaries might be so affected. Their nuclei might, indeed, now and then be seen to contain a few fat molecules, but the delicate membrane of the capillary itself he had never been able to find converted into fatty granules. Hence, he could not persuade himself that the opinion of Mr Paget was correct, who attributed such appearances as he now exhibited to the Society to a fatty degeneration of the blood-vessels. On the contrary, he believed that the explanation he had himself given was the true one,—viz., that an exudation occurred from the blood-vessels, constituting inflammation, which exudation, instead of being converted into fibres as occurred on serous membranes, or into pus, as occurred on mucous membranes, was transformed into molecules and granules, and subsequently became associated with granular cells and granular masses in the manner he had elsewhere described.

2. *Dr Bennett* showed a number of cysts, closely resembling the urinary bladders of certain animals, which were asserted to have been passed from the vagina of a patient in the country. He promised to give the history of the case, with further details, at a future period.

3. A communication, from *Dr Haldane*, on cirrhosis of the liver, and descriptive of its minute anatomy, was read by the Secretary. *Dr Gairdner*, *Dr Sanders*, and *Dr Begbie*, were appointed to report. The author considered cirrhosis to depend essentially on the occurrence of an exudation, which, assuming the form of fibrous matter, compresses the various tissues of the liver, and, in particular, produces atrophy of the secreting substance of the organ.

4. *Dr Murchison* exhibited to the Society, a demonstration of urinary crystals, as to the exact nature of which he expressed some doubt. They had been found in urine, along with a large quantity of the ordinary octohedral crystals of oxalate of lime. They seemed to consist of a number of acicular crystals, radiating from a common centre, and forming a mass resembling a dumb-bell oxalate; they differed, however, from dumb-bell oxalates in not depolarizing light.

5. *Dr Murchison* exhibited a series of drawings, showing the structure of a tumour of the brain which he had examined, and thought to be of a cancerous nature, although none of the characteristic cellular elements of cancer could be detected in it. It consisted of fibrous tissue, with a large quantity of oily and albuminous granular matter, and a few small nucleated cells, with a diameter not exceeding  $\frac{1}{2000}$  of an inch. A morbid deposit had been found in the left kidney of the same patient, exactly resembling cancer when viewed by the naked eye; but on a microscopic examination, yielding elements not more characteristic than those of the tumour in the brain. *Dr M.* stated that he had examined many other specimens of undoubted cancer, in which none of the cellular elements, described as essential to cancer, could be detected, after careful and protracted examination.

*Dr Bennett* observed, that he also had examined the structure of this tumour, as well as of the morbid deposit found in the kidney. In both textures the foreign matter had evidently been present for a long period, and was in a state of disintegration. If, then, it was originally cancerous matter, the present case was one where cancer was in progress of spontaneous cure, a result which should only be admitted with great caution. On the other hand, it was very possible that the masses consisted of tubercular exudation, mingled perhaps with a few nucleated cells derived from the neighbouring serous or mucous membrane. He had always maintained the opinion, that it would be vain endeavouring to advance our knowledge of morbid growths, unless some definition was given to cancer, and all tumours composed of a certain structure were called by that name; whilst other tumours, however much they may resemble the former in external characters, yet, if they differed essentially in structure, should be distinguished from them. He felt satisfied that it was only in this way that the laws regulating these different kinds of growth could ever be determined, and that, confounding them together as had hitherto been done by practical surgeons, could only lead to confusion.



MEETING XVII.—*July 13, 1852.*—Mr BARLOW in the Chair.

1. The following report on Dr Haldane's communication was given in and read :—

The Committee appointed to consider Dr Haldane's communication on Cirrhosis of the Liver, beg to report, that the paper in question appears to them to contain the results of independent observation.

The Committee can confirm generally, Dr Haldane's descriptions of the cirrhotic liver. As regards the theoretical views, however, in this paper, the Committee are divided in opinion; but they consider the subject worthy of detailed inquiry, and suggest the following points for discussion and examination :—

1st, The dependence of cirrhosis on inflammatory exudation.

2d, The presence of adventitious fibrous tissue.

3d, The existence of hypertrophy as an essential part of the first stage of cirrhosis.

4th, The cause of atrophy in the latter stages.

5th, The condition of the different blood vessels, and ultimate biliary ducts, in commencing and in advanced cirrhosis, a subject on which Dr Haldane has not expressed any definite opinion.

(Signed)

W. T. GAIRDNER.

J. W. BEGBIE.

The discussion of the points above suggested by the Committee, was postponed to a future meeting.

2. *Dr Cobbold* then showed a dissection of a small aneurism of the ascending aorta which had burst into the pericardium; the inner surface of the pericardium was exceedingly roughened by a coating of fibrin, which had all the appearances of being an inflammatory exudation.

*Dr Dobie* stated, that the patient from whom the preparation was taken, was a patient in Dr Robertson's male ward, No. 5, in the Royal Infirmary. About a week before his admission, on the 15th June, he was seized with a violent rigor, followed by dyspnoea, and great pain in the region of the heart. At the time of admission, the beat of the heart was nowhere felt punctuate; there was a slight bulging in the third intercostal space of the right side. Friction sound heard during systole, diastole and interval; valvular sounds natural; cardiac dulness extended, but not greatly. The patient was treated, in the first instance, with antimonials, and with leeching and blistering of the region of the heart; he was then brought under the influence of mercury, and apparently with much benefit. On the 23d he began to suffer from diarrhoea, which was subdued by chalk mixture. On the morning of July 3d, the patient stated that he breathed more easily; he sat up in bed for a while, and died suddenly from syncope. All medicine had been omitted for some days.

*Dr W. T. Gairdner* remarked, that although in this case there were many of the clinical and pathological features of pericarditis, he was disposed to attribute to the lymph upon the surfaces of the pericardium, a different origin. In the history of the case, and in the pathological appearances, there was tolerably good evidence that soft adhesions, resulting from a moderate pericardial exudation, had existed, and that these had been forcibly torn up by the hemorrhage from the aneurism on the night of death. These adhesions were assumed to be the result of a previous acute inflammation of the pericardium; but all that was proved by the history, or by the post-mortem examination, was the existence of lymph and fluid in the pericardial sac, and the subsequent formation of adhesions, and removal of some of the fluid. Now, pathological observations showed, that when hemorrhage took place into a serous cavity, without causing immediate death, the secondary results were invariably the separation of the fibrin of the blood

from the serum, and its organisation upon the surface of the membrane. Dr G. had observed this both in the pleura and in the arachnoid, and had last year shown the Society an excellent specimen of a highly organised membrane in the latter situation, unquestionably of hemorrhagic origin. In the case of the pericardium, the constant motion of the heart would tend still more completely to defibrinate blood effused into its cavity; the fibrin would then assume the appearance of inflammatory lymph on the surface of the membrane, and the fluid portion of the blood might be, in part or in whole, absorbed. Such, Dr G. believed, was the real nature of some cases, at least, of the so-called "*hemorrhagic*" pericarditis, described by authors. How far these principles were applicable to the present case was a matter for consideration, and possibly not admitting of a positive judgment. But considering that pericarditis of this extremely intense kind was a rare disease, as Dr Taylor of Huddersfield had shown, with well-marked constitutional disorders, and, on the other hand, not at all prone to accompany aneurism of the aorta, Dr G. believed that a strong case existed for presuming that the pericardial effusion was in the first instance, as it undoubtedly was in the last few hours of life, hemorrhagic; and that the fatal bleeding from the aneurism was only the conclusion of a process which had been proceeding with intermissions for a considerably longer period. What gave strength to this view of the case was, that there was no trace of pus in the pericardium, or in the meshes of the soft lymph adhering to it, and that the more recent portions of the coagulum and fluid blood in the pericardial cavity, were likewise intersected everywhere by masses of decolourised fibrin, evidently separated by the churning action of the heart, even in the last hours of life.

MEETING XVIII.—*July 27, 1852.*—Dr BENNETT, President, in the Chair.

1. After the transaction of private business, the meeting proceeded to the discussion of the report on cirrhosis, postponed from last meeting.

*Dr Bennett* said, that he had examined numerous specimens of cirrhosis, of the hob-nailed, granular, and nutmeg livers, with the greatest care, and it appeared to him that the lesion partially consisted of an increase of the fibrous, and diminution of the cell, elements of the organ. Dr Haldane seemed to be of opinion, that the former depended on an exudation which was subsequently converted into fibres, but Dr Bennett had never been able to trace those transformations which are observed in an exudation undergoing fibrous transformation. Hence, he was induced to think that the growth was more analogous to that form of increased nutrition described as hypertrophy,—that is to say, slow and insensible increase of texture closely resembling the augmentation of bulk which occurs in the growing animal. He was aware that a membrane surrounding the lobules of the human liver had not yet been demonstrated to exist, but its presence was to be inferred by analogy; and it was a question worthy of consideration, whether the increase of fibrous matter in cirrhosis did not constitute a proof of its being there. With regard to the diminution and atrophy of the cell elements in the liver, it might be asked, whether this was a primary lesion, or a secondary one dependent upon the pressure and contraction of the surrounding fibres. It was very possible that both changes might occur together, but he ascribed the principal influence to the increase of the fibrous element. He had been further led to believe that the nutmeg liver was only an incipient stage of cirrhosis, indicative of the increased vascularity which preceded and accompanied the commencing process of the fibrous hypertrophy. Dr Bennett also described a lesion which closely resembled the nutmeg liver, and was commonly confounded with it, in which atrophy of the lobules proceeded from without inwards, those cells in connection with the portal capillaries having undergone fatty degeneration, while those in connection with the hepatic system were still normal. This lesion, he thought, might be a peculiar form of fatty degeneration of the liver.

*Dr W. T. Gairdner* said, that he had examined, with great care, numerous instances of cirrhosis of the liver, and would state some of the results of his observa-

tions to the Society, although, in many respects, he considered the pathology of the disease extremely obscure and unsatisfactory. In the first place, Dr G. saw no good reason for assuming that a preliminary hypertrophy was an essential part of the pathology of cirrhosis. It was quite true that a hypertrophied liver might in certain cases be the starting-point of cirrhosis; but in other instances the disease appeared to be unattended by increase of volume, even in its earliest stages. This fact was calculated to throw doubt upon the commonly received pathology, which ascribed the atrophy of cirrhosis to the contraction of a new fibrous tissue, the result of a sub-acute inflammatory exudation, or *adhesive inflammation*, as it was called by Dr Budd. Without affirming positively the nature of the process concerned in the production of cirrhosis of the liver, Dr G. considered that the essential anatomical condition of the disease was, undoubtedly, the atrophy of the glandular structure; and that the hypertrophy, or *apparent* hypertrophy, of the fibrous element, was probably a result of the loss of due proportion between these two tissues. He had observed, in one instance, where the fibrous tissue around the granulations was in particularly large proportion, that it showed under the microscope an immense number of very peculiar nuclei, which were possibly the remains of the obliterated glandular cells. Dr G. remarked that, according to his observations, there was no fixed relation between cirrhosis of the liver and any other primary form of disease of the organ. He had undoubtedly seen it apparently arising from the "nutmeg liver," as Dr Bennett had observed; but not less frequently it was found apart from that condition and in connection with other morbid states, such as the pure fatty degeneration, or the waxy liver. In other cases, cirrhosis seemed to occur without any appreciable morbid condition of the remaining glandular tissue. As to the dependence of this lesion on obstruction of the biliary ducts or ultimate portal circulation, as asserted by Rokitansky, Dr G. said he had not been able to come to a decided conclusion on this subject. The investigation was very difficult, from the fact that injection of the cirrhotic liver was generally unsuccessful, and the separate anatomical elements, or the limits of the acini, could rarely be made out in specimens, even of the incipient stage of this lesion.

The *President* said—Gentlemen, in bringing this session to a conclusion, I think I may congratulate the Society on having fully carried out the object which it proposed to itself. This, as you know, was the investigation by every possible means of the structure and functions of organised beings—embryonic or mature, healthy or morbid. The abstracts of our proceedings, which have from time to time appeared in the "Monthly Journal of Medical Science," must have sufficiently shown the tendency of our labours, although these, from their brief character, convey a very imperfect notion of the benefit which we derive from meeting together; considering also that demonstration of facts and a careful confirmation of new discoveries, determined by high magnifying powers, occupies our attention to no small extent, it must be remembered that much that passes here is not communicable. Hence the progress which is made is not to be estimated by lengthy memoirs, elaborate reports, or verbose discussions, but by that serious effort at investigating truths which characterises our transactions, and the instruction obtained by each member from the co-operation of his fellows. During the period that must elapse before we meet again, it cannot but happen that your attention will be directed to numerous subjects which you will endeavour to elucidate by observation and experiment. The results of your labours will, I hope, be communicated to this society next session. The progress of physiology demands all the attention and labour that we can bestow upon it, more especially when we regard it as the only basis for a rational therapeutics. Hence I regret to observe that some of our members have expended much energy and time in the cultivation of medical statistics—a study that will certainly in no way reward their exertions, and one which I regard in the present state of science to be wholly useless. If medicine be, as all confess, an imperfect art, if the causes of death are not ascertained in the vast majority of mankind,

and if the nature of most organic and functional alterations in the body has yet to be determined, how is it possible to further a knowledge of these unknown points by already accepting them as data, from which, in a concentrated form, further conclusions are to be drawn? It humbly appears to me that, in endeavouring to build up scientific truth in this manner, the gentlemen who do so are imitating the artificers who laboriously erect a house on a sandy foundation. I allude to this subject because such notions are in my opinion diametrically opposed to the object we have in view, which is to advance knowledge by investigating the essential nature of diseases, and by determining the laws which regulate them. The necessity of this investigation implies that these laws have not yet been determined; and every practitioner is aware that diagnosis, or the art of detecting particular diseases, is anything but perfect. But correct statistics can only be based on unmistakeable facts governed by these laws. As such do not yet exist in medicine, they must first be discovered; and this, I hope, you will agree with me in thinking, is our only true legitimate task.

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## Part First.

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### ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Illustrations of Laryngeal and Pharyngeal Diseases, which are frequently mistaken for, or associated with, Phthisis Pulmonalis.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and of Clinical Medicine, in the University of Edinburgh.

(Read to the Medico-Chirurgical Society of Edinburgh, November 24th 1852.)

THE pharyngeal and laryngeal complications of phthisis pulmonalis are much more common than is generally supposed, and I believe that to them ought to be ascribed many of those symptoms which are generally attributed to the pulmonary lesion. Occasionally I have known laryngeal disease alone, mistaken and treated for phthisis; and when phthisis really exists, I have seen much benefit derived from a local treatment applied to the pharynx and larynx.

My attention was first directed to this subject by the following case:—

CASE I.—On the 11th December 1849, Captain B. entered my room, to consult me regarding an occasional expectoration of blood, which caused him, but more especially his lady, much anxiety. He was a tall vigorous-looking man, between thirty and forty years of age, who had no cough or any complaint whatever, but from time to time had hawked up a small clot of blood about the size of a pea. On a few other occasions he had observed some mucous expectoration tinged or streaked with blood. His chest was finely developed, and its most careful examination failed to elicit anything abnormal. His appetite and digestive functions were excellent; and, as commandant of a depot in the neighbourhood of Edinburgh, he had never experienced uneasiness from his professional duties. After repeated examination, I had no hesitation in stating that the lungs and large vessels were perfectly healthy, and that I hoped the expectoration of blood would cease spontaneously.

The origin of the blood in this case appeared to me at that time to

be very mysterious. It was not florid. There was no reason to suppose it to be of pulmonary origin. There was nothing in his voice to indicate laryngeal disease. I did not examine the pharynx, not being then aware of the importance which ought to be attached to it. I was consequently left in great doubt as to the origin of the blood, and of the best means of removing anxiety from my patient. My uncertainty, however, was partly dispelled by the following case :—

CASE II.—I was requested by an assurance office, in July 1850, to examine the chest of Mr M., a merchant, aged about 30, who said he laboured under no kind of complaint, with the exception of occasional sore throat, and expectoration of mucus tinged with blood. He was tolerably stout, took long walks without uneasiness, and suffered from no difficulty of respiration or from cough. Repeated examination of his chest failed to elicit any physical sign indicative of pulmonary disease. I therefore certified that his lungs were healthy. In October 1851, this gentleman called upon me again for advice, under the following circumstances. The soreness of the throat had latterly increased, and considerable cough was induced, after which he spit up mouthfuls of purulent matter, frequently tinged of a red colour. He brought me some of this sputum to examine, which consisted of mixed blood and pus, of a dirty brick-red colour. Examination of his chest again convinced me that the lungs were unaffected; but in the interval I had paid attention to the writings and practice of Dr Horace Green, of New York; and I now examined his throat, when the cause of his symptoms was at once apparent. The fauces and upper part of the pharynx were studded over with nodular swellings, varying in size from a pin head to that of a pea. Many of them were bright red and fungoid in character, probably the origin of the extravasated blood, whilst considerable patches of purulent matter adhered to several parts of the mucous membrane. I applied a sponge, saturated with a strong solution of the nitrate of silver, to the affected parts. In three days he returned, having been much relieved, when the application was repeated. I have not seen him since.

These two cases convinced me that certain symptoms which have hitherto been considered as indicative of phthisis might have their origin entirely in the fauces, pharynx, and upper part of the larynx. The cough so occasioned, with the purulent expectoration, often tinged with blood, frequently so resembles that occasioned by phthisis, as not only to induce alarm in the minds of the patients, but frequently to mislead the medical practitioner. I have now met with many such cases, which have been mistaken for phthisis, and which have been treated for that disease without any effect, until local remedies were applied, when they, for the most part, disappeared, or became much better.

The following case illustrates still further the occasional similitude



of laryngeal disease to phthisis pulmonalis, and the erroneous treatment to which error in diagnosis may lead.

CASE III.—Margaret Dickie, a staymaker, æt. 25, admitted to the Royal Infirmary, Sept. 9th, 1851, labouring under occasional vomiting, frequent cough with hemoptysis, and copious purulent expectoration. There was considerable sweating at night, and her general health, owing to want of sleep and the harassing cough, was much broken down. At the commencement of the winter session in November, I found her taking an acid mixture to relieve the sweating, a cough mixture to diminish the cough, together with cod-liver oil. The chest had also been blistered. Careful percussion and auscultation convinced me that the thoracic physical signs were perfectly normal. I then examined the fauces, which were covered with purulent mucus, but presenting here and there red and prominent follicles. The cough was also ascertained to be convulsive, the voice hoarse and broken, and, on placing the stethoscope over the larynx, a loud ringing sound accompanied the inspiration. From these facts I had no difficulty in diagnosing laryngitis; and on ascertaining that the woman was a prostitute, and addicted to drink, there could be little doubt that it was of syphilitic origin. The fauces were freely touched with a solution of nitrate of silver (3ss. to 3j. of water). This was repeated on the following day, and on the next the upper part of the glottis was touched, causing severe convulsive cough. I subsequently passed the sponge, saturated with the solution, into the larynx every second or third day during the month of November, which at first caused very severe and prolonged convulsive cough, that gradually became somewhat diminished. On the whole, however, no great amendment was produced, although the expectoration and cough during the intervals were lessened. The local applications were then suspended, but it soon appeared that they had been beneficial in checking the symptoms, from their severity again increasing, especially the amount of expectoration streaked with blood, and the want of sleep at night, owing to the severity of the cough. In the second week of December, therefore, the topical applications were resumed, together with occasional blisters to the larynx, and once more a certain amount of benefit was obtained. But as this treatment, combined with the internal administration of iodide of potassium and bitter infusions, for a period of four weeks, seemed to produce no further improvement, she was dismissed on January 7th, 1852.

In this case all the symptoms of phthisis pulmonalis were present, including emaciation, profuse sweating, cough, expectoration of pus mingled with blood, bad appetite, hectic, and in consequence cod-liver oil, cough mixtures, acid drops, wine, and good diet were administered, and all without effect. Indeed her appetite was so bad, that the diet was not taken, and nutrition suffered. When a careful examination of the chest enabled me to form a correct diagnosis, the treatment was changed. The cough and acid mixtures were

abolished, the stomach gradually regained its tone, her appearance slowly improved, and although, from necrosis of the ossified cartilages, the local disease was not removed, it was considerably benefited by topical applications.

**CASE IV.**—Miss G., æt. 26, had been treated by a homœopathic practitioner, for three years, who informed her that she was labouring under consumption, and at last that she had better go to Australia. Her friends, unwilling that this sentence of banishment should be carried out without further advice, brought her to me on the 19th of October last. I failed to discover the slightest alteration of the lungs, either by percussion or auscultation. On the contrary, repeated examination convinced me that the inspiratory and expiratory murmurs both possessed their natural softness and duration. There was, however, frequent cough with copious purulent expectoration. She had had constant sore throat since her childhood, and was labouring, in addition, under headaches, loss of appetite, constipation, leucorrhœa, excessive menstruation, hemorrhoids, occasioning frequent hemorrhage, so that she presented the anemic appearance, with all the symptoms of confirmed chlorosis. When I informed her that her lungs were not diseased, and that her cough entirely depended on some affection of the throat, she could not believe me. She had so long been convinced that her case was one of consumption, and that nothing but a change of climate could be of any advantage to her, that I think it was with some reluctance she heard a different opinion advanced. To oblige her relations, however, she allowed me to apply the solution of nitrate of silver first to the fauces and subsequently down the œsophagus. She then became convinced that there was a spot at the upper part of the throat which, when touched, gave rise to burning pain, induced severe spasms for a few moments, and subsequently left her free from cough, and enjoying remarkable ease. The applications were consequently continued every other day, and were conjoined with the internal administration of iron and vegetable bitters. Under this treatment she has much improved in health. I soon perceived, on passing the sponge, that there was a constriction at the upper part of the œsophagus, which I attribute to long-continued ulceration, followed by contraction, the termination of which causes me considerable anxiety.

Even when the lungs are decidedly tubercular, much of the cough and irritation may be owing to laryngeal complication, although, in the majority of cases, they are attributed to the pulmonary disease. I am satisfied that the constant cough and succussion of the chest so occasioned increases, if it does not actually sometimes induce, pulmonary disease, especially the most common of all phthisical complications—bronchitis. I was very much struck with the amount of cough in the following case, which was removed by paying attention to the laryngeal complication.

CASE V.—Dr C——n, a medical man, about 25 years of age, had long suffered from delicate health, and latterly the fatigue of his practice, which necessitated long journeys on horseback, frequently in the middle of the night, had induced constant coughing and thoracic pain. He had found such remedies as cod-liver oil, expectorants, demulcents, and anodynes useless. On examining his chest, there was slight dulness on percussion under one clavicle, somewhat harsh inspiration, and prolonged expiration in the same situation, with a little increase of vocal resonance. The disease in this case was evidently incipient, and yet I noticed the violent suffocative cough, followed by expectoration of purulent mucus, and was struck with the evident disparity between the incipient pulmonary lesion and the advanced cough and expectoration. This was explained by inspection of the fauces, which were red, rugous, and covered with patches of pus. Further, it was clear from the symptoms that the glottis was also affected. The local application every other day of a sponge saturated in a solution of nitrate of silver, was soon followed by the best results, and in a few weeks the cough entirely ceased, and with good diet he regained his general health, although the pulmonary signs remained unchanged.

The removal of the cough and expectoration in this case, although incipient phthisis was undoubtedly present, proves that the former were in no way caused by the latter, which continued to remain, notwithstanding the disappearance of his distressing symptoms. Expectorant and anodyne remedies in such cases are evidently useless and even injurious. Useless, because it cannot be supposed that squills, ipecacuanha, etc., by being introduced into the stomach, can act upon the follicular disease of the pharynx and larynx; and injurious, because these remedies, combined as they usually are with opium, occasion nausea, derange the appetite, destroy the capacity of taking food, and thus cause that diminution of vigour in the patient, so favourable to the development of the pulmonary tubercular exudation. In the following instance even a better result was obtained.

CASE VI.—Dr B., æt. 34, a medical practitioner in the island of Surinam, applied to me, during a visit he made to this country, in June 1850. He had frequent cough and sore throat, with copious expectoration, increased by exposure to cold. There were also the usual symptoms of incipient phthisis. On examining his chest physically, I discovered comparative dulness under the right clavicle, slight crepitation with the inspiration, prolonged expiration, with marked increase of the vocal resonance; the left lung was healthy; the mucous membrane of the fauces was of a dark red colour, scattered over with prominent follicles. I applied the sponge first to the fauces and afterwards introduced it into the larynx every other day, with evident benefit. He also took cod-liver oil, with an alkaline and vegetable bitter mixture. In the autumn he returned to

Surinam, and soon afterwards informed me by letter that his health was greatly improved. He again visited Edinburgh in August 1851. The throat had latterly again become troublesome, from exposure to the inclemency of the weather; but on examining the chest, although there was still slight dulness and increased vocal resonance under the right clavicle, all crepitation had disappeared. He spent the following winter at Sligo, and this summer commenced practice in a large village in Perthshire. Last month he again visited me, and asked if he could venture to insure his life. On percussing the chest no dulness could now be discovered, a mere shade of increased vocal resonance remained, and the breath sounds were perfectly natural. Under these circumstances I considered his phthisis to be arrested, and had no hesitation in sanctioning his application to an Edinburgh life insurance company, which at once admitted him, without any extra premium. Now I am of opinion that the arrestment of the phthisis in this case was mainly due to the good effects of the applications applied to the pharynx and larynx, and that the diminution of irritation there, and the removal of the cough, enabled the exuded tubercle to become absorbed with more readiness than it would otherwise have done.

I could cite a considerable number of cases in which laryngeal symptoms have been more or less mistaken for or complicated with phthisis, and which have been greatly benefited by a local treatment. At the same time, I need not say that there are a large number of cases in which no such complication exists, and that they must be judged of only by a careful auscultatory examination of the lungs and larynx, and by inspection of the pharynx. I have also had abundant opportunities of satisfying myself that many so-called cases of chronic bronchitis in persons of advanced life are entirely owing to throat disease,—a point, however, which has been so ably illustrated by Dr Horace Green, that I need not dwell upon it here.

The propriety of local applications in cases of tubercular ulceration of the glottis or larynx has in this country been much doubted, although highly recommended by Dr Horace Green. The following case, in which the larynx was greatly involved, has served to persuade me of its occasional benefit.

CASE VII.—Mr P., an advocate, spent the winter of 1851-2 at Torquay, and consulted me in March following. He was thirty-nine years of age, and told me, in a hoarse whisper, that for three or four winters previously he had suffered from cough, with discharge of matter from the nose. During the summer he was quite well. While resident in Devonshire he gradually lost his voice, and his medical attendant there had passed a sponge saturated with a solution of nitrate of silver every day,—a treatment, however, that had failed to arrest the aphonia, which, when I saw him, was complete. On examining his chest, I ascertained that there was impaired resonance under both clavicles, harsh and blowing murmur on inspiration,

which, with spitting, left little doubt that the pulmonary organs had been long affected, but were now in a quiescent state. His countenance was expressive of much suffering; there was considerable emaciation, great weakness, much sweating; and he complained of almost constant spasmodic cough, which shook the entire frame. There was pain and dryness of the larynx and throat, frequent expectoration of purulent mucus, often streaked with blood. Difficult deglutition, especially of fluids, which never failed to excite cough and prolonged spasms. On placing the stethoscope over the larynx, inspiration was accompanied with a hoarse sound; and on inspecting the fauces and pharynx, the mucous surface was seen to be rough, sprinkled over with red prominent follicles, and streaked with adherent purulent mucus.

As this gentleman assured me that the sponge had been daily passed into the larynx by his medical attendant at Torquay, I did not hesitate to introduce it at my first visit. There followed, however, the most violent general spasms, the greatest difficulty in inspiration, rendering suffocation imminent, and then prolonged cough shaking the body, accompanied with purulent expectoration tinged with blood. The violence of the spasm somewhat abated in from two to three minutes, but he was unable to address me for ten minutes more. He then said that he had never experienced similar sensations previously, and was satisfied that the sponge had never been introduced into the larynx at Torquay, as he had been informed that it had by his medical attendant there. On visiting him the next day, I learned that the local application had been productive of the best effects, that the cough and spasms had entirely ceased, deglutition had been performed with more ease, and that he had passed a better night than he had enjoyed for many months. His appetite, I understood, was anything but good, and he had for a long time laboured under dyspeptic symptoms. I recommended him to remain quiet, not to speak, and to take a nutritious solid diet. In the course of the night the cough and spasms returned, and next day I again passed the sponge, which once more excited spasms and suffocation, but not to so great an extent as on the former occasion. I continued to pass the sponge every other day, and its good effects were well marked. In a fortnight it excited little irritation, and was invariably succeeded by a sense of ease, diminution of cough, which generally continued to the following night. He was now also enabled to swallow his food with more comfort and more abundantly, and in consequence his general strength was slowly improving. During the months of April and May, the local application was continued every second or third day. Towards the end of that month he was enabled to take short walks, and instead of my going to him at Morningside, he came into Edinburgh and visited me. I had great difficulty, however, in preventing him from endeavouring to speak, and he was continually exciting the vocal cords. Indeed there could be little doubt that the voice, though not distinct, was much better, and occasionally, on making



an effort, he was pleased to hear himself utter articulate sounds. He now changed his residence, and it is presumed, from having slept in a damp bed-room, or from some other cause, a fresh attack of laryngitis was produced, attended with return of the cough, pain in the throat, and spasms, with fever and great restlessness at night. The pain was sometimes most severe on the right, at others on the left side, but was diminished by counter-irritants, and afterwards by the local application. In the middle of June I found it impossible to pass the sponge fairly into the larynx, and it was singular to observe that the patient became worse, felt more pain, and especially complained of pain extending back to the ear. It was apparent to me, however, that the ulcerated surface was cicatrising, although I felt some difficulty in understanding how the glottis was impenetrable. It then occurred to me that probably fungous granulations were obstructing the orifice. One day towards the end of June, he told me that on making a deep inspiration he felt something vibrating at the orifice of the larynx, and it then appeared to me probable that a small polypus had formed there. A few days afterwards, in attempting to pass the sponge, it was ascertained that this was really the case, as he immediately spat up a fleshy mass, the size of a pea, with a small neck at one side. The next day the sponge entered as usual, without any difficulty, and continued to do so till the middle of July, when it again met with an obstruction. His general health, however, had greatly improved; the appetite was tolerably good; the pulmonary signs throughout had remained stationary. In the early part of August he went to the country on a visit, and his health became much improved. During September he visited a hydropathic establishment, and submitted to a course of treatment, consisting of a wet sheet every morning, a sitz bath twice a day, a wet belt round his abdomen worn from morning until dinner time, and a saturated towel round his throat every evening, with walking three times a-day in spite of all weathers. This heroic treatment caused the sweating and weakness, which had previously disappeared, again to return. He felt shivering on one occasion, after the sitz bath, and acute pain in his chest, violent cough and epistaxis, which fortunately subsided next day. On my seeing him early in October, he was pale, thinner than I left him, the voice, throat, and larynx were in the same condition; but he expressed himself as having been relieved of his occasional headache and dyspepsia. Towards the latter end of October, however, he complained of severe pain, deep in the nostrils, extending in the direction of the frontal sinus, and backwards to the ear on the left side. This continued to increase, and the discharge from the nose became more abundant, and formed inspissated moulds in the nares during the night, which were with difficulty discharged on the following morning. During the present month (November), two pieces of laminated bone have been discharged at different times, one from the left nostril, the other by the mouth, it having fallen backwards into the throat. At this juncture I thought



it possible that surgical interference might facilitate the removal of a sequestrum, should one be present in the nasal passages, and on stating this to the patient, he requested me to consult with Mr Syme on that point. The opinion of that gentleman, after seeing the patient, was that the vomer was the bone diseased, and that no surgical interference was warrantable.

Such is the present condition of this case, which I regard as one of arrested pulmonary *and* laryngeal phthisis, complicated latterly with ulceration and necrosis of the nasal passages. The latter is the only active disease under which he now labours, and is the source of the slight pharyngeal cough which still lingers. The local treatment of the larynx has, to use his own expression, "made him a new man,"—and it is in this respect that the case is instructive. No doubt the severity of the hydropathic treatment exposed him to unnecessary risk; for had a fresh inflammation seized either upon his larynx or lungs, it would have been most injurious, if not fatal, and it must be obvious he very narrowly escaped this. On the other hand, his general health was no way improved by it, if we except the better appetite, dependent probably on the increased amount of exercise he was induced to take.

The cases now given, with others that might have been adduced, have satisfied me that lesions of the pharynx and larynx ought to occupy the serious attention of the practitioner in all cases of pulmonary diseases, and that the following practical conclusions may be drawn from them:—

1st, That not unfrequently diseases, entirely seated in the larynx or pharynx, are mistaken for phthisis pulmonalis.

2d, That even when pulmonary phthisis exists, many of the urgent symptoms are not so much owing to disease in the lung as to the pharyngeal and laryngeal complications.

3d, That a local treatment may not only remove or alleviate these complications, but that, in conjunction with general remedies, it tends in a marked manner to induce arrestment of the pulmonary disease.

## ARTICLE II.—*Contributions to Obstetric Pathology and Practice.*

By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—(*Continued from p. 375.*)

### NO. IX.—ON INFLAMMATORY AND NON-INFLAMMATORY RUPTURES OF OVARIAN DROPSICAL CYSTS.

THE common multilocular dropsy of the ovary may terminate fatally in various ways when left altogether uninterfered with by art.

In some cases, when the tumour at last reaches those enormous dimensions which it sometimes acquires, the mere strong compres-

sion laterally of the diseased mass upon the various abdominal viscera, vessels, and walls, and upwards upon the diaphragm and thoracic organs, proves sufficient in itself to lead gradually on to a fatal termination, preceded by marasmus, exhaustion, dyspnoea, etc. In such instances, there is a slow but increasing physical clog set to the machinery of various organs that are necessary to the continuance of life,—more particularly to the processes of nutrition and assimilation; but latterly, even respiration and circulation come to be more and more interfered with; till at last the impaired and obstructed mechanism of the body is fatally arrested. Very often, however, before such a termination occurs, œdema, particularly of the lower extremities, and ascites, come to be superadded, hastening the fatal result by their presence; and almost always it is also hurried onward by the supervention, during the last stages of the disease, of a greater or less amount of irritative fever.

But patients suffering under dropsy of the ovary do not always die from the mere mechanical increase of the tumour, and its mere mechanical pressure and irritation. Much more frequently *inflammatory action* attacks the walls or dissepiments of the diseased mass during the latter stages of its growth, and expedites the progress of the malady towards a fatal termination. Occasionally the inflammatory action recurs from time to time in the same cyst or in different cysts, accompanied with fever and local pains, generally of a slight and obscure kind. Under repetitions of such attacks, the cysts rapidly increase in size, inflammatory effusions being poured into their cavities or deposited upon their lining membranes; and sometimes the intervening septa and walls of the tumour become diseased and broken down under them. Its external or peritoneal surface is frequently also the seat of inflammatory effusions, and of consequent local adhesions between it and the neighbouring viscera and surfaces. One or more cysts, left with purulent effusions in their cavities, or with the structure of their walls disintegrated, often enough remain as permanent sources of local and constantly recurrent inflammatory action in the tumour. Hectic fever generally comes to be set up in the system as a consequence; and under the repeated recurrence of such local and constitutional irritation, the powers of life gradually give way.

Again, occasionally, when inflammatory action, whether acute or sub-acute in its course, is present in an ovarian multilocular tumour, it proves fearfully more rapid in its course, and leads to speedy death by a different mode. For when the tumour, in one or more of its cysts, is the seat of acute inflammation, such cyst or cysts sometimes become so over-distended by inflammatory effusions, as ultimately to rupture, and allow of the escape of their contents into the peritoneum. In these cases, the walls of the over-dilated cysts are occasionally rendered friable and lacerable in their structure by the inflammatory action of which they are the seat, and in consequence of this morbid softening of tissues, the rupture in question the more

readily occurs. In one or two cases, I have seen the walls of the inflamed and ruptured cyst present ulcerations upon its interior, the perforation of the cyst in such instances being begun by ulceration of the lining membrane and tissues of the cyst, and perfected by mechanical laceration of its exterior or peritoneal coat.

During the last two months I have met with two cases of rapid death from the rupture of inflamed multilocular tumours of the ovary and the consequent effusion of their morbid contents into the cavity of the abdomen. The first of these cases occurred in a patient under my care in the ward set aside for female diseases in the Royal Infirmary. She had been previously under the care of Dr Brown of Carronshore.

CASE I.—Mary W——, æt. 34, married, was admitted October 6, 1852. She had borne three children, the youngest of them now four years old. In May last she first observed a small rounded tumour in the lower part of the abdomen. The tumour was then of firm consistence, painful on pressure, and slightly moveable. At the date of admission, the abdominal tumour was nearly as large as the uterus at the full term of utero-gestation, and unequal and bosselated on its surface. It was tense and painful under pressure, and a distinct fluctuation could be traced in it. The os uteri was situated very high in the pelvis (*elevatio uteri*), and the vagina was much narrowed and stretched at its upper extremity.

During the week subsequent to her admission, the patient was merely confined to bed for the sake of rest, and placed under some simple antiphlogistic treatment. On the morning of October 13th, (seven days after she entered the hospital), she was suddenly attacked with severe pain in the right hypochondriac region, preceded by a feeling as if something had burst in the abdomen. This pain increased in intensity during the forenoon, with great and general tenderness of the abdomen on pressure. In the after part of the day, nausea and vomiting supervened, and the pulse rose in frequency and sunk in strength.

On the following day, at the time of visit, she expressed herself, however, much easier; the abdomen was tense, and increased in size, but scarcely now painful on pressure. The face, however, was very pinched and anxious, the surface cold, and the pulse so small, that it could not be counted, and she was altogether so collapsed that there seemed little hope of her surviving the attack. That night, however, and during the subsequent day, she rallied considerably. The pulse became stronger and reduced in frequency, and she improved gradually for the next fifty or sixty hours. Five days, however, after this first rupture and attack of peritonitis, another and more fatal one supervened. For, early in the morning of the 18th, when attempting to raise herself in bed, she was suddenly seized with great renewed pain in the epigastrium, followed by vomiting of green-

ish-coloured fluid, extreme coldness of the surface, and other symptoms of sinking and collapse. The pulse became imperceptible; and she died within twelve hours.

On a post-mortem examination, the abdomen was found to contain two or three quarts of turbid serum, thick layers of unorganised and recently secreted coagulable lymph covered the peritoneal surface of the ovarian tumour and the more exposed parts of the abdominal viscera. At some parts, this layer of lymph was fully an inch in thickness, but quite soft and easily broken down. The left ovary was small and slightly indurated. The enormous ovarian tumour which was present, was attached to the site of the right ovary by a pedicle measuring about two fingers, in breadth and thickness.

On removing the tumour out of the abdomen, a quantity of dirty foetid purulent fluid made its escape from an opening at the upper and back part of its right side. The aperture was of such size as to admit the forefinger. Its edges were ragged and ulcerated; and it exactly corresponded in position to the seat of pain, when the symptoms of sinking first appeared. On cutting into the cyst with which the opening communicated, it was found to contain upwards of a pint of fluid such as has been described. The wall of the cyst, for about an inch round the point of rupture, was much thinned and softened, and presented a black colour.

The remaining portion of the lining membrane of the cyst was covered with patches of recent lymph, and red spots, having the granulated appearance of the intestinal canal in acute dysentery. At the lower and anterior part of the left side of the ovarian tumour, there existed another largish cyst, presenting similar characters when cut into. Several of the smaller cysts of the tumour showed strong signs of recent acute inflammatory action; and pus oozed out in various parts when the large multilocular mass was bisected. The intervening septa were at some parts found to vary from half an inch to an inch in thickness, and presented on division a fibro-cystic structure. No other opening could be found in the external or peritoneal coat of the tumour, except the one already noticed.

The following analogous case of fatal inflammatory laceration of an ovarian dropsical tumour, has occurred still more lately in my private practice. In this, as in the preceding instance, the tumour was unusually rapid in its general growth.

CASE II.—The patient, aged 43, was married in 1836, and had been for two years a widow. She was the mother of three children. Up till the middle of last year she enjoyed uninterrupted good health. At that time she began to complain of pain in the right side; and at the commencement of the present year, the presence of an abdominal swelling was detected by her medical attendants at Boulogne, where she was residing. In May last she came to Edin-

burgh, and a large ovarian tumour was then easily diagnosed. It continued to increase and soften with extreme rapidity; and in despite of antiphlogistic measures, iodine, etc., the abdominal swelling and distension were so very great by the first week of September, that tapping became necessary. Twelve imperial pints of a clear glairy fluid were withdrawn, but the bulk of the tumour seemed not much reduced in size by this evacuation; and it was evident that an enormous mass of cysts remained untouched, while a single and comparatively small one only had been opened. She speedily recovered from this paracentesis. By the 16th October the re-accumulation of fluid was already so great, that tapping was again had recourse to. The fluid now evacuated was of a dark colour, and was evidently commixed with pus and inflammatory secretions from the lining membrane of the perforated cyst. During the few following weeks she complained occasionally of more or less pain and tenderness in the tumour, but was able to take some out-door exercise. On November the 17th, she walked to my house, a distance of about half-a-mile, to ask if I would allow her again to begin the use of some iodine which she had formerly taken. She became sick and vomited on her return home, and subsequently complained of some abdominal pain. Next day vomiting again recurred, and the pain became far more severe and diffused over the whole abdomen. By evening ~~the pulse~~ <sup>the pulse</sup> was very rapid, and almost imperceptible at the wrist, the extremities cold, and the vomiting almost incessant. The symptoms of sinking increased during the night, and she died on the following day about two o'clock, retaining her consciousness to the last, and expressing herself, for some hours previously, as free from all pain.

On making a *post mortem* examination, a very large multilocular tumour of the right ovary was found adhering, in different parts, to the abdominal parietes. A quantity of yellow glairy fluid, mixed with coagulable lymph, was effused into the cavity of the peritoneum. Upon the right side of the tumour, immediately below the liver, two small apertures, in two different but neighbouring cysts, were observed; and from these, fluid similar to that in the abdomen welled out upon pressure. At that part of the tumour the cysts were exceedingly numerous, and their walls, at various points, so transparent and attenuated, that under slight pressure they burst, and had their contents evacuated.

Most of the larger cysts in the tumour, and several of the smaller, showed signs of high preceding inflammatory action in their parietes. Many of them contained pus in their cavities, and their injected lining membrane was coated freely, in various parts, with particles and layers of yellowish coagulable lymph. There were perforations in the walls of most of the larger cysts, allowing of free communication between their cavities and the cysts adjoining them.

In few or none of our accounts of the pathology of ovarian

tumours, are those morbid appearances described which are produced by inflammation in ovarian multilocular cysts. But when the lining membrane of a compound ovarian cyst is the seat of inflammatory action, it generally presents, as in the preceding instances, morbid appearances similar to those that we see upon inflamed normal serous surfaces, such as the pleura and pericardium. When the effusion from the inflamed membrane is limited to serum merely, this alone is scarcely traceable; because it becomes at once commixed with, and lost among, the normal serous or gelatinous contents of the cyst. But pus is very often a result of inflammation of an ovarian cyst, more particularly if the inflammation has been sub-acute rather than acute in its type. The purulent matter is usually not seen on tapping or dissection, till the very lowest part of the cyst is emptied, for it is generally observed to have gravitated downwards to the more dependent parts of it. Flakes and masses of loose coagulable lymph are also often present in the contents of the inflamed cyst; or the lymph is attached to the lining surface of it, in the form of granulated spots, or larger patches and layers, or as an organised false membrane covering the interior of the cyst. When the coagulable lymph effused on the interior of an ovarian cyst becomes organised,<sup>1</sup> and the walls of the cyst *again* happen to be the seat of renewed inflammation, blood is frequently poured out to a greater or less extent into the cavity of the cyst, as in hemorrhagic pleurisy; and this termination is perhaps more frequently the result of inflammation of the ovarian cyst, than the result of inflammation of any natural serous membrane. Occasionally the organised lymph deposited by inflammation upon the interior of an ovarian cyst, becomes highly injected under sub-acute or chronic inflammation; and I have seen it give an appearance of roughness to the interior of the cyst, as if it had been lined by mucous membrane, beset with numerous elongated injected villi. In a few instances the interior of an inflamed ovarian cyst becomes ulcerated, the ulcerated spots being sometimes round, in other cases irregular. The perforation of the walls of continuous cysts seem sometimes to be the result of ulceration; but more frequently, perhaps, they are ruptures produced by mechanical or inflammatory distension. And now and again portions of the walls of a cyst, or of several cysts, are found dead and gangrenous, either as the result of destructive inflammatory action in their tissues, or from the circulation through them being mechanically arrested by the compression and obliteration of the vessels which supplied them with blood.

In both the cases which I have above described, the effusion of the contents of the lacerated ovarian cyst into the cavity of the peritoneum, was followed by rapid and fatal peritonitis. But many

<sup>1</sup> In the Anatomical Museum of the University there are some specimens of ovarian cysts beautifully injected by Professor Goodsir, and the lymph on their interior is seen to be highly vascular.



cases are on record, and I have myself seen several, in which the rupture of ovarian cysts into the cavity of the abdomen, led to no such pathological result. And it becomes not only an interesting, but an important practical question to consider,—“*Under what circumstances is the rupture of an ovarian cyst followed by inflammatory action in the peritoneum; and under what circumstances does this dangerous consequence not supervene?*”

I believe the proper answer to this question consists in a reference to the condition and contents of the cyst at the time of its laceration. If the walls of the cyst, previous to laceration, have been the seat of inflammatory action, and its contents consist of inflammatory secretions, or perhaps of some other forms of morbid irritating matter, the escape of such morbid fluids into the cavity of the peritoneum, will be found, I imagine, to give rise invariably to inflammatory action in the peritoneum itself; and hence, to be always accompanied with danger to the life of the patient. But if, on the other hand, the cyst does not lacerate under inflammatory distension, but has either given way in consequence merely of the gradual thinning and attenuation of its own over-distended walls—or has ruptured under external mechanical injury, as falls, etc.—(and the fluid which escapes through laceration into the cavity of the peritoneum, is of the mild unirritating character which *naturally* belongs to ovarian cysts in an uninflamed condition), then inflammation of the peritoneum has little or no tendency to supervene. The bland fluid which, under these last conditions, becomes discharged into the cavity of the peritoneum, is not a morbid irritant to that serous membrane, such as a fluid commixed with inflammatory secretions is. Nay, the lacerations of multilocular *uninflamed* ovarian cysts, instead of leading to imminent danger and probably speedy death, have frequently, though accidentally, led to the actual preservation, or at least to the prolongation, of the life of the patient. The explanation of this result perhaps merits one or two remarks.

The interior of an ovarian cyst has no power whatever of absorption; and consequently no diuretics, or de-obstruents of any kind, have any therapeutic influence on the reduction of an ovarian tumour by removal of its fluid contents by the tissues of the tumour itself. But if the bland uninflamed contents of an ovarian cyst become evacuated by accidental rupture into the cavity of the peritoneum, they may be, and often are, readily absorbed from that position; the peritoneum being normally provided with abundant absorbing powers, and these powers being generally capable of being excited, when required, by the action of diuretics, etc. Consequently, when an escape of innocuous unirritating fluid takes place from the sac of an ovarian cyst into the sac of the peritoneum, it may be, and often is, rapidly absorbed and removed from the peritoneal cavity. Cases occasionally occur where nature in this way from time to time spontaneously taps, if we may so speak, an ovarian dropsy into the

cavity of the peritoneum; thus ever and anon relieving the patient of the recurrent accumulations of fluid. But another and still happier result has sometimes followed the mechanical laceration of an ovarian cyst into the peritoneal cavity. In fact, in repeated instances it has been observed that in this way, a cure, which may be termed a permanent, though a palliative one, has taken place. For, when the laceration in the walls of the ovarian cyst has been originally large,—or though originally small, has remained *permanently* open,—so as to allow of the continuous escape of the fluid secreted by the ovarian sac into the cavity of the peritoneum itself, the peritoneum under these circumstances, has sometimes acted as a permanent absorbing surface, removing constantly the fluid eliminated by the lining membrane of the ovarian cyst as a permanent secreting surface. In these fortunate, but rare cases, another result appears sometimes to follow, namely, the ovarian tumour, if it has happened to contain one large and preponderating cyst, becomes collapsed, the fluids which have originally escaped from its own cavity surrounding and compressing its walls externally; and the interior of the cyst, thus kept with its walls in apposition, at last secretes little or no fluid; and possibly perhaps its sides may ultimately adhere together in some very rare cases.

I am acquainted with the history of two cases in which the first tapping of an ovarian dropsy has never been followed by any re-accumulation, the operation in both having now been performed several years ago. And I believe that the secret of this very unusual termination in the two cases in question, is ascribable to the circumstance, that the perforation formed in the walls of the ovarian cyst by the trocar, has remained permanently open like a fistula, allowing of the continuous drain of the ovarian fluid into the cavity of the peritoneum.

Perhaps art will yet be able to imitate both successfully and with certainty in an appropriate set of ovarian cases, this fortunate accidental termination.

Occasionally, after a patient has been often submitted to the operation of paracentesis, an accidental rupture of the ovarian cyst has produced a comparatively permanent cure, as in the following instance :—

CASE III.—A patient, now aged 56, the mother of five children, and naturally of a very robust and strong constitution, had up to the end of last year been tapped for ovarian dropsy 44 times by myself and others. Latterly, the paracentesis was required every few weeks, and an enormous amount of fluid was always evacuated. I have repeatedly seen above four gallons of fluid drawn off at a single tapping. Last winter, this patient slipped in walking upon a frozen path, and so violently struck the abdomen and ovarian tumour against the ground in her fall as to rupture the cyst. Since that time, however, no new tapping has been required. The abdominal

swelling, though still large, is considerably less than it was at the time of the fall, and does not increase in size. For a time the fluid of the cyst evidently escaped freely into the cavity of the peritoneum, and was as regularly absorbed from it. Latterly there has been apparently much less, or indeed no, perceptible amount of fluid in the cavity of the peritoneum. For several months the patient's skin was in an almost constant state of diaphoresis—a result which, to her, appeared the more strange, as for years previously she had never been able to excite any perceptible degree of perspiration. This tendency to spontaneous diaphoresis has latterly decreased. The urinary secretion was often previously affected and greatly diminished as the ovarian tumour enlarged. Since the fall and rupture of the cyst, the kidneys have continued to act very freely and uninterruptedly, the urine secreted being now always clear and limpid.

In the course of the preceding remarks, I have referred to the rupture of ovarian cysts into the cavity of the peritoneum only. But they rupture occasionally also into the intestinal genital or urinary canals, or upon the external surface of the abdomen. When ovarian cysts rupture, not into the peritoneum, but into these mucous canals, or on the external cutaneous surface, it is a matter of little moment, in relation to the life and safety of the mother, whether the cysts before bursting have been inflamed or not inflamed; and whether their contents be of an acrid and irritating, or of a bland and un-irritating character. For there is no danger to the mother from the mere nature of the contents of the cysts when these contents have once escaped into a free mucous canal, or upon the free cutaneous surface of the body. And whatever may be the character of the escaped or escaping fluid, we may equally hope for a temporary or more permanent amelioration of the disease. If the opening is slight and valvular, the fluid accumulating in the cyst may only escape intermittingly or imperfectly. If the opening is larger and more permanent, the contents of the cyst are sometimes kept constantly draining off from the morbid cavity of the ovarian tumour; and this cavity has in consequence in some cases diminished and collapsed to a degree amounting to a kind of perfect cure.

I have seen two or three instances in which ovarian cysts have ruptured externally, or into the adjoining mucons canals. The case which I have had an opportunity of watching the longest is the following:—

CASE IV.—This patient felt a moveable tumour in the abdomen of the size of the fist, about the age of sixteen or seventeen. I first saw her about eight years afterwards, when the abdomen was greatly distended with a dropsical ovary, larger than the uterus at the full period of pregnancy. She was complaining greatly of the

symptoms of over-distension. I removed the fluid by tapping in 1840. Fifteen months afterwards the same operation was repeated, in consequence of the re-accumulation of the fluid. But no paracentesis has been required since that time; and at the present date (November 1852) she enjoys good health. A few months after the second tapping, the patient had a tedious attack of typhus fever, accompanied and followed by peritoneal inflammation. She was confined for many long weeks to bed. Sometime after her recovery, and when the tumour was again increased to a great size, there suddenly supervened, one night on going to bed, much soreness in the tumour; and this was followed ere morning by an abundant and large escape of thickish clouded fluid from the genital canals. The tumour subsided much in size under this discharge, which only, however, lasted for a few days. Again, after this discharge ceased, the tumour increased to an exceeding size; and, on the patient one day twisting herself round on the sofa, she felt, as she herself described it, "something tear" in the right side. In the course of that day a clear limpid fluid again began to pour profusely from the vagina, and the tumour immediately softened and decreased in size. This discharge has since that time continued, and has now gone on for several years. The discharge is always greatest when the patient is lying or walking, but its total daily amount is at present not great. The ovarian tumour is, she herself believes, now much less in size than after its last rupture, though it is still larger in volume than the adult head. It moves readily under pressure. The patient's menstrual life is regular and normal in all respects; she now enjoys, as I have already stated, the most excellent health; daily performs active house duties; and has latterly become so stout as to weigh upwards of fourteen stones.

The seat of the opening between the ovarian cyst and the genital tubes in the preceding case, it is of course impossible to ascertain. But judging from the dissections which have been made in analogous cases of ovarian cysts emptying themselves by the genital canals, the seat of communication is, in all likelihood, between the ovarian cyst and the Fallopian tube. Lately Richard has shown, that ovarian cysts do not so unfrequently as was formerly supposed communicate with the Fallopian tube; and that the cavity of the tube in consequence often becomes distended with the fluid of the adjoining cyst. But though thus distended, the tube is not in many cases sufficiently open at its uterine extremity to allow of the escape through the uterus of the contained fluid. Several cases, however, have now been observed by Morgagni, Boivin, Robertson, and others, in which, after death, ovarian cysts have been found to have such a free communication along the canal of the Fallopian tube, and this tube again with the cavity of the uterus, as that the contents of the cyst escaped freely outward along the course of the genital canals.

The desultory observations offered in the preceding remarks may, perhaps, be all briefly recapitulated in the form of the following conclusions :—

1. The cysts forming an ovarian dropsy, occasionally rupture, *first*, from inflammatory effusion into and distension of their cavities ; or, *secondly* (the contents of the cysts being only the common bland secretion of such cysts, and unmixed with any inflammatory matter), they may rupture from mere over-dilatation and gradual attenuation of their coats, or under sudden mechanical pressure and injury.

2. When a cyst ruptures from the effects of inflammation, or contains within it at the time of rupture inflammatory secretions and materials, the escaping fluid, if effused into the cavity of the peritoneum, is always liable to be followed by dangerous, and generally fatal, peritonitis.

3. If, however, a cyst bursts into the peritoneum under mechanical injury, or in consequence of simple laceration from over-distension of its cavity, and the fluid effused into the sac of the peritoneum is consequently not commixed with inflammatory secretion, there is little or no great tendency to peritonitis.

4. Sometimes, indeed, when thus a non-inflamed ovarian cyst ruptures into the cavity of the peritoneum, the life of the patient is preserved, or at least prolonged, by this accident.

5. When an ovarian cyst ruptures into a mucous canal, or upon the cutaneous surface, the safety or danger attendant on the laceration is not regulated by the inflamed or non-inflamed character of the effused fluid.

6. In cases in which the fluid of an ovarian cyst obtains an outlet by a mucous canal, or by the skin, a temporary or more permanent reduction of the tumour and comparative cure of it may be the consequence.

Lastly, let me add that, as in many cases and points the surgery of art is an imitation of the surgery of nature, possibly the artificial repetition and establishment of the above modes of relief, if they could be imitated safely and certainly, may yet be found capable of temporarily arresting, if not curing, ovarian dropsies in some appropriate cases, and more particularly in instances in which the bulk of the tumour is formed by one large preponderating cyst.

## NO. X.—ON VESICO-UTERINE, VESICO-INTESTINAL, AND UTERO-INTESTINAL FISTULÆ, AS RESULTS OF PELVIC ABSCESS.

Cellular tissue exists abundantly within the pelvis, along the lining of the walls of the pelvic cavity, within the layers of the broad ligament, and between the intestinal, genital, and urinary canals, at all those points in which they come into organic coherence. Inflammation of this tissue, or Pelvic Cellulitis, is a common affection, particularly as a consequence of parturition, etc. Pelvic cellulitis, after giving rise to great swelling and induration, by the effusion of serum, coagulable lymph, etc., into the inflamed portion of the cellular tissue, very often terminates sooner or later in resolution; the disease not unfrequently assuming a subacute or chronic type. In other instances, however, the inflammatory action runs on toward suppuration, and forms a so-called Pelvic Abscess. When this termination occurs, the collected purulent matter is found to obtain egress by different outlets. The abscess sometimes bursts into the intestinal canal, or into the genital canal, or into the urinary bladder. Occasionally it discharges externally upon the cutaneous surface; and in a few rare instances it opens into the cavity of the peritoneum. Sometimes the collected pus is found to make its escape simultaneously, or rather consecutively, by *two* different exits. Thus, we may have the cavity of the same abscess opening into two different pelvic mucous canals. Where such *double* perforations, originating in the escape of matter from a pelvic abscess, become chronic in their character, they lead to the formation of several species of deep pelvic fistulæ, which have not, as far as I am aware, been hitherto described by obstetric pathologists.

Very few instances of the existence of fistulæ of any kind between the bladder and uterus have hitherto been put upon record. Indeed, the number of *vesico-uterine fistulæ* hitherto recorded, seems to be limited to three instances, reported severally by Mad. Lachapelle, Professor Stoltz, and M. Jobert; and in all of these three cases, the perforation which existed between the bladder and cavity of the neck of the uterus, was the result of injury during parturition.

In the following case this rare form of lesion was produced as a consequence of pelvic cellulitis; or, to speak more definitely, it was produced by a purulent collection formed in the cellular tissue lying between the bladder and the neck of the uterus, and ultimately rupturing—on one side into the bladder, and on the other side into the cavity of the uterus, or rather the cavity of the cervix uteri.

CASE I.—The patient, aged 22, and the mother of two children, was admitted into the female ward of the Royal Infirmary in June last. Her youngest child was then eleven months old; and she had made a perfect recovery after her confinement with it. About six months, however, subsequently to her delivery, she was seized with local pelvic



pain, dysuria, and the usual symptoms of pelvic cellular inflammation. Three or four weeks after the commencement of this attack she had shiverings and perspirations, and other symptoms of hectic fever. These symptoms were shortly followed by evidence of the escape of purulent matter; and subsequently complete incontinence of urine came on. After this the urine continued to be discharged per vaginam up to the date of her admission into the hospital, four months after the commencement of the inflammatory symptoms. The urine contained a considerable quantity of pus. On examination, the urethra was found perfectly patent, although the urinary secretion was not discharged through it. There was still a considerable mass of fixed inflammatory deposit, in front of the cervix uteri, or in the cellular tissue between it and the posterior wall of the bladder. The cervix uteri itself was considerably hypertrophied, particularly its anterior lip. That the urine passed from the bladder through the os and cervix uteri, was ascertained by the simple experiment of filling up the os uteri for a day, with a small sponge tent. During the time the cavity of the os uteri was stopped up with this plug, the urinary discharge was evacuated through the urethra; but immediately again began, and continued to pass through the artificial vesico-uterine opening as soon as the sponge plug was withdrawn. After withdrawing the plug, the cervical cavity, which had been dilated by its presence, was examined by the finger, and two apertures were found passing into it, or rather leading from it,—one, the normal aperture leading upwards into the cavity of the uterus, as ascertained by the uterine sound,—the other, tending obliquely forwards towards the cavity of the bladder. This latter artificial opening was freely cauterised by solid nitrate of silver. Subsequently, local and general measures were employed (as external counter-irritation, iodine, etc.), to promote the absorption of the inflammatory deposit. In the course of a few weeks the swelling from the deposit between the bladder and cervix uteri diminished, the incontinence of urine became gradually lessened, and was ultimately totally arrested; the cure being, as I believe, the result of the natural contraction of parts following upon the absorption of the original inflammatory deposit. Subsequently this poor patient was attacked with symptoms of acute pulmonary phthisis, and died a short time ago at a distance in the country, but without any return whatever of the incontinence of urine.

I have seen other cases of pelvic cellulitis which had run on to suppuration, leave other forms of fistulous perforation, perhaps still more strange and singular in their anatomical relations.

Two years ago, I had under my care, a case in which there was produced as the result of this disease, a *utero-intestinal fistula*.

CASE II.—A lady, a few days after her first confinement, was at-

tacked with symptoms of fever, and local pelvic inflammation. These terminated in a very tedious pelvic abscess. About a year subsequent to her accouchement, she was brought to Edinburgh, and placed for some time under my care. She still had considerable thickening and pain on pressure in the left side of the pelvis, which had been the seat of the pelvic cellulitis. The cervix uteri, and indeed the whole uterus, was elevated upwards, and drawn much laterally to the same side. On examining simultaneously, with the fingers of the right hand on the roof of the vagina, and with those of the left hand placed externally over the left iliac region, much thickening and agglutination of the uterus and intestines could be readily ascertained in the left pelvic and iliac regions. Discharges of slight accumulations of pus recurred from time to time through the os uteri; and, occasionally, after these discharges, small quantities of feculent matter were found in the vagina,—showing a communication to exist between the intestinal canal, at some part (perhaps the sigmoid flexure), and the cavity of the uterus. When the canal of the cervix uteri was gently examined by a slender probe or sound, a fistulous communication could be traced, passing up from the cavity of the cervix laterally towards the left iliac region; but this sinus could not be followed for any great length.

In this case there was a constant tendency to the recurrence of inflammatory attacks in the original seat of the pelvic inflammation, some of which were extremely severe. The patient subsequently removed to the south of England, where she died under, I believe, one of these renewed inflammatory attacks. My friend, Professor Lawrie of Glasgow, saw this case repeatedly.

Some time since, I was consulted by a patient, in whom there existed a still more rare and curious form of fistula, the result of a pelvic abscess under which she had suffered. Professor Macfarlane of Glasgow, and Dr Miller of London, also saw the case. Its peculiarity consisted in this,—that, as a result of pelvic abscess, a fistula was formed between the bladder and rectum—a *recto-vesical fistula*—and yet the intermediate uterine and vaginal canals were not implicated in it. The principal particulars regarding the case are as follow:—

CASE III.—The patient, when about 23 years of age, and unmarried, was attacked with fever and severe local inflammatory pain in the pelvis and left groin. After these symptoms had continued for some weeks, she was at length relieved by the discharge of a large quantity of purulent matter from the rectum. About twelve months after the occurrence of this pelvic abscess, she was considered so well as to be allowed to be married. But from that time she suffered from repeated attacks of pelvic irritation and inflammation, with leucorrhœa, irregular menstruation, etc. She never became

pregnant. Several years subsequent to marriage, during one of these recurrent pelvic attacks, the bladder became greatly irritated; and, after this painful dysuria had lasted for some time, purulent matter was discharged along with the urine. Subsequently to this period, and on to the time of her death (four years afterwards), small portions of feculent matter and flatus passed from time to time by the urethra, along with the urine,—showing a communication to exist between the intestinal and urinary canals. As high up the rectum as could be reached with the finger, a fistulous opening was traceable in the anterior and lateral part of the bowel, and a probe could be passed forwards to some extent through it. There was much thickening and agglutination of the pelvic tissues at that part. No treatment was of any avail in relieving the patient from her distressing symptoms. She died ultimately of a short illness, from (as reported to me by Dr Miller) some affection of the brain.

In the preceding and in other cases of pelvic abscess, the different openings through which the matter becomes discharged do not, as I have already remarked, always occur simultaneously, but generally consecutively. After the purulent matter has escaped, apparently with sufficient freedom, by one opening, it will occasionally, in consequence either of its temporary obstruction and retention in the sac of the abscess, or in consequence of the walls of the abscess themselves ulcerating, again open at a subsequent period into another canal. The following case, which I had occasion to see often under the kind and able care of Dr Johnson of Stirling, will serve to illustrate this remark:—

CASE IV.—A patient in the country was seized with acute symptoms of pelvic cellulitis; and a large inflammatory tumour speedily formed in the cellular tissue of the left broad ligament, and in the left iliac fossa. In despite of the active antiphlogistic treatment that was adopted, the disease ran on towards suppuration, and dangerous symptoms of irritative fever and exhaustion supervened, with great local tenderness in the affected part. An exploring needle, introduced by the spine of the ilium into the inflammatory swelling, lying in the iliac fossa, showed the presence of a deep collection of pus. I evacuated it twice by a trocar, with great relief to the patient. Pus continued to be freely discharged for a considerable time through this artificial opening on the edge of the left iliac fossa. After this discharge had gone on for some weeks, the opening from time to time offering to become nearly closed, a new and distressing symptom supervened. For, along with a discharge of matter through the artificial opening, there passed from time to time some flatus, and occasionally a slight appearance of feculent matter,—showing that the abscess had ulcerated into the bowel, probably the sigmoid flexure of the colon.

In this instance, the spontaneous opening of the abscess into the intestinal canal did not, as I have said, occur till weeks subsequent to the discharge of the matter through the external opening. After this occurrence, the external fistula was attempted to be shut up by various means, but for some time without avail. At last, by altering the position of the external orifice, by a new incision, the whole external aperture was happily obliterated, and the patient has made a perfect recovery.

In the preceding cases, the fistulous communications resulting from the pelvic abscess, were probably valvular in their form, inasmuch as, probably the fluids and air escaped through the track of the fistula only occasionally, and not constantly. Perhaps, in some, the transit of these matters was only effected, when the canal of the fistula was preternaturally distended by an accumulation within and behind it of the morbid matters that passed; the walls of the canal at other times being so much in contact as not to allow of the passage of any foreign body.

In the case of vesico-intestinal fistula which I have mentioned—(SEE CASE III.) feculent matter and flatus, appeared to pass from the bowel into the bladder; but the patient was never aware of the urine passing from the bladder backward into the bowel. It is quite possible, however, that it may occasionally have done so in small quantity, without her being able to recognise it distinctly. If it were not so, then, in that case, the fistulous communication was of such a valvular structure, as to allow of the transit of matters in one direction, and not in another,—a point in its anatomy, which it is not difficult to conceive.

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ARTICLE III.—*Case of Severe and Dangerous Salivation, from Ectrotic Treatment by Mercurial Ointment in Small-Pox.* By GEORGE PATERSON, M.D., F.R.C.P. Edin., Tiverton.

On the 15th October, I was requested to see a young lady in consultation with Mr Mackenzie of this town. An eruption, which proved to be that of small-pox, had been out upon her for two days, and the symptoms of initiatory fever had been severe, and followed by a degree of exhaustion and failure of the vital powers, which, even in this early period of the case, necessitated a free administration of wine and other stimulants. The spots of eruption were numerous, and showed a tendency to coalesce upon the face, so that although she had been vaccinated in infancy, yet the case altogether presented characters of severity which led us to apprehend a confluent form of the disease, and possible pitting of the features after recovery. It appeared to us that the case was one in which it was right to attempt to prevent the disfigurement that, from all

the symptoms, seemed likely to follow—and from the statements as to the success and safety of the ectrotic treatment by application of mercurial ointment, which had lately appeared in the medical journals and elsewhere, I recommended Mr Mackenzie to try this plan. Accordingly the ointment, thickened with starch in the proportions recommended by Dr Hughes Bennett, in his clinical lectures published in the “Edinburgh Monthly Journal,” was for several successive days smeared over the face, and allowed to remain in contact with the skin. Altogether, the ointment continued to be used from the Friday evening to the following Wednesday, the whole quantity employed amounting to not more than  $1\frac{1}{2}$  oz. It was then discontinued, because the period for the maturative stage having arrived, the eruption instead of advancing began to decline and fade away in horny crusts, proving that notwithstanding the severity of the initiatory symptoms, vaccination had retained its modifying power. In fact, all cause for apprehension ceased, and I took my leave of the case, anticipating no further untoward occurrence. Such portions of the ointment as had dried upon the skin were allowed to remain for two or three days after any more had ceased to be applied, when, the state of the surface permitting it to be done, they were carefully removed by sponge and water. Not a particle of mercury had been given by the mouth for any purpose during the whole progress of the case.

It is necessary to state, that when I first saw the patient at the commencement of the eruptive stage, there was sore throat with spots of eruption on the tongue and fauces, and that the voice was, and continued throughout my attendance on the case, feeble and whispering, indicating, in my opinion, an extension of the variolous eruption to the larynx.

On Wednesday, 27th October, just a week after the ointment had been last used, I was requested again to visit this young lady. Two days previously symptoms of mercurial salivation had begun to show themselves, and now she was suffering from an attack of acute laryngitis. By such prompt and active treatment as the state of the patient admitted of, this attack was happily subdued. But profuse salivation, to an extent that I have seldom witnessed, with ulceration of the gums, and inflammation of the mucous membrane of the mouth and throat, continued for fully a week, most intractable to remedies, and causing much constitutional irritation and suffering to the patient, as well as anxiety to her medical attendants. The patient, though now convalescent and able to be removed to the residence of her parents in an adjacent county, still suffers from alteration of the voice, and extreme susceptibility of the vocal and respiratory mucous membrane to atmospheric changes.

It has long been a desideratum in medical practice to find some means of preventing the inflammation, pitting, and disfigurement of the features which are so apt to follow upon severe cases of small-



pox, especially the confluent form of the disease. It is unnecessary here to give the long list of means that have been recommended with this view, but which have successively been abandoned, either from proving utterly inefficient, or from the time and trouble required to treat each individual pustule, or from the pain and inflammation which the treatment itself is apt to cause. Of all these means, the nitrate of silver certainly appears to have been the most effectual, but the two latter objections are found to apply to it, when used to the integuments, in a very considerable degree. I have seen the same as respects the application of nitrate of silver in erysipelas of the head and face. Instead of affording relief, it has, in several cases which I have treated, in the first instance at least, greatly aggravated the pain and swelling. On mucous surfaces it generally agrees better, and I should always recommend the application of a strong solution (as I ought to have mentioned was done in this case) where there is any considerable appearance of the variolous pustules on the tongue and fauces. Such being the objections which have generally deterred practitioners from applying the nitrate of silver to any extent over the cutaneous surface in these cases, it has of late been the custom in certain of the French hospitals to employ mercurial applications. The practice has also been tried in this country and America, and it would appear generally with encouraging results. M. Briquet, as quoted by Dr Copland, says that "if mercurial plaster be applied before the fifth day of the eruption, either the pustules disappear by resolution, or they are changed into vesicles or into tubercles. The mercurial plaster must be kept on from eight to twelve days." More recently the same physician has introduced the particular method of treatment which was adopted in this case, and which as being far cooler, and not calculated to retain offensive discharge, appears to me infinitely preferable to the use of plasters. "He causes," says Dr Copland, "mercurial ointment, rendered more consistent by means of starch, to be spread over the face, and to be renewed once or twice daily. He says that it produces abortion of the pustules, and prevents the swelling attending the confluent form of the variola." Dr Hughes Bennett having been induced to try the same practice in the clinical wards of the Edinburgh Infirmary, gives even more emphatic evidence in its favour. "Its good effects," he says, "in locally modifying the intensity of the inflammation and preventing cicatrices, are unequivocal. The absence of swelling in the eyelids, the freedom from pain in the face throughout the whole course of the disease, and the presence of pit marks only in such situations there as are not covered by the application, show most satisfactorily the advantage of the remedy." So also Dr West, in his lectures on the diseases of children, says, "The weight of evidence appears to me to be in favour of some proceeding of this kind."

It is very singular that not one of these authorities mentions salivation as having occurred in his experience, or as an accident against



which we should be upon our guard. Even Dr Gregory, who is adverse to the practice, objects to it rather on the ground of its having failed under his observation, than of any risk of the specific action of mercury being produced upon the system. Speaking of the various forms in which the application has been made, by plasters containing calomel or corrosive sublimate, or covering the whole surface with mercurial ointment, he merely observes,—“I have seen all the plans fairly tried at the Small-Pox Hospital. The ointment and calomel plasters were inefficient. The plaster of corrosive sublimate converted a mass of confluent vesicles into a painful and extensive blister; but I am still to learn what benefit the patient derived from the change.” Not one word is here said of salivation; on the contrary, the utmost evil that is predicated of the application of mercurial ointment, is its total inefficiency. The natural inference is, that salivation is a rare accident, perhaps from the state of the surface during the eruptive period of small-pox not being such as to favour absorption. And it is worthy of notice, as bearing upon this point, that here there had been no fresh application of the ointment for a week before the ptyalism occurred, only it had remained in contact with the skin in a dry and hardened state for two or three days longer, until the condition of the surface consequent upon the fading of the eruption admitted of its removal.

I am not disposed to attribute any efficacy to the mercurial application in this instance in favouring the early absorption of the pustules, for they followed the same course on the body and extremities where the ointment had not been applied. This favourable result I would rather ascribe to the modifying power of vaccination in early life. The itching and irritation were certainly less felt where the ointment was applied, but the same would doubtless have been the case with any other unctuous application.

The jeopardy in which life was placed from the attack of laryngitis after all risk from the original disease had ceased to be apprehended, it would hardly be fair to insist upon as aggravating the danger of this mode of treatment. Laryngitis would be just as likely to follow upon variola, simply from an extension of the eruption into the larynx, as it is upon the action of mercury on the mucous membrane of the gums and throat. Indeed more so, for it is not a common effect of mercurial salivation; while it is far from uncommon to meet with considerable distress and irritation from the throat and air passages partaking of the condition of the external surface in small-pox. That the specific inflammation of variola had at a very early period of this case extended from the tongue and throat, which were covered with numerous pustules, into the larynx, seems evident from the altered state of the voice at our very first visit to the patient, and which never recovered its natural character. There was thus from the beginning a predisposition, connected with the original disease, to inflammatory action in the larynx. But the period at which the laryngitis became developed, after the abortion of

the external variolous eruption, and after the influence of mercury had manifested itself on the gums, and in connection with a severe and protracted stomatitis from this cause, renders it improbable that it would have occurred, if at all, at least in so formidable a shape, had the patient not laboured at the time under mercurial salivation.

There are circumstances in which it becomes a duty to relate the opprobria not less than the successes of our practice, and the present appears to me to be a case of the sort. It reveals a risk from a particular mode of treatment, pursued in strict conformity to the instructions of those in whose hands it has proved successful, the possibility of which every one would of course admit, but which appears to have been treated of, even by authorities opposed to the practice, as scarcely worthy of being taken into their account. I am far from wishing to say that a new practice is to be judged and condemned by the untoward accidents of a single case. But I cannot help looking upon mercurial salivation as so great an evil, when super-added to the already existing local irritation and swelling of the face and mucous membrane, which usually occur in severe cases of small-pox, that the risk of its occurring in even a single instance, may well affect the general question of its safety and expediency. And that more especially, when we find the drug, as in this case, accumulating and lying latent in the system, and its specific effects manifesting themselves suddenly and with unusual severity some days after its use had been given up, and after all cause of apprehension on the patient's account, either from this source or from the original disease, seemed to be at an end.

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ARTICLE IV.—*Reports and Observations in Surgical Practice.*  
By E. R. BICKERSTETH, Surgeon, Liverpool.

*Large Hæmatocele of the Thyroid Gland, causing urgent Dyspnœa by pressure upon the Windpipe—Treatment by Free Incision—Cure.*

ON the 27th of September last, I was consulted by Mrs M——, regarding a tumour which had been growing on the front of her neck, for upwards of three years, and had latterly, from its increasing size, rendered her breathing exceedingly laborious, and deprived her almost altogether of her voice.

The tumour, which presented the ordinary appearance of a large bronchocele, was situated directly in the median line, and extended from the top of the sternum to the upper margin of the thyroid cartilage. It was of nearly globular shape, and possessed a perfectly smooth and regular surface, except at one part near the upper and right border, where it was larger and more prominent than elsewhere. It followed the movements of the larynx during the act of swallowing, but sluggishly. Fluctuation was readily detected throughout the

entire tumour, but was by far the most distinctly evident over the more prominent portion, at which part the fluid appeared to be quite superficial. In this situation I introduced a small hydrocele trocar and canula, and drew off, in a full stream, from four to five ounces of thin dark fluid, such as is usually met with in idiopathic hæmatocele. The tumour disappeared, and my patient experienced immediate relief. She returned home, and I did not see her again till the 4th of October.

Her breathing was then most distressing; the tumour was fully its former size, and she stated that it had increased to this magnitude, and that all her symptoms had returned, in less than twelve hours after her previous visit. The integuments around it and over the upper part of the chest were discoloured by ecchymosis; there was no tenderness or sign of inflammation. Fluctuation was as distinct as before.

From the consideration of the circumstance that the tumour was composed of a single cyst, and that its contents were entirely fluid, as apparently proved by the former tapping, admitting, consequently, of complete evacuation, I thought that injection with tincture of iodine, after the sac had been completely emptied, would afford a reasonable prospect of inducing its obliteration; and I was further encouraged to attempt this treatment by the recollection that, even if it should fail, no obstacle would be presented to the more severe measure of laying the cavity freely open.

Three drachms of the pure tincture of iodine were placed in a glass syringe, and I prepared to inject, having first emptied the cyst as far as possible. About four ounces of dark fluid blood, much thicker and more tarry than upon the last occasion, passed freely through the canula, and the swelling was removed as effectually as before. But now florid blood began to ooze very rapidly, and continued to do so in spite of the means used to check it. On this account the injection was discarded, for I feared the free hemorrhage would prevent the tincture from coming into contact with the walls of the cyst, and thus fail in producing the requisite degree of inflammation; moreover, I did not like the consequences of mixing tincture of iodine with fresh blood in the interior of a cyst occupying this situation. The canula was therefore withdrawn, and perhaps it was as well, for within five minutes the tumour was almost as large as ever. Still the relief to her immediate distress was considerable, and at her request a short delay was granted, before proceeding to more radical treatment.

*October 8th.*—She is now earnestly desirous that something should be done immediately to relieve her. The breathing is more laborious than ever. The inspiration prolonged, and performed with considerable effort. Each attempt is accompanied by a peculiar crowing sound. The tumour is very firm and tense; and the skin is ecchymosed over the whole of the neck and chest. The patient being seated upon a chair, with the head thrown back and supported

by an assistant, I introduced a sharp-pointed bistoury into the most prominent part of the tumour, a little to the right of the mesial line, and having opened the cyst, I enlarged the wound downwards to the extent of about two inches, by means of a curved probe-pointed bistoury. I now ascertained that the cyst was of much greater size than I had previously imagined. It passed downwards and backwards deeply into the root of the neck, behind the sternum and in front of the trachea, further than my finger would reach; and from this part I was able to turn out large clots of dark solid blood. Hitherto there had been very little hemorrhage; but, in order to treat the case effectually, it appeared necessary to make the external opening still larger, for as yet not more than one-third of the known extent of the cavity had been opened. But at the lower part its depth from the surface greatly increased, and a considerable portion of the substance of the thyroid was evidently placed before it, so that by cutting in this direction, the danger of wounding important vessels was great in proportion. Using my knife in the same manner, I again extended the incision to within half an inch of the top of the sternum, and thus laid open the cyst as freely as it was possible. Even then I could not find the bottom of the cavity. The hemorrhage from this last incision was very profuse; the blood spouted in every direction as from a sponge, and proceeded manifestly from the divided walls of the thyroid. After the loss of a few seconds spent in a vain endeavour to secure the vessels by ligature, the cavity was stuffed with lint till the cut surfaces were widely distended, and then, by drawing a few straps of adhesive plaster from side to side, a uniform and steady pressure was secured, which immediately and effectually checked all further bleeding.

The future progress of the case has in all respects been most satisfactory. Not a single unfavourable symptom occurred. On the third day discharge from the wound commenced, and part of the lint came away; and at each succeeding dressing a portion separated, until on the eighth day the whole had been discharged. Then the cavity began to contract, and the swelling and discharge to decrease. At the same time the margins of the wound approximated, and showed so great a tendency to unite, that it was necessary every second or third day to introduce my finger, in order to keep the opening patent until the obliteration of the cyst was complete, etc. To hasten the process, I directed a blister to be applied, and with most beneficial results. The discharge became thin, and of small amount. The edges of the incision puckered in, and have now (five weeks after the operation) all but closed, forming a deep furrow, with the divided lobes of the thyroid on either side. The breathing has continued perfectly free, and the voice also is slowly recovering its natural tone and force.

The perfect relief afforded by this treatment to the most urgent and distressing symptoms was most gratifying, and the simplicity of

the operation, together with the absence of any unfavourable occurrence, is highly encouraging for future guidance.

The real, as compared with the apparent, size of the cyst,—its extension downwards so much further than was indicated by external examination,—its partially solid contents, when fluid alone was discovered with the trocar, and every reason existed for supposing that none other was present,—are facts not to be forgotten, and must always more or less influence our diagnosis, until an actual exploration with the finger has been effected. It may be remembered also with advantage how utterly useless in the wounded thyroid the ordinary method of securing the vessels by ligature proved, and how easily profuse arterial hemorrhage was stopped by continued pressure.

*Hæmatocele in Cheek of a Boy æt. Five Years—Incision—Cure.*

Edward D——, æt. five years, a healthy-looking and most intelligent boy, was brought to me, on the 24th of September last, on account of a large swelling of the right cheek. It had commenced, without any assignable cause, rather more than a year before, and had since been slowly increasing. Occasionally it was observed to vary considerably in size during the course of a single day; but at other times its growth was steadily progressive. He had never complained of pain or uneasiness from it.

The tumour was about the size of a hen's egg. It was situated in the centre of the cheek, and caused a considerable bulging both externally and internally. Fluctuation was perfectly distinct from one side to the other. Suspecting, from the history and from the position of the tumour, that the swelling was caused by dilatation of the duct of the parotid gland, I made search for the orifice of that canal, and was somewhat confirmed in my conjecture by finding that the opening was exceedingly minute, so that only the finest hair probe could with difficulty be insinuated along the passage. No direct obstruction was encountered, but a small quantity of clear fluid escaped immediately upon its withdrawal, which was more than had been previously observed. Pressure upon the tumour did not increase the flow, neither was there any sensible diminution of its size. Under these circumstances, it appeared advisable to make a free incision, and thoroughly evacuate the contents of the swelling. This was accordingly done from the inside; but, instead of a clear fluid as expected, a quantity of thick tarry blood escaped into the mouth, and the tumour disappeared. With my finger introduced through the wound, I was now able to feel the precise extent of the cyst, and care was taken to lay it as freely open as possible. No bleeding of consequence occurred. A little lint was put into the cavity, and this was changed every second day for a fortnight, when the wound had soundly healed, and the cure was completed without

any traces of the deformity. The duct of the parotid was not injured. It continues patent, and the secretion passes without obstruction.

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ARTICLE V.—*Notes of Observations at the Field-Hospital of Rangoon, and the Convalescent Hospital at Amherst, during the late Military Operations in Burmah.* Communicated by the Medical Officers.

THE army which invaded Burmah last spring was furnished with an hospital ship of 800 tons burden, the main-deck of which was to be exclusively applied to the reception of the sick and wounded. A convalescent hospital, which it was at first proposed to establish at Moulmein, was subsequently fixed at Amherst, thirty miles down the coast from that place, and right across the bay from Rangoon. This locality has been for some time noted for its salubrity. When the troops were landing for the attack of Rangoon, a priest's house, outside the great stockade on the river bank, was taken possession of for a field-hospital. At a later period more roomy quarters were found within the town.

The hospital-staff consisted of Dr Montgomerie, superintending surgeon and medical chief of the expedition, with five assistant-surgeons, Dr J. Fayer, Dr W. White, Dr A. Christison, Mr J. P. Bowling, and Dr G. M. Govan.

The field-hospital at Rangoon was brought into immediate requisition when the troops landed on the 12th April, and advanced to attack the town. The hospital-ship was put to less use than had been intended, as it had been allowed inadvertently by the embarkation officers to be too much lumbered with army stores.

#### I.—NOTES ON THE CLIMATE OF RANGOON.

It now fully appears that most mistaken views had been formed of the insalubrity of the climate of Burmah, and especially of Rangoon. In the former war the troops were defectively supplied with ill-preserved provisions, so that scurvy broke out extensively among them. It was not wonderful, therefore, that diseases at large should also ravage the army, and particularly endemic and epidemic diseases. Besides, there is reason to suppose that, during our occupation, an epidemic fever, of rare occurrence in Burmah, prevailed throughout many parts of that country besides Rangoon.

The experience of our troops hitherto forms an agreeable contrast to that of their predecessors in 1825. Cholera and dysentery broke out at the first with rather alarming violence. But this unlucky incident could be satisfactorily referred to the great exposure and



temporary privations inseparable from the abrupt commencement of operations before Rangoon. These diseases subsequently disappeared in a great measure. Nor has fever ever been prevalent during the four months of May, June, July, and August, although this period comprises a material part of the rainy season. It is true that the troops have still to encounter the more trying season which follows the departure of the rains in November. By that time, however, the greater part of the army will be as high up the river Irrawaddy as Prome. In the month of August the casualties were only sixteen in the European force, and six in the native corps; and on the 22d September the returns for that month promised to be even more favourable.

Very erroneous ideas had been formed as to the amount of rain which falls at Rangoon. At Moulmein, towards the upper part of the Bay or Gulf of Martaban, the annual average fall is 150 inches. But the officers of the Field Hospital have been informed by a gentleman, who had resided some years at Rangoon, that the fall there never exceeded 90 inches; and down to 1st September observations of the rain-guage, made from 8th May by Dr Fayrer, showed a fall of  $68\frac{1}{2}$  inches; so that, for the present year, the total fall will scarcely exceed 80 inches. [This amount does not exceed the mean fall of rain at Arddarroch, half-way up Loch Long in Dumbartonshire.] During the rainy season at Rangoon there are many days when the rain ceases for a time, so that Europeans may go out for exercise, and might do so with tolerable comfort, were it not for the muddiness of the roads.

The minimum temperature observed at sunrise in May was  $73^{\circ}$ ; June,  $74^{\circ}.5$ ; July,  $74^{\circ}$ ; August,  $75^{\circ}$ . The maximum at noon in May was  $96^{\circ}$ ; June,  $90^{\circ}$ ; July,  $88^{\circ}$ ; August,  $87^{\circ}$ ; and at three P.M. in May,  $95^{\circ}$ ; June,  $90^{\circ}.5$ ; July,  $89^{\circ}$ ; August,  $88^{\circ}.5$ . At all periods the temperature and sense of heat are mitigated after two o'clock, either by a sea-breeze which springs up about that time in the dry season, or by thunder and rain, as the evening succeeds a forenoon of unusual heat. The moisture of the atmosphere is, of course, great during these months; and often the difference of temperature between the dry and wet-bulb thermometers is very trifling.

The table on the succeeding page gives the monthly abstracts of the thermometric and barometric observations kept by Dr Fayrer.

I.—THERMOMETER.

		SUNRISE.			9 A.M.			NOON.			3 P.M.			SUNSET.			9 P.M.		
		Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
May	Wet	78	73	75.37	80	75	77.98	81	71.5	73.41	80.5	74	77.59	79	74	76.64	78	73	76.04
	Dry	80	74	77.27	88	76	82.29	96	82	85.5	95	75	83.19	87	75	80.17	83	74	78.46
June	Wet	78	74.5	76.64	80	75	78.41	80	74.5	78.66	80.5	74.5	77.92	81	73.5	77.26	79.5	74	76.86
	Dry	80	76.5	77.84	84	76.5	81.19	90	76	83.02	90.5	76	82.28	86.5	74	80.05	83	75	78.56
July	Wet	79	74	76.5	79.5	75	77.25	81.5	75.5	78.5	83	74.5	78.75	79.5	73.5	76.5	79	74	76.5
	Dry	81	75	78	84.5	76	80.25	88.5	77.5	83	89.	75.5	82.25	81.5	75	78.25	82	75	78.5
August	Wet	78	75	76.09	80.5	75.5	77.77	81	77.5	78.63	80	74.5	77.43	77	74	76.15	79.5	74.5	76.59
	Dry	79	76	77.18	83	76	79.88	87	77.5	81.87	88.5	75	80.41	82	74.5	78.0	81.5	75	78.02

II.—ANEROID BAROMETER.

May	29.96	29.88	29.91	30.0	29.89	29.94	30.0	29.87	29.92	29.96	29.84	29.89	29.96	29.85	29.90	30.0	29.88	29.93
June	29.98	29.88	29.93	30.02	29.91	29.95	30.0	29.91	29.94	29.98	29.87	29.91	29.98	29.88	29.90	29.98	29.80	29.95
July	30.0	29.83	29.91	30.03	29.86	29.94	30.0	29.83	29.91	29.98	29.82	29.90	29.98	29.84	29.91	30.02	29.84	29.93
Aug.	30.07	29.91	29.97	30.09	29.92	29.99	30.05	29.93	29.97	30.02	29.89	29.95	30.03	29.91	29.94	30.07	29.91	29.98

## III.—PLUVIOMETER, WINDS, ETC.

May.—Quantity of rain registered in pluviometer 11·79 inches;—about two inches lost by oversetting of pluviometer on the 12th. Prevailing winds in the latter part of the month south and south-west, in the early part unsteady, and occasionally north-east and north-west. Several heavy squalls of wind with rain, and at night thunder and lightning. Since the first shower fell the air has been much cooler.

June.—Quantity of rain 16·43 inches. Prevailing wind south-west. First ten days of the month fine—with rain in showers, occasional squalls with thunder, weather close and sultry at times, and the air damp; sky cloudy. During the middle and latter parts of the month sky cloudy, temperature cool, and air damp. Wind latterly very squally in the afternoon and night, attended by thunder, violent gusts, and heavy showers of rain, though short in duration.

July.—Quantity of rain 21·35 inches. Weather during the early part of the month fine, especially in the day time. Wind generally from south-west, with occasional squalls and showers. Air cool, but very damp. On several days so little rain fell, as not to be appreciable by the guage. The heaviest falls were on 9th, 10th, and 12th. The latter part of the month wetter. Prevailing wind still south-west, but shifting occasionally to north and north-east. Rain has fallen more in the day time than in the earlier part of the month.

August.—Quantity of rain 17·07 inches. During the first half of the month the weather has been wet, and the prevailing wind south-west. During the third week the wind was variable. Frequent squalls, with rain and thunder, the wind changing frequently and suddenly, and the barometer keeping high. The latter part of the month was fine, warm, with occasional showers in the last three days. Wind variable, but much from the south-east.

## II.—SURGICAL CASES ADMITTED INTO THE FIELD HOSPITAL AT THE CAPTURE OF RANGOON.

In general the wounds were flesh ones, passing through the muscular and other soft textures. The result of these was almost always favourable, though the progress in some cases was very tedious. Among the rarer cases are the following:—

1. *Gunshot Wound of the Lip.*—A Sepoy was struck in the raphe of the upper lip by a musket ball; on examination no fracture or injury to the mouth was detected; but on everting the lip a wound was seen on its mucous surface, through which the ball was easily removed.

2. *Gunshot Wound of the Tongue.*—A fine tall soldier of the 80th was hit at the base of the tongue, the ball grazing the lower lip as it entered the mouth. Alarming hemorrhage continued for some time. A ligature was applied to the tongue, by which the bleeding was arrested, and afterwards the part separated. The ball was extracted from the proximity of the body of the jaw in a torn condition resembling honeycomb, caused possibly by the variously disposed muscles it encountered. The man got quite well.

3. *Gunshot Wound of the Abdomen, non-penetrant.*—Lieut. —, on the advance, felt a rude shock in the abdomen which caused him to whirl round. He walked on, but immediately felt sick and fell down, and on putting his hand to his side he felt blood flowing out. He was then taken to the hospital. The ball had entered a little above the umbilicus, and emerged at a distance of four inches hori-

zontally to the left. It was thought the peritoneum was touched, but though he was considerably depressed at first, he very rapidly recovered.

4. *Gunshot Wound of the Bowels.*—Capt. —, of the Queen's service, a large, handsome man, was brought to the Field Hospital on the 12th April, with a wound of the abdomen. He had a sunken aspect, with coldness and feeble pulse. The ball entered, it is believed, from below, viz., through the left os ilium about two inches from its anterior spine, and made its exit  $1\frac{1}{2}$  inch above, and to the left of the umbilicus.

He was transferred to the hospital ship on the 14th, in a very depressed state, with cold, clammy skin, rapid, feeble pulse, and distressing hiccup; but he made little or no complaint, and merely called for something to drink. The anterior wound had a sloughy appearance, and well formed feculent matter passed freely out by it. The stomach was very irritable; and soda water and similar fluids, which were all he could take, would not remain on it. An injection caused no motion per anum. To the wound was applied a hollow sponge covered with oil-cloth and retained by a bandage; oil-cloth was put on the bed; and by frequent changes he was kept tolerably clean; but a copious oozing of fluid matter could not be restrained.

On the 16th port wine was given, with obvious benefit, and that in considerable quantities. On the 18th he became much weaker, with slight unconsciousness and restlessness. Stimulants were of no avail—the skin was all along cold and clammy, and a cold perspiration was on the forehead always. The wound showed no change. The patient betrayed little interest in anything that was going on. In the evening of the same day he became quite quiet, and expired so easily that the time could hardly be told. It was about nine P.M. He had thus lived six days, a very unusual length of time after such an injury.

On examination, the abdominal parietes contained fat to the thickness of  $1\frac{1}{2}$  inch. The anterior wound was found to lead into a large sac in the fatty and muscular tissue which was stained brown by the feculent matter it contained; an opening from this led to a fold of small intestine which adhered by soft lymph to the parietes. The intestine generally was dry and of a deep brown colour, and loosely adherent in many places. The fold of intestine was nearly quite cut through by a wound; the course of the ball was then through the mesentery, and again through a fold of small intestine; and lastly, the ilium was much shattered.

The other organs appeared healthy.

5. *Gunshot Wound of the Fore-arm.*—Lieut. —, of the Bengal service, was struck on the back of the arm over the left radius, near the wrist; the ball passed five inches up the arm and fractured the ulna severely as it passed out. Several pieces of bone were removed at the time, and some small pieces about a fortnight after. The

radius did not appear to be injured. Notwithstanding the extensive injury, no bad consequence followed, and the cure was complete in eight weeks.

6. *Round-shot Wound of the Chest.*—A fine young man of the Bengal artillery, was struck by a round shot when serving his gun. On being brought to the hospital it was found that the ball had lacerated the left arm internally, and then passed through the left side of the chest. The wounds were large and lacerated, and behind the finger could be passed into the thorax. The loss of blood was great. The poor man, nevertheless, lived five hours.

7. *Gunshot Wound of the Abdomen.*—Lieut. —, of the Bengal service, received a ball through the abdomen in a very similar way to Captain —; but there was no escape of the contents of the bowels, nor any proof of their being wounded. He was much depressed, and never rallied; suffering much from dull pain, which was greatly relieved at intervals by chloroform. He died at twelve P.M., about fourteen hours after he was struck.

8. *Gunshot Wound of the Lungs; Bullet found within the Heart.*—A private in the 80th, a stout well-made man, was struck on the 14th April, on the left shoulder by a musket ball about an inch to the outside of the coracoid process. The course was then downwards and inwards into the thorax. The breathing was at once interfered with, being short and catching, with cough and bloody sputa; and there was considerable emphysema of the cellular tissue near the wound. He went on very well, though obviously getting thin and pale, and expressed himself as wonderfully easy. The chest in time contracted, while percussion became dull, and the respiratory sound could not be heard, while on the right side the sound became puerile.

On the 5th May he was removed to the depot at Amherst; there he gradually became thinner and weaker, till he was reduced to a skeleton, at the same time he continued to say he was "very well, considering." The side was now resonant, but there was no respiratory sound. Emphysema re-appeared after being absent for several weeks. About the end of June he began to sink, and one evening he suddenly expired.

On dissection, the course of the ball could not be traced among the textures of the shoulder; but between the second and third ribs it passed obliquely through a narrow canal with cartilaginous sides, and then through the costal pleura; a large abscess occupied the cavity of the pleura, except superiorly, where there was air; and the pleura was much thickened. The lung was very much condensed and pressed towards the heart, an opening in its pleural covering showed the continuation of the course of the ball, and this was farther traced as far as the root of the lung, where the examiner failed to trace it further. In the lung was found a piece of red cloth, and another of white cotton, closely appressed. On opening the pericardium, the apex of the heart appeared thickened, and a

hard body was distinctly felt at that point. When the cavities were laid open, the musket-ball was found in the left ventricle, lying at the apex, with a thin covering of white lymph partly covering it.

No injury to the heart could be found, nor any evidence of diseased action. The right lung was healthy, as well as the other organs of the body. The heart, as found, was put in spirit, to be sent to Calcutta.

[This is one of the most strange cases on record. The only conceivable way by which the ball found its way into the ventricle is by one of the pulmonary veins first into the auricle, and then by the mitral valve into the ventricle. I am sorry I did not see the whole dissection, as the tracing of the ball at the root of the lung would have been most interesting. But I saw the heart as it lay before it was opened, and felt the bullet at the apex.—A. C.]

9. *Gunshot Wound of Ilium and Sacrum.*—A soldier was struck at the taking of Martaban by a musket-ball, which entered behind the anterior spine of the right ilium, and made its exit over the middle of the sacrum. Both wounds discharged greatly, and the man became emaciated and weak to the last degree, suffering severely in the leg, which he kept entirely motionless, supported by pillows, in a direction across the other limb. Eight weeks elapsed, and he expired.

On examination, extensive sloughing abscesses were found passing in an upward direction, and likewise down the thigh, from the line of wound. And, finally, it was found that the ball had passed through the cotyloid ligament of the hip-joint, fracturing the acetabulum severely, and emerging at the opposite side of the joint, continued its course to its point of exit over the sacrum.

The fractured pieces were denuded of cartilage and ununited, and the head of the femur was entirely denuded, and showed the cancellated bone underneath, without any attempt at reparative action. The soft textures of the joint were much inflamed.

10. *Gunshot Wound of the Brain.*—A sepoy received, on his right frontal bone, on its upper aspect, a ball, which penetrated the skull. The only remarkable point was that the left eye had an undue degree of protrusion. The man was quite sensible for twenty-four hours; but on the second day, his senses left him; coma came on, and he expired the same day.

The ball was found lodged over the roof of the left orbit, having previously traversed the anterior part of the brain.

11. *Gunshot Wound of the Shoulder.*—A European soldier was struck on the right shoulder near the acromion process by a musket-ball; it entered, but its course could not be traced; rapid and extensive inflammation followed, and matter formed in the whole region of the right chest, anteriorly. Free incisions were made. The man suffered very much, and leaned his body greatly to the affected side. An abscess formed after six weeks at the fold of the axilla, and on incision the ball dropped out; some time after which a piece of red



cloth came away by the same opening. The swelling diminished, the discharge lessened, and the wounds healed up; showing at last, that the only injury was fracture of the clavicle, which was found to have both fractured ends forced backwards. He is gradually becoming more erect, and is slowly regaining the use of the shoulder-joint. His constitution was strong, or he never could have passed through so much suffering and so exhausting a drain from his wounds.

12. *Gunshot Wound of the Head.*—A sepoy was struck on the 12th April by a musket-ball behind the left ear; the ball passed through the mastoid process and entered the petrous portion of the temporal bone; but its further course could not be found, nor was it made out whether the ball had come out again. Complete paralysis of the left side of the face followed, with giddiness and confusion of ideas, and considerable pain. Some pieces of bone were removed, and water dressing was applied. On the 17th there was a good deal of foetid discharge from the wound; and on the 20th he lost the use of his left eye and left ear. On the 2d of May he was attacked with symptoms of bronchitis, for which he got purgatives and expectorants. On the 11th there was some pneumonia; and on the 13th he was better again, but with a quick weak pulse. On the 24th he was feeble and emaciated; but the chest symptoms were gone.

On the 1st of June the giddiness was worse, with beating in the head, and continued loss of vision and hearing; small pieces of bone still came away. On the 19th he was getting so stout, that his diet was diminished, and exercise enjoined. On the 11th of July the symptoms of giddiness continuing, he took a fit of epilepsy, for which he was bled, and had cold applied to the head. On his recovery from this, the other symptoms remained as before. On the 14th of August he had still the paralysis and other symptoms; and though his general health was improved, his manner was peculiar and idiotic. The discharge from the ear continued; but the point of entrance of the ball was healed up. There is little doubt the ball is still in his head.

Thirteen amputations were performed in the hospital for wounds received at the capture of Rangoon. Five were of the thigh. Of these, one died soon after the operation from shock; and another, whose limb was removed secondarily a fortnight after the action, died of hectic. The other three got well. Three were above the elbow, and all did well; three were below the knee, and all recovered; one was above the wrist, and also recovered; one lost a finger, and got well.

Some time afterwards, Mr ——— of the H. C. "Medusa," at the attack on Prome by the steamers, had his arm carried away by a round shot. The arm was amputated at the shoulder-joint, and he got on very well, starting finally for Calcutta in progress of cure.

## III.—GENERAL CASES AND OBSERVATIONS.

13. *Stricture and Perineal Fistula*.—The following case came under notice at Amherst:—An officer of Her Majesty's service arrived on sick-leave at the general hospital at Amherst on the 7th June. He ate a fair breakfast that morning, and said he had not done so for many a day at Rangoon. He had been operated on in London two years ago for urethral stricture by internal incision; and he said he was cured, and that he was quite well when he came to Rangoon, but that he used bougies now and then himself. His appearance was sallow and unhealthy in the extreme, and after the first day his appetite became capricious, and he ate almost nothing. The symptoms of stricture had returned; but no instrument could be passed through it. On the 20th sudden and alarming sinking came on, with severe pain in the hypogastric region: the hands and arms were cold and clammy, the pulse imperceptible at the wrist, the face of an anxious expression. He said it was of no use trying to pass instruments, but talked incoherently. Instruments could not be passed. The bladder was not over distended. Urine of ammoniacal odour came away in the bed.

He was treated with laxatives, enemata, suppositories, full opiates repeatedly, and warm fomentations. No rally took place, and he expired on the forenoon of the 14th of June.

On examination of the abdomen, the anterior aspect of the liver and omentum, and the peritoneal surface of the anterior abdominal wall, were covered with a thin layer of soft lymph. On removing the liver, an abscess gave way, which was found to occupy its posterior right portion, being as large as an ostrich-egg. The texture of the liver was friable, and deeply congested near the abscess; its pyogenic membrane was soft, and the pus well formed. The kidneys were congested, but healthy. The bladder, and the entire urethra, with the rectum, were removed. When the urethra was laid open, a thickened and constricted portion was found immediately behind the bulb; but the passage was free, and the bulbous portion itself ulcerated. At the prostatic part a false passage led between the bladder and rectum, and another entered the gland itself, the whole right portion of which constituted an abscess, with thin ulcerated septa of tissue covering the cavity. The gland was a little enlarged. The bladder was thickened, and contained half a pint of thick muco-purulent fluid. Its whole mucous membrane was covered with a thin layer of lymph, and its vessels were much congested. The hemorrhoidal vessels, and those on the peritoneal surface of the bladder, were highly congested. The organs of the chest were healthy.

[*Note by the Editor*.—This gentleman was far from being cured when he left England. On the contrary, in 1850 he had suffered so much, that, after a short correspondence from the south of England with Professor Syme, he had resolved to repair to Edin-

burgh, and place himself under that gentleman's care; and had actually written to bespeak lodgings for the purpose. Soon afterwards, however, he set off for India with his purpose unfulfilled.]

14. *Wound of the Radial Artery by a Bullock.*—A native of Bengal was brought to the Field Hospital of Rangoon on 31st August, considerably exhausted by hemorrhage from a lacerated wound of the left fore-arm, caused by the horn of a buffalo, which he was milking two hours before. The wound extended four inches along the line of the radial artery. The muscles were torn and separated from each other; and the artery, to the extent of two inches, was divided and separated from all connections, and lying upon the skin of the arm, its end torn, and blocked up by a coagulum, so as entirely to prevent hemorrhage. Each beat of the pulse raised the artery slightly; and this went on quite regularly. A ligature was applied close to the muscles, and the remainder cut away. The distal end was looked for, but could not be readily detected. As there was no bleeding, it was not further searched for. \* \* \* Since the sloughs have separated, no hemorrhage has occurred, and the wound is fast closing.

15. *Snake-Bite.*—On the 20th of August the body of a stout dooly-bearer was brought to the hospital at seven A.M., with the following history:—Last night at eight o'clock, on passing by the lines of the 80th to join some friends, he was bitten by a snake on the right ankle, as he told them—showing the bitten part with a drop of blood on it. He sat and smoked with the others for about an hour, when, they say, he became insensible and “like a stick,” with slight spasms and vomiting. They then performed incantations and other native ceremonies over him. An officer passing sent them to the 80th hospital; by the time they reached it the man was found to be dead: this was about three hours after he was bitten. The body was sent to the Field Hospital.

On examination, no mark of bite or wound could be detected. The inside of the right ankle was a little swollen, but not discoloured. The skin was dissected off, and serous fluid escaped; while at one point, dark discolouration about an inch in diameter was observed. On holding the skin to the light, some thought a small perforation, corresponding to the discoloured portion could be seen, but there was great doubt about it. The body was quite healthy—the venous system of the brain, large vessels, etc., was congested, the blood everywhere fluid.—No such occurrence has taken place for a number of years.

16. *Death of an Opium Eater.*—A sergeant of Her Majesty's 51st was gradually wasting away without any apparent reason, unless dyspepsia would account for it: he was sent to Amherst for a change. His appearance was sallow and unhealthy, and his movements were very languid and depressed. No treatment did him any good. It

was suspected that he ate opium. On being asked he resolutely denied it, and even did so on the very day he died. No one ever saw him take opium at Amherst, but there was a hint of his having taken it elsewhere: he one day said, "I was allowed a good deal of laudanum when I was in hospital at Rangoon." He died in a state of great langour and exhaustion, and much emaciated. His friends said his affairs were in a greatly entangled condition.

On examination after death, the abdomen contained about half a pint of sero-purulent fluid, and the intestines were dry, and loosely adherent to each other. In the region of the cœcum, on attempting to remove the intestine, an abscess gave way, which was found to involve the cœcal portion of the small intestine with the ilio-cœcal valve, and the caput cœcum. It contained sloughy and purulent matter, among which there were several black angular fragments, of the bulk of a pea, which, from their odour and general appearance, there was no doubt were fragments of opium. These may have set a-going, by mechanical irritation, the action which caused the abscess. A mass of opium, weighing an ounce, was found concealed in the bed.

Almost the only diseases prevalent at Rangoon were cholera and dysentery.

*Cholera.*—The first appeared only at the commencement of operations against Rangoon, and was obviously the result of exposure and privation, combined with the excesses which men give way to on such occasions. The wing of the 80th came from Moulmein on board a transport. The men were ill-supplied with food, and had been irregular previously at Moulmein, having got access to wine and spirits, and drunk a large quantity. The result was, that the very night we came off Rangoon, our hospital-ship had to receive seven cases of cholera from that corps, and of these four died. The other cases occurred in the field: and this is not to be wondered at, when it is considered, that for three successive days they were exposed to a scorching sun by day, and to heavy dews on the bare ground by night; and that they drank as much as they could get, both of bad water and bad spirits. The effect of all this continued when they got into quarters; so that the disease did not disappear till a fortnight afterwards.

*Dysentery* at once began, and from it was most of the mortality.

Its course was either quickly fatal, with bloody evacuations, rapid emaciation, pallor and sinking, without much pain; or it lingered for many weeks, assuming the form of chronic diarrhœa,—the stools being frequent, scanty, thin and pale, with streaks of blood occasionally, great emaciation, feeble quick pulse, raw, or white-coated tongue. Few got better who fell into this state. The acute cases had a better chance.

The appearances in the dead body were the usual signs of inflammation and ulceration of the large intestine and ilio-cœcal region:

and, almost without exception, abscess of the liver. The only case without this had the most fatty liver I ever saw. Of 230 deaths which had occurred in the force at the end of July, 150 at least were from dysentery.

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ARTICLE VI.—*On the Administration of Iron as one of the Indications to be pursued in the Treatment of Inflammation.* By R. W. CRIGHTON, M.D., Edin., etc.

HAVING, in the Number of the "Monthly Journal" for June 1851, read the paper of Dr Charles Bell and Mr G. H. Bell, on "The Treatment of Erysipelas by the Muriated Tincture of Iron," I gave the remedy a trial, in several cases which occurred in my practice, shortly afterwards, and finding its administration followed by beneficial results in all of them, I was led to inquire into the *rationale* of its mode of action.

The cases of erysipelas thus treated amount to six in number, and present no features of sufficient interest to merit their being related in detail, farther than that all were adults of previously robust constitution, that in two the disease was the result of external injury applied to the face, and that in a third case the cerebral excitement was very great. In all the improvement was rapid, and equally as much so in the cases of traumatic as of the idiopathic form of the disease. The remedy was given as directed by the Messrs Bell.<sup>1</sup>

During the discussion at the Edinburgh Medico-Chirurgical Society on the paper adverted to, it was suggested by Dr Seller, that the tincture of the muriate of iron might operate on the chemical character of the urine; but in none of the cases in which I employed it could I observe a marked increase of any of the principles of that secretion. Its mode of action in erysipelas, as in inflammation occurring in other textures, seems to me to be explained only by a reference to the pathological changes occurring during these states. Inflammation, whether caused by external agency, or arising from various changes occurring within the animal economy, is now generally considered as a process of "abnormal nutrition," the primary change in which is an alteration in the vital properties of the inflamed part, the power of selecting the due amount of nutritive material being diminished, while the attractive force is much increased. Hence arise the changes in the blood and blood-vessels, of which exudation is the chief. Of the other phenomena occurring in inflammation, to which reference will be made, are diminution of

<sup>1</sup> I have more lately given the saccharated carbonate of iron in erysipelas with equally good results.

the corpuscles and increase of fibrine ;<sup>1</sup> and as the latter is in a great measure derived from the former, and chiefly to be considered as effete matter, we may conclude that the iron set free by the solution of the *red* corpuscles becomes likewise such, and is separated from the circulation.

Although we may never be able to counteract the primary alteration occurring in the inflamed part, yet the results arising from it, especially those occurring in the blood, are more open to observation, and should regulate the method of treatment.

On these principles, blood-letting, in cases otherwise indicating its necessity, is now generally limited to the early stage, with the view of limiting it before exudation has taken place to any extent ; while, after it has occurred, means calculated to diminish the rapidity and force of the circulation, and rouse the absorbents to remove the exuded material, are resorted to ; but so far as I am aware, a proposal to remedy the diminished amount of the red corpuscles (the chief amount of the corpuscular element), has never yet been made. The cases of inflammation in which I employed iron have been three in number, two of pneumonia and one of pleurisy ; and though in all of them I gave the tartarised antimony in doses of a quarter of a grain every three hours, yet I cannot avoid attributing, in a great measure, their speedy convalescence to the use of the iron, as in all this was more rapid than I have ever seen under other treatment, and such as to excite the astonishment of the patients.

CASE I.—*April 2d*, 1852.—E. S., a factory girl, aged 16, states that she has enjoyed good health till within the last three weeks, during which time she has been troubled with slight cough ; after rigors, she was yesterday seized with acute pain of left side, which has continued ever since ; skin hot and dry ; pulse 120, of good strength ; tongue clean ; bowels opened by medicine. On percussion, there is slight dulness at the lower angle of left scapula ; and on auscultation, loud friction is heard at the termination of inspiration ; sonorous râles heard over same side superiorly ; has had considerable delirium during last night. Ordered Antim. Tart. gr.  $\frac{1}{4}$  every three hours, and ten drops of Tinct. Ferri. Sesq. every two hours ; a blister to be applied to the side.

*April 3d.*—Has slept well during the greater part of the night ; pulse 96 ; no pain on ordinary inspiration ; face less flushed, and heat of skin less. She improved rapidly afterwards, and was able to resume her work the next week.

CASE II.—Mr K., aged about 45, seen June 28th, 1852. Has previously enjoyed good general health, is moderately robust, and of

<sup>1</sup> Andral gives as evidence his analysis of blood in venesection, performed in several cases of inflammation, the fibrin in all being increased, while the corpuscles are diminished. Simon also states, that in erysipelas the corpuscles are reduced to 100 per 1000, and in pneumonia to 122.



temperate habit. Two days ago changed his linen while perspiring freely, afterwards felt chilled, and the same evening had pain in the lower part of the right side, with hard dry cough; pulse 100, small; now much troubled with cough; sputum rusty coloured; pain still continues. Percussion over rather less than the lower third of the right lung considerably impaired. On auscultation, crepitous râle distinctly heard over the same space. To have the antimonial solution and Tinct. Ferri. Sesq. gtt. xv. every three hours.

*June 30th.*—Much relieved; rusty coloured sputum nearly gone, as also pain. Repeat the muriated tincture of iron.

*July 2d.*—Continues to improve. In two or three days more he was able to attend to his ordinary duties.

CASE III.—Ambleton-Peak Forest, seen August 10th. Some days ago, while returning from the wakes in a neighbouring town, where he had been drinking freely, he got drenched with rain, and felt a sharp pain in the left side. Has been since troubled with cough, and has had rusty coloured sputum. On percussion, dulness is found over the lower half of the left lung; crepitation distinctly heard over the same space; pulse much accelerated, of good strength; skin hot; face much flushed; bowels opened by laxative medicine. Was ordered the antimonial solution every four hours, and the Tinct. Ferri. Sesq. in doses of fifteen drops every two hours.

*August 11th.*—Somewhat easier, but pain still present, and cough troublesome. Continue the medicines.

*August 12th.*—Expectoration easier; feels much relieved; pulse reduced in frequency.

*August 13th.*—Continues improving.

*August 14th.*—Able to be out of bed.

*August 16th.*—Walked out of doors a considerable way, and in a few days more resumed his work.

In conclusion, I may remark, that in neither of the cases of pneumonia above related was there any evidence of inflammation being present in the upper lobe of the inflamed lung, nor any of the typhoid symptoms usually accompanying this form of the disease.

CHAPEL-EN-LE-FRITH, Sept. 2, 1852.

ARTICLE VII.—*Case of Combustion and Death of the Human Body in the Open Air—Spontaneous or not?* By JOHN GRIGOR, M.D., Nairn.

ON the evening of the 29th July last, the body of John Anderson, æt. 50, about five feet four inches in height, and of a spare habit, a carter of wood from the forest of Darnaway to the pier of Nairn, and a notorious dram-drinker, was found dead by the road-side, seven miles from Nairn, and in a state of combustion, the process

having proceeded so far as blackening and charring of the body and head, and complete disfiguration of the features, so much so that the person was only recognised from his horses and carts being known. The case was taken up medico-legally by the Procurator-Fiscal of the county of Nairn, and I was requested to inspect the body, and report. On approaching the unfortunate man's dwelling on the forenoon of 31st July, I found that the funeral had passed on to the churchyard of the parish of Dyke, and after a little explanation to the attendants, I succeeded in getting a hurried autopsy within the church. On removing the grave-sheet, I found a black, incinerated, and stiffened body. The legs and arms were crossed; the latter raised from the chest. The position was one of ease, and the body had not been touched since first rolled up. The eyes, ears, and nose, were burned away; teeth clenched; and from the mouth bubbled out some white froth and gas. The lining membrane on the inside of the lips and cheeks was quite burned; also the edges of the tongue, and the hair and skin of the head. The skin and cellular tissue of the body were much charred, the thighs not to the same extent, and the burning had ceased about midway between the knees and feet, where there was a reddish and slightly blistered line. The back was not so much destroyed. The pharynx, œsophagus, etc., exhibited no appearance of burning. The villous coat throughout was much congested, and that of the stomach presented those cherry-red appearances, with thickening, which are sometimes noticed in the stomachs of drunkards. It was almost empty, gave out no smell of alcohol, nor did the contents on after-examination. On opening the peritoneum, there was a great escape of fetid gas. The bowels were healthy, but dry from heat. The state of the heart, blood, and lungs, could not be examined.

On inquiry, I found the wretched man's history to be the following:—He has been a carter, as above-stated, for several years; has drunk, at least, of ardent spirits *daily*, on an average, a common bottleful, besides porter, beer, etc.; left Nairn, on the day of his death, intoxicated; in passing an intermediate village, was seen coming on "all fours" out of one of those many "publics" which are the opprobria of our smaller towns and villages in the north of Scotland. He was, however, one of those "soaking" individuals, who much sooner lose the locomotive balance than a knowledge of his situation and work; hence, when on his cart, he could talk and manage his horses tolerably well. He had a brother carter with him, a neighbouring toll-keeper, who was sober; and they parted company at the toll-gate of Harmuir, within half a mile of the place where the body was found. Before this, however, Anderson wished his pipe to be lit and handed to him; but his friend, thinking that he had no need of a smoke, merely put a little fire on the old tobacco ash, when he drew, and immediately said, "She is not

in." The conversation went on for ten minutes, when the poor man turned his horses' heads homewards. All this time the pipe was in his hand. The tollman, who was much on the road with him, declared that Anderson seldom lighted his own pipe, and never almost knew him to carry lucifers. The dress was a woollen shirt, canvas frock, corduroy trousers, and "a wide-awake." The weather was very warm and dry. When a little farther on his way homewards, smoke was seen rising up from the cart in which the man was, and which contained a good deal of hay, by a herd-boy on a neighbouring rising ground, about one-fourth of a mile distant. The man was next seen to descend from the cart, to stand, then to stagger and fall. The horses stood still. In a few minutes, smoke again appeared from the ground, when the boy ran down, and found the body lifeless, black, disfigured, and burning. He hurried to a cottage close by, and returned with a woman having a water-pail, with which they drew water several times from a rivulet almost at their feet, and thereby extinguished the burning body and garments. The position was on the back, inclining to one side; arms and legs as before-mentioned. The time that elapsed between the boy seeing the man come down from his cart and the water being dashed on, is represented as not more than fifteen minutes. The body was wrapped into a sheet, and removed home. The pipe was found lying below the body with the cap on, apparently as it had been put into his hands. The clothes were all consumed, except the lower parts of the legs of the trousers, where the burning had ceased, and a small portion of the shirt, frock, and hat, immediately between the body and the ground. There was none of the hay burned.

*Remarks.*—The case at first sight appeared to me to have arisen from the clothes having by some means caught fire, and the smoke therefrom producing death by asphyxia—the subject being much intoxicated; but second thoughts demonstrated a few points not reconcileable to my mind with this view, such as the position on the back, etc.—the event taking place in the open air—rigidity of the limbs—no trace of fire—and the rapidity and extent of the combustion, whilst this latter (compared with the accounts of martyrs, suttees, and others who have been consumed, and the great quantity of fuel and the time that have been required) and no apparent struggle or attempt having been made to cast off the burning garments, or to quench the flames in the brook running alongside, whilst the man was not at all in a state of insensibility from his potations, led me to the belief, that it was no ordinary combustion from the application of fire. I have then been induced to regard it as a case of progressive igneous decomposition, commencing during life without the application or approach of any hot or burning body, as believed in by several continental physiologists of eminence. Such a state of matters I know has been regarded by many as almost fabulous; but the numbers of general instances from good authorities, and from

all parts of the world, of spontaneous combustion, or, as Beck more properly terms it, preternatural combustibility of the human body, and written on by Dr Mason Good, and received into the Statistical Nosology from the General Register Office, now in the hands of most medical practitioners under the appellation of *Catacausis Ebriosa*, show that the doctrine cannot be wholly set aside. It is not my intention to lengthen these observations by quoting cases, the popular reports of the nature and causes of many of which I will allow are valueless from misrepresentation, and "biased by alarm, credulity, stupidity, and a love of the marvellous;" yet there are many British, continental, and American authors, whose favourable testimonies are very valuable. Nor will I inquire into the chemical possibilities of the generation in the body of inflammable gases, such as hydrogen, phosphuretted hydrogen, etc., nor into the existence of oil in the serum of the blood of the habitually intemperate. And, as regards the alcoholic theory, in the sense of the popular advocate for temperance, I will take leave to quote, from an article in the last July Number of the "Edinburgh Medical and Surgical Journal," which embodies the opinions of MM. Liebig and Bischoff on this part of the subject, etc., as evidenced on the interesting medico-legal investigation, in reference to the murder of the Countess Goerlitz, at Darmstadt, in June 1847.

"To suppose, therefore, that any amount of alcohol, or alcoholic liquor, could cause either the breath exhaled to take fire on the animal tissues being impregnated with it, is to admit an amount of accumulation of this poison in the system totally incompatible with the continuance of life. M. Bischoff states that he is convinced that, though a dead body were soaked in spirits, it would not, in consequence of that, become combustible. He took parts of a dog, into the arteries of which alcohol had been injected at 92°. These parts did not burn, either at the flame or exposed upon coals. They became roasted and charred, but ceased to burn so soon as they were withdrawn from the fire.

"It must not be denied, nevertheless, that this experiment furnishes no satisfactory conclusion against the hypothesis of spontaneous combustion, such as is supposed to be induced by habits of incessant drinking. It is not impossible that, under the long continuance of these habits, the whole organism may be modified in such a manner as to become, upon the application of certain causes, combustible. But in this case it is at variance with everything known, to imagine that the alcohol in the body is the agent or means of the combustion. It cannot be the alcohol that burns. It must be an animal body converted into a combustible body."

I must here express great regret that it did not occur to me, till it was too late, to apply a light to the body at the time of examination, which would have been almost decisive of this curious medico-legal point.

NAIRN, October 5th, 1852.

[Dr Grigor's case is interesting in a medico-legal point of view, though all the circumstances seem reconcileable with the ordinary phenomena of combustion. We have fire and combustibles about the person of a man helplessly intoxicated,—and can account for what followed, without resorting to the apocryphal theory of spontaneous combustion.—EDITOR.]

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## Part Second.

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### REVIEWS.

*Observations on the Treatment of Lateral Curvature of the Spine.*  
By EDWARD F. LONSDALE, F.R.C.S., &c. London: Churchill.  
1852.

IN all the late requirements of the College of Surgeons of England for raising the character of their Fellows, we are surprised that, in addition to mathematics, algebra, and knowledge of modern languages, they should not also have insisted upon at least a moderate proficiency in English composition. Should any doubts be entertained as to the necessity of this, we think they would be dispelled by reading a very little of Mr Lonsdale's book,—indeed even no further than his Preface.

Proceeding, we confess, with no great expectation of any substantial change in ringing out the tiresome old chime on spinal curvature, so familiar to the ears, not only of medical men, but also of the lady superintendents of private seminaries, the mothers of families, and nursery governesses, we were agreeably disappointed to find a spick-and-span new theory proposed by the author. This is, that a powerful cause of displacement lies in the "pressure of the lungs upon the ribs," and that hence may be explained the frequency or almost constancy of the chest being rendered convex on the right side—of which the lung is larger than that of the left. This is no hasty or passing idea, but a favourite hobby, which is openly, we may almost say ostentatiously, mounted at the outset, and complacently ambled to the conclusion. Upon you, then, Mr Bishop,—upon you, gentlemen of the court of examiners,—and upon you, the other members of the council, we call for attention to this heresy, so revolting to established physiological principles.

It is now just thirty years since Dr Carson, of Liverpool, and Dr Williams fought their great battle, to determine whether the lungs

expanded *proprio motu*—like an “omelette soufflée,” as the latter maintained—or, on the contrary, always exerted a strong tendency to contract, which was prevented from effecting their collapse only by the resistance of the parietes of the chest, within the close cavity of which they are enclosed. Dr Carson was left in such unquestionable possession of the field, that we did not expect ever to meet with a retainer of the enemy. But here we have a Fellow of the College of Surgeons founding his pathological theory upon the ground of the lungs not only possessing a power of dilatation, but one so strong as to press the ribs this way or that, according to their relative size in the two sides of the chest. In these circumstances we venture to suggest the following experiment for Mr Lonsdale's performance. Let him take a pair of bellows, and line the interior with a piece of leather, taking care that it allows the apertures of the apparatus to communicate freely with the interior, but does not permit any air to get in between it and the external covering. If the handles are now separated, the operator may remark, by putting his finger into the lateral opening, that the leather follows the expanding boards, but does not exert any force in pressing them asunder. It will then be proper to vary the experiment by substituting a sheet of India rubber for the leather, when, in addition to what was before observed, no great difficulty should be experienced in conceiving the truth, which it cost Dr Carson so much trouble to explain, in regard to the contractile tendency of the pulmonary textures. We hope that Mr Lonsdale may thus be led to see, that if the lungs, as he says, are of unequal size and strength, the effect of their force should be exactly the opposite of what he alleges, since the contractile tendency, being greater in the right side of the chest, would render it concave, instead of convex.

The author does not think the muscles have anything to do with the production of lateral curvature:—

“And here I may state, contrary to the generally received opinion, that I do not think the muscles proper to the spine and ribs take any part in the original production of the curvature; for there is no evidence or reason to suppose that the muscles on the one side are weaker than on the other, though they may both be generally so, and less developed than natural.”—P. 22.

But Mr Lonsdale forgets that, although the muscles do not perhaps, strictly speaking, cause curvature, they are a powerful means of preventing it, and therefore, when imperfect in their power, demand great attention to this negative influence. In order to maintain the trunk erect and straight, a certain amount of muscular effort is required; but if the patient be weak, or confined long to the same position, the muscles concerned in producing this effect get tired, and allow the ribs and vertebræ to sink, in obedience to their superincumbent pressure. If Mr Lonsdale takes the body of a dead or drunk man, and tries to make it sit upright in a chair,



although "the muscles on the one side are not weaker than those of the other," he will find the head and shoulders obstinately preponderate in some direction different from the erect posture, and by this experimental inquiry may have his views on the subject still further freed from their present rather misty condition.

Upon such a basis of physiology and pathology, it would be unreasonable to expect any sound or useful instruction of a practical kind, and here the reader will not be disappointed. Mr Lonsdale is a great admirer of the Mr Tamplin, whose performance in the orthopædic department we had lately occasion to notice, and whose principles of treatment seemed to us so much more promising of benefit to the practitioner than the patient. He does not venture to supersede, but modestly endeavours to improve, the apparatus of his colleague, which our readers may recollect, though presented under a different name, is in truth nothing but a modification of that detestable engine, the "steel stays." We have the "foundation pelvic hoop,"—the "crutch," to raise the depressed shoulder,—and the "plate," to "press against the convex side of the thorax." Again, therefore, we beg to remark, that if the depressed shoulder be raised, the pectoral and latissimus dorsi muscles must be put on the stretch so as to increase their contractile tendency, or, in other words, to tighten the string of the bow, and thus increase the evil, instead of alleviating it,—in reality, at least, though not perhaps in appearance at first sight; for the immediate effect of squeezing up the sunk shoulder is to lessen the most characteristic sign of the deformity, so that, when concealed by the clothes, it may, if slight, be hardly perceptible. And hence the confidence placed by patients and their friends in this delusive system, which not only affords no real benefit, but is sure to render the state of matters worse by the effects of confinement and-restraint. It is true that Mr Lonsdale claims the credit of a new feature in the mechanical treatment of spinal curvature, which is, bending the patient's body for an hour or so every day, by placing it on the side, suspended by a sling resting on the convexity. But if he recollected that the bones concerned are altered from their natural form by interstitial absorption, and can be restored to it only by a process of the same kind, he could hardly expect that the spine may thus be rendered "more flexible," as if it were constructed of wood or leather.

But it is needless to pursue the subject further, since there is no chance of our observations confronting this publication in its proper field, which is the table of a boarding-school parlour or the school-room. It is a sad thing for our profession when its doctrines and practice fall into the hands of specialists; and, as there can be no more sure sign of decay in a tree than withering of the top branches, with numerous sprouts from the lower part of its trunk, we confess that what seems to us a not unsimilar condition in regard to our metropolitan brethren, suggests serious apprehensions as to the soundness of their system.

*Facts and Observations on the Physical Education of Children, especially as regards the prevention of Spinal and other Deformities.* By SAMUEL HARE, Fellow of the Royal College of Surgeons, etc. 8vo. London: Churchill. 1852.

THIS book has been sent to us for review; but it does not appear to be intended for the information or instruction of the medical profession. We have failed to discover any very striking "facts" in it, except allusions to sundry cases treated by Mr Hare, none of which are given in detail, and which for the practical enlightening of less experienced curers of spinal deformities, are entirely useless. Of "observations," we have plenty; but they consist almost entirely of the stalest remarks on the management of children, none of which are new to medical men, and few of which, from their vague, unexplicit character, can be of much use to any body out of the profession.

We are told that—

"From the very earliest period, the most scrupulous attention to cleanliness is essential; and, with this object in view, the use of the tepid-bath, or sponging the body with tepid-water, should be had recourse to once or twice daily; by these means the skin is kept free from any impurities, whether arising from without, or from its own secretions, and thus any impediment to the very important function which it should perform is removed."—P. 11.

"In selecting a nursery, care should be taken that it be spacious, lofty, dry, well ventilated, and well lighted."—P. 13.

"The pleasure, or the reverse, with which children will go to the nursery, will depend very materially upon the character of the nurse, whose duties, as regards the special subject of these pages, are really important. She ought to be healthy, active, and somewhat young, and should possess adequate strength to enable her to give the child sufficient exercise, by keeping its body in almost constant action during its waking hours. A decided preference should be given to young women of good temper, and lively disposition, and particularly to such as are fond of children, and in whom there is reason to believe that full confidence can be placed."—P. 15.

We might go on quoting common places of this sort throughout the whole of this book, for the rest of it is quite akin to the first fifteen pages, from which these extracts have been made; but we see no profit to our readers in so doing. What would any medical man learn from such observations as the following on food?

"As regards quality, the first consideration should be its supply—food at once bland, unexciting, light to the stomach, readily digested and assimilated, —yet highly nutritious, and not exposed to the objections to which animal food is liable."—P. 18.

Or, what fond parent would profit much by being first frightened, and then left in the lurch by this?

"Children, too, by eating to excess that which gratifies the palate are especially liable to overload the stomach, so as to produce at least temporary derangement of the health; and in many instances, by these repeated "derange-

ments," the foundation of subsequent, and, too often, permanent disease. Very constantly, indeed, they are allowed thus to eat too much, and take too much liquid; so that, in each way, there is a tendency to have the stomach distended, and its powers consequently weakened. Parents must exercise their own judgments in these matters; and, as no rules can be laid down of universal application, they should, if they feel any doubt, appeal to their medical adviser."—P. 19.

Surely if Mr Hare was going to write a book about how to feed children, he ought to have had some more precise information to put into it than this; or, if he had no more precise information to communicate, he ought to have thought twice before he wrote a book.

We do not purpose to follow him through the rest of his *opusculum*, which extends to seventy-one pages—twenty-one of them upon dress, *i.e.*, upon corsets, and thirteen upon exercise, including gymnastics; the remainder, under the title of "conclusion," being a disquisition upon "recumbency" in the treatment of deformities.

We do not find in all this writing anything that is either sufficiently new or sufficiently precise to be of any practical use to medical readers. Thus, when Mr Hare tells us that "the state of the chest and its contents must also be particularly attended to, and measures taken to increase the breathing capacity of the lungs," he insists upon the value of accurately measuring how much air a patient can expire by means of a bell-jar suspended in water, 'yclept a pulmometer; but he does not give us any more of the results of his experience than to refer us to some of his own successful cases—nothing is said of the peculiar treatment they underwent; and the only "measures to be taken," that we can discover, are those by the pulmometer, which, we believe, to be a device far more imposing in appearance than useful in reality.

When Mr Hare has anything original to communicate, either as to the pathology or treatment,—hygienic or medical,—of spinal deformities, we shall be glad to give him credit for his contributions to our stores of professional knowledge. If he wishes to enlighten, not the profession, but the laity, as to his knowledge of these matters, let him say so manfully, and we shall be happy to express an opinion as to whether his book is a safe guide for their direction or not. We sincerely believe that Mr Hare has paid attention to spinal deformities, and that he may have seen more cases of this nature than some of his brethren; but he does not seem to us to have made any discoveries as to the physical education of children, in relation to these or any other diseases, worthy of being erected into a book. We fear that he has fallen into the mistake of publishing a fresh volume for the sake of mere notoriety; for he has written two other small works on kindred subjects already. We must, however, do him the justice to say, that what he does write is at least grammatical and intelligible, and so he is distinguished from many of the book-makers of

the metropolis, whose horrible productions it has been our duty to expose, as a disgrace to our medical literature; but we wish that, before he sent this volume to the press, he had remembered the sage advice, "When you have nothing to say—say it."

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## Part Third.

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### CLINICAL REPORTS, LECTURES, ETC.

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#### CLINICAL MEDICINE.—PROFESSOR BENNETT.

##### PATHOLOGY OF RHEUMATISM AND GOUT.

THE present theory with regard to these affections is, that they are both connected with an increase of lithic acid in the blood. In rheumatism, this is dependent on excess of the secondary, and in gout on excess of the primary, digestion. In rheumatism, however, there is considerable excretion of lactic acid by the skin (Todd), whilst in gout there is an excess of soda, which, uniting with the lithic acid, produces a compound of lithate of soda, that may be detected as such in the blood (Garrod), while sometimes it exudes into the cellular tissue of the skin, constituting tophaceous deposits. In both diseases there is an undue balance between the excess of lithic acid and the power of excretion,—in rheumatism by the skin, and in gout by the kidney. This pathology serves to explain the similitudes and differences existing between the two affections. In both there is a certain constitutional state, dependent on deranged digestion, during which exciting causes occasion local effects. These exciting causes in rheumatism are bad diet, hard work, exposure to cold and wet, and its subjects generally are the poor and labouring population. In gout the causes are good diet, indolence, repletion, or indigestion, and its subjects are for the most part the rich and sedentary. The local manifestations in both are acute wandering pains, with pain and swelling—in rheumatism of the large, and in gout of the small joints, constituting the acute attack in the one, and the so-called regular attack in the other. These are combined with a tendency to various complications of the internal viscera, which are more or less dangerous to life.

The general indications of treatment are, in both diseases, (1st) so to regulate the nutritive functions as to ensure a due balance between the amount of matters entering the blood as the result of digestion, primary or secondary, and the amount of matters discharged from the economy by the excretory organs. (2d) To conduct the acute attack to a favourable termination, carefully watching the internal viscera, and being prepared to act with vigour should these become affected. Hence the treatment of these diseases resolves itself into what may be called curative and preventive,—the first having reference to the acute attack, the second to the means most likely to hinder its return; the one must be carried out by remedies which act upon the blood and excretory organs, the other by the management of diet and exercise.

## TREATMENT OF ACUTE RHEUMATISM BY NITRATE OF POTASH.

Although the general pathology above-mentioned, which considers rheumatism as a blood disease, may be considered on the whole as correct, we are not yet enabled to explain by it the symptoms of an acute attack of the disease, where, in addition to the constitutional disorder, we have local pain, occasional heat, redness and swelling, with febrile symptoms. Most practical men have attributed these phenomena to a superinduced inflammation, although it has not been shown that exudation occurs, or that it is followed by the usual results of that condition. Besides, its erratic character is opposed to what we know of the process of true inflammation, and calling it an unhealthy inflammation in no way clears up the mystery. The real pathology of acute rheumatism, therefore, has yet to be determined, and, as a preliminary step, a careful histological examination of the affected tissues is absolutely necessary. So far as I am aware, this has never yet been attempted, if we except some observations by Hasse on the structure of the bones in rheumatism (see the *Monthly Journal* for June 1847).

Our treatment of this disease, therefore, is purely empirical, sometimes directed against the pain, at others against the supposed inflammation; now attempting to combat the pathological condition of the blood, then striving to remedy its effects by acting on the excretions, and not unfrequently giving specifics, in the hope that any change in the constitution, however produced, may be beneficial. In no disorder, probably, has such a crowd of opposite remedies and plans of treatment been extolled, and yet none of them can be depended on, so that it has been imagined that six weeks' rest is the most useful prescription (Warren). The latest author on rheumatism endeavours to explain this by observing, that this need not be wondered at by "those who consider the true nature of the disorder, and the variety of circumstances under which the physician may be called upon to minister to his patient's relief. The bleeding, which in the young, plethoric, and robust, may be necessary to allay excessive vascular action and cause free secretion, may in the weakly induce irritability of the heart, and a consequent attack of cardiac inflammation. The opium which in one person may prove of the greatest service in promoting free perspiration, and in allaying the general irritability of the system, may in another check the biliary and other secretions, and thus prevent the elimination of the rheumatic poison. The continued use of calomel, and the constant purging, which may be beneficial to one patient by removing large quantities of unhealthy secretions, may unnecessarily exhaust the strength of another, and tend very greatly to impede recovery. And so in regard to every remedy which has been proposed. What is useful at one time proves useless, or positively injurious, at another; and the conclusion is forced upon us, that what is wanted 'is far less the discovery of untried methods of treating disease than of discriminative canons for the proper use of those we possess;—far less the discovery of any new medicines, than the adaptation of our present remedies to the exigencies of each case.'" (Fuller on Rheumatism, p. 73). These judicious observations may serve to explain the cause of our failure; but until we obtain more exact information regarding the *special* pathology of rheumatism, it is in vain to hope for a rational treatment.

For my own part, I generally treat rheumatism on what is called "general principles;" these are, to alleviate severe pain by anodynes, diminish excessive vascular action by moderate bleeding and saline antiphlogistics, and encourage every attempt at critical discharges by diaphoretics, diuretics, purgatives, etc. Occasionally I have tried the effects of special remedies in this disease, and watched a series of cases, all of which were treated in the same manner. Thus I have tried aconite, and believe that alone it is of little service; colchicum also I have given frequently, and am of opinion that in pure



rheumatism it is of no advantage, although in gout it is invaluable. This session (1851-2) you have witnessed another trial of this kind with the nitrate of potash, a remedy formerly recommended by Dr Brocklesby, and which has been given with good effect by M. Gendrin, in the wards of La Pitié, in Paris, as recorded by Dr Henry Bennet (*Lancet*, 1844, vol. i. p. 374). It has more lately been pressed on our attention by Dr Basham (*Medico-Chir. Trans.*, vol. xxxii.), who tells us that from one to three ounces of the salt, if freely diluted in water, may be taken by the patient in the course of twenty-four hours, without any injurious results, but with the effect of relieving in a marked manner the swelling, heat, and pain in the joints. In the following cases the remedy was tried in much smaller doses, and it appears to me with more than average success :—

CASE I.—Mary M'Kenzie, æt. 46, servant, admitted January 4th, 1852. States that she was suddenly seized with a severe pain in the left side two days ago, after undergoing unusual fatigue, and this was followed the same night by rigors and other febrile symptoms, which have since continued without intermission. The pulse is 84, natural; heart's sounds normal; complains of headache and loss of sleep, and especially of a severe cutting pain in the left axillary region, increased by pressure and motion. Careful examination of the pulmonary organs elicited nothing abnormal. Tongue is coated; there is some thirst, but the appetite is good; bowels are costive; the skin is hot, and she sweats at night. *Ordered Potas. Nitratis*, ʒss.; *Aq.*, ʒvj. *M. A tablespoonful every four hours. Hirudines*, xv. January 6th.—No improvement to-day; has been taking the medicine regularly. *Ordered v.s. ad ʒviij.* January 7th.—Pain not so severe to-day, but is still present. January 11th.—Continues much better; pain is only felt on coughing. Convalescence proceeded somewhat slowly after this date, and she was dismissed cured, 2d February 1852.

*Commentary.*—This was a case of severe rheumatism affecting the pectoral and intercostal muscles on the left side, which was subdued on the ninth day, after six days of treatment. The nitrate of potash produced apparently little relief, although fifteen leeches had been applied to the painful part, until a small bleeding of eight ounces was practised, when marked amendment immediately occurred.

CASE II.—Mrs Anderson, æt. 48, sick nurse, admitted December 3d, 1851. States that previous to the present attack she has always enjoyed pretty good health, with the exception of a liability to a slight cough; has been lately subjected to much fatigue in her occupation as a sick nurse, and has been exposed to cold from sitting up for several nights in succession in a large room, heated by a fire, and ventilated by keeping the windows open. Having no adequate protection from the cold draught thus caused, she became affected with sore throat, and had pain in the chest. This occurred in the latter part of October last, and from that time up to November 20th she has suffered from slight shivering and uneasiness; transient pain in different parts of the body; nausea and vomiting. About a fortnight before admission, she had a distinct rigor, followed by heat of skin and other febrile symptoms, with very severe pain in the joints especially, much increased by any attempt at motion. The vomiting also continued; and last week she suffered from pain and palpitation in the cardiac region, and at the same time an aggravation of her former symptoms, so that at present she cannot move without suffering excruciating agony, having severe pain apparently in every joint of the body. Heart's sounds, impulse, rhythm, and position normal; pulse about 100, weak. Irregular fits of copious clammy perspiration, of acid smell; no œdema of the joints. Urine scanty, dark-coloured, deposits crystals of the triple phosphates, with some mucus. Tongue loaded; anorexia; thirst; occasional vomiting; no tenderness on pressing the epigastrium; bowels confined; pulmonary functions normal. *R. Murialis Morph. semigranum; Pulveris Aromaticæ grana quinque. M. Ft. Pul.*



*Mittantur sex. One to be taken every half hour.* December 4th.—She took three of the powders last night, after which she fell asleep; and this morning feels somewhat better; she has also had the bowels emptied by an enema, and is now using a diuretic mixture. December 5th.—Pains in limbs much the same; gets no sleep; perspiration still copious; urine not increased in quantity; vomiting continues; has been taking diuretics and Dover's powder. December 6th.—Had an exacerbation last night, the pain in the joints and limbs being excruciating. *R. Potassæ Nitratis semiunciam; Aquæ uncias sex. Misce et signetur—a tablespoonful every four hours.* December 7th.—Has taken three doses of the medicine; she perspired a good deal during the night; urine not increased in quantity; pain is less severe. December 8th.—Still sweats a good deal; pains much the same as yesterday. *Adde Misturæ Nitratis Potas., 3j.* December 9th.—Pains better; copious perspiration; urine increased in quantity; increase of the nausea and vomiting and of the thirst. December 10th.—Pains nearly gone; sickness continues; refuses to use her medicine; pulse 80, weak; much general debility. After this date the pains ceased entirely, and she was shortly afterwards discharged cured.

*Commentary.*—This was a severe case of both general muscular and articular rheumatism, of a fortnight's standing, when she entered the house. There was still, however, great pain on the slightest movement, which, during two days, in no way yielded to morphia, diaphoretics, and diuretics. On the exhibition of the nitrate of potash, profuse diaphoresis came on, which was apparently kept up by the medicine, with marked amendment to the rheumatic pains, followed by rapid recovery. The improvement could not be attributed to the occurrence of any critical day in this case; and the night previous to the exhibition of the remedy, there had been a marked exacerbation. Every one who saw this case felt persuaded that the good effects were attributable to the nitrate of potash.

CASE III.—Janet Wright. This woman had been admitted early in December, labouring under the usual symptoms of acute rheumatism, and has been undergoing treatment by Dover's powder, diuretics, leeching, etc., up to the 6th, without any benefit whatever, when on that day she was ordered *R. Potas. Nitratis, 3iij.; Aq., 3vj. Misce. A tablespoonful every three hours.* December 7th.—Has taken four doses of the medicine, but without effect for so far; took a dose of Dover's powder last night, and slept well; pain in the shoulders very severe, and also in the knees. December 8th.—Pains still continue. *Adde Misturæ Potas. Nitratis, 3j.* December 10th.—Has been using the medicine regularly; she says it makes her very weak, sleepy, and stupid. She sweats a good deal at night, and the urine is increased in quantity; is very thirsty, and complains of bad taste in her mouth; pains gone from knee. December 13th.—Still continues the medicine. No return of pain in the knee; greatly relieved in shoulders, etc.; the increased secretion from the skin and kidneys continues. The improvement continued up to the 16th, when she was dismissed for disorderly conduct.

*Commentary.*—In this case the nitrate of potash, after being taken for three days, caused much diaphoresis and diuresis, followed by diminution in the rheumatic pains and rapid improvement, when she was dismissed.

CASE IV.—Jane Irvine, æt. 17, servant, admitted 19th December 1851. States that seven days ago, whilst engaged at her usual occupation, she was suddenly seized with severe febrile symptoms, and constant pain in the left ankle, which was increased by pressure and motion; it was red and tumefied. On the following day the right ankle became similarly affected, and then in succession the knees, shoulders, wrists, and fingers; the pain still continuing, but modified in severity in the parts first attacked. She has been undergoing treatment by diaphoretics, without, however, having experienced any relief from them. On admission the pulse is 100, full and soft. A soft

bellows murmur, synchronous with the radial pulse, accompanies the first sound, heard loudest at the base, and is propagated along the course of the large arteries. Cannot sleep from the pain, which is general, and is causing intense suffering. Tongue moist, preternaturally red at the tip and margin; no appetite; thirst; nausea, and vomiting; the bowels are costive; some tenderness on pressure in the epigastrium. Urine high coloured, deposits a slight sediment of lithates. Skin moist, from copious perspiration; knees and ankles are swollen and painful on the least pressure. The right wrist, especially near the metacarpal bone of the thumb, is at present the seat of greatest suffering, and is red, painful, and swollen. *Ordered to be bled to 3xvj., and to have a purgative enema.* December 20th.—Is much worse to-day; the pains in the wrist and hands are especially aggravated. Copious perspiration still continues. *Ordered R. Potas. Nitratis, 3ss.; Aq., 3vj. A tablespoonful every four hours.* December 21st.—Slept during the night. The sweating is still profuse. Urine in moderate quantity, sp. gr. 1016, deposits lithates. Pulse 90, weak; cardiac murmur very indistinct. The pain is considerably relieved, except in the left lower extremity. December 22d.—Still continues taking the Potass. Nit.; the improvement more marked, and she can allow the limbs to be moved about to-day. December 23d.—She presents quite a cheerful appearance to-day, and is entirely relieved from pain; all the joints can be moved quite freely, without exciting uneasiness. Pulse 68; skin cool; tongue clean; appetite returning; bowels regular; urine natural—some sediment. Cardiac murmur is more distinct to-day. Convalescence proceeded satisfactorily from this date till January 5th, when she was attacked by typhus fever, from which, however, she ultimately recovered, and was dismissed well.

*Commentary.*—This was also a very severe case of general rheumatism, which was in no degree benefited by diaphoretics, and a large bleeding on the seventh day. On the eighth day she was if anything worse, and then nitrate of potash was given, producing marked relief on the following day. On the eleventh day of the disease, and third from the exhibition of the salt, the disease was subdued, and she became convalescent. Here, again, the period of improvement cannot be confounded with critical days, and strictly corresponds to the administration of the remedy. The bleeding *may* have assisted its effects, but certainly was not followed, as is usually the case, by any evident amelioration. This girl had an endocardial murmur on admission, which continued during the progress of the case, and I ascertained from the medical practitioner who sent her into the house that she had laboured under this before the attack of rheumatism came on. Was this, therefore, an anemic murmur independent of the general disease, or produced by it? We may ask another question, viz., Are all the endocardial murmurs occurring in conjunction with rheumatism caused by endocarditis, and attributable to the rheumatic diathesis? These questions demand more careful attention to these murmurs in young women than has, I think, hitherto been paid to them. For my own part, I am satisfied that these anemic murmurs in young girls are very common; and as servant maids, as a class, are commonly affected with rheumatism, I cannot help suspecting that they are more frequently considered to labour under endocarditis than is really the case. Further observation, however, is required to determine this point.

CASE V.—Margaret Bell, æt. 23, servant, admitted 29th December 1851. States that last Wednesday she was engaged in washing before an open window; some time after which she was suddenly attacked with rigors, hot skin, and perspiration; and on the same night severe pain commenced in the left hip, and extended subsequently to the knee and ankle of the same limb. Shortly after the right limb became similarly affected, though the pain did leave the left. The suffering became so intense as to prohibit the slightest motion; and it became exacerbated at night, and at the same time copious perspiration broke out. At present the lower extremities are affected the most severely; and the joints are slightly red, and tumefied. The heart's sounds are

normal. *R. Potas. Nitratis*, 3vj. ; *Aq.*, 3vj. *Solve. A tablespoonful every four hours.* December 30th.—Has taken six doses of the medicine, but without any improvement as yet in the pains; pulse 128. January 1st, 1852.—Pulse 120; skin cooler; slept well during the night; urine deposits a slight sediment; bowels have been opened. There is now no pain anywhere, except on motion and pressure of the ankle. In the evening, however, after rashly exposing herself to cold, the pain returned to the right elbow. January 2d.—Pain very severe in the right arm and hand; skin hot; urine loaded with sediment; no sleep. *Ordered to take Potass. Nit. 3ss., every four hours, dissolved in half a tumblerful of water.* January 3d.—Pains have again disappeared from all the joints, with the exception of the wrist, which is painful on motion. On the 5th of January, there was again an exacerbation, and she was bled to 3xvj, after which the pains ceased entirely for the time. She did not leave the hospital, however, till the 25th, as she was liable to occasional slight returns of the disease; which appeared to yield readily enough to further doses of the Potas. Nit.

*Commentary.*—In this case the nitrate of potash did not produce so marked and permanent an effect as in the last one, although the dose was considerably increased; and yet it apparently was of much benefit. There was a great tendency to exacerbations, and the rheumatism was of an erratic character. After continuing the exhibition of the salt for six days, I ordered a bleeding on the occurrence of another exacerbation, which produced a marked amendment. She continued the nitrate of potash; but the disease lingered in a trifling degree, and continued to fly from joint to joint, until a few days before her dismissal.

CASE VI.—James Rough, æt. 26, blacksmith, admitted December 29th. States that he has suffered on two former occasions from attacks of rheumatism. During his last attack, three years ago, he was treated in this hospital, and it lasted five weeks. The present attack came on nine days ago with great severity, having been preceded by febrile symptoms, which appeared to have followed exposure to cold; the pain was very severe in all the joints, but especially so in the wrists and knees. He has noticed within the last year or two that considerable palpitation of the heart ensues after much exertion, or indulgence in ardent spirits; but in his ordinary conditions he is not troubled with it. At present the pain in the joints is not severe, unless on attempting motion; pressure on the right shoulder and ankle causes considerable tenderness. The cardiac dulness measures a few lines more than two inches across, but strikes the thoracic parietes in the normal position. A very distinct bellows murmur accompanies the first sound, is heard loudest at the apex, and is not prolonged along the course of the great vessels; the second sound is more sharp and abrupt than natural. The radial pulse is not synchronous with the impulse of the heart, but follows it after a very appreciable interval. A few sibilant râles can be heard here and there over the chest. Tongue is slightly furred; appetite is impaired; thirst not excessive. There is slight diarrhoea. The urine is normal. Skin is moist, but no excessive perspiration. *R. Potas. Nitratis*, 3ss. ; *Aq.*, 3vj. *M. A tablespoonful to be taken, diluted with much water, three times a-day.* December 31st.—Pains much easier to-day. The bellows murmur is much softer also. Urine deposits some lithates. Is sweating a little to-day. Pulse 86, soft, and regular. January 2d, 1852.—(Thirteenth day.) Has no pain to-day. Continues to perspire a good deal; and the urine deposits a copious precipitate of the lithate of ammonia. Pulse 68, soft, and regular. Complains much of weakness. After this date, the amendment continued uninterruptedly, although only one bottle of the Nit. of Potash mixture had been used, and he was dismissed cured on the 12th January.

*Commentary.*—The exhibition of the nitrate of potash was followed by apparently marked effects in this case, producing diaphoresis and evident benefit on the twelfth day, and removal of pain on the thirteenth day of the disease. As

the attack commenced nine days before admission, we cannot suppose that the recovery was owing to the occurrence of a critical day. Besides, the good effects were apparent the day after the exhibition of the salt, and on the following day the pains had disappeared. The valvular murmur with the first sound at the apex, and the character of the pulse, could leave little doubt as to the mitral incompetency; and, as he had been previously subject to rheumatism, there is every probability that the cardiac lesion was the result of previous attacks of the disease.

## GOUT.

James M'Pherson, æt. 42, a seaman, admitted into the clinical ward, May 20, 1852. He states that his first gouty attack occurred about 15 years ago. He had not been exposed to any great changes of temperature, and neither of his parents, nor any of his relations, have had a similar disease. He has not been a particularly temperate man, but has never indulged much in malt liquors. He has enjoyed uniform good health, with the exception of these gouty attacks. He has had since the first one about seven others, which have rather increased in severity. They come on without any premonitory symptoms. The *first* began with pain in the dorsum of the right foot, confined to one spot, which was red and swollen. The symptoms slowly shifted to the ankles, first the right, then the left being affected. The attack confined him to bed for three weeks, when the symptoms disappeared, leaving no trace of their presence. He was free from the disease for two years. It then re-appeared in the toes and ankles, similar to the former attack, but more severe, and longer in duration. The next was after an interval of eighteen months; it began in the toes, and extended to the knees, elbows, wrists, and hands. Nodosities on the hands commenced about five years ago; and he has had about seven attacks altogether since. Every recurrence of the disease since then has increased the previously existing nodosities, and added new ones, which remain during the intervals of the acute attacks. Last year a swelling over the meta-carpal bone of the index finger of right hand burst, and discharged during four weeks a white chalky deposit of the urate of soda.

At present the wound before-mentioned has healed, but two others are discharging, one on the meta-carpal bone of the little finger of the *right* hand, the other over the second joint of the ring finger on the *left* hand, and a third on the meta-tarsal joint of the left great toe. The hands are covered with nodosities, the effects of previous attacks. An inflammatory attack of a joint is always followed by great œdema, then by desquamation of the cuticle and chronic enlargement. He had an attack eighteen months ago; he recovered in about three months, and continued well and able to pursue his occupation till about a fortnight ago, when the first joint of the left great toe became red, swollen, and painful, and in about four days burst, and is now discharging. A box fell upon the part about three weeks before, but the pain caused by that accident subsided before the gouty attack. He takes food well, has never been subject to dyspepsia, but sleeps ill at night. *R. Sol. Morph., gtt. xx.; Tinct. Colchici, 3ss.; Aq. Menthae, 3j.; Ft. Haustus. To be taken at night.*

*Progress of Case.*—Up to June 6th he took the draught above prescribed regularly every night, and had no complaint whatever, the ulcers healing, and the discharge abating naturally. On the 5th of June, however, pain, redness, and slight swelling of the left wrist-joint appeared, as if another attack was about to ensue. A bag containing ice and salt was applied to the part, and kept on for half an hour. The pain began soon to abate, and did not return. No other symptoms followed.

With the exception of slight *costiveness*, he continued well till the 20th of June, when he had similar premonitory symptoms, occurring in the right knee and ankle. Leeches were first applied, but afforded no relief. Ice was again used, and great decrease of pain followed. The knee continued weak for some

time, but gradually regained strength. On the 27th June he was ordered colchicum in powder (5 grs. morning and evening).

Up to the 25th July the patient was quite free from pain; but on that day it returned in the ankles and *left* shoulder, when a mustard blister was applied to the ankles, but without relief. Ice was applied (on the following day), and on the 31st July he was ordered 5 grs. of colchicum three times a-day, the pain continuing, though less in intensity. From this time he experienced no further pains, and the nodosities had nearly disappeared on the 30th of August, when he was dismissed.

*Commentary.*—This case is remarkable on many accounts. In the first place, as being a patient of the Royal Infirmary, where cases of gout are so extremely rare, that this is the first I ever met there, while my colleagues, with their longer experience, have not had more than two or three in the clinical wards. Secondly, in the absence of all hereditary tendency as well as of those other causes supposed to induce the disease. Thirdly, in the large nodosities under such circumstances, and the good effects of local applications of ice, in arresting the advance of gouty pains and swellings in other joints. The immunity of the Scotch from gout is popularly attributed to their frugal habits, and to the use of whisky instead of wines or malt liquors. Certain it is that the disease is scarcely ever met with in hospital practice.

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## Part Fourth.

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### PERISCOPE.

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#### MEDICAL JURISPRUDENCE.

CASE OF HERMAPHRODISM, INVOLVING THE OPERATION OF CASTRATION, AND ILLUSTRATING A NEW PRINCIPLE IN JURIDICAL MEDICINE. BY S. D. GROSS, M.D.

The following case, which came under my observation in 1849, will, if I mistake not, prove both novel and interesting to my professional brethren. So far as my information extends, there is no account of any operation for a similar object upon record.

The subject of the case, at the time I first saw her, was three years of age, having been born on the 10th of July 1846. She had always, up to this period, been regarded as a girl, and had been so pronounced at her birth by the accoucheur. At the age of two, however, she began to evince the tastes, dispositions, and feelings of the other sex; she rejected dolls and similar articles of amusement, and became fond of boyish sports. She was well-grown, perfectly healthy, and quite fleshy. Her hair was dark, and long, the eyes black, and the whole expression most agreeable. A careful examination of the external genitals disclosed the following circumstances:—There was neither a penis nor a vagina; but, instead of the former, there was a small clitoris, and, instead of the latter, a superficial depression, or *cul-de-sac*, covered with mucous membrane, and devoid of everything like an aperture, or inlet. The urethra occupied the usual situation, and appeared to be entirely natural; the nymphæ were remarkably diminutive; but the labia were well developed, and contained each a well-formed testis, quite as large and consistent as this organ generally



is at the same age in boys. Her hips and chest, thighs and superior extremities, were perfect.

It being apparent, from the facts of the case, that it was one of malformation of the genital organs usually denominated hermaphroditism, the question occurred whether anything could or ought to be done to deprive the poor child of that portion of the genital apparatus which, if permitted to remain until the age of puberty, would be sure to be followed by sexual desire, and which might thus conduce to the establishment of a matrimonial connection. Such an alliance, it was evident, could eventuate only in chagrin and disappointment, if not in disgrace, ruin of character, or even loss of life. Certainly, impregnation could never occur, and even copulation could not be performed, except in the most imperfect manner.

I need not say that I gave the subject all the consideration and reflection that I was capable of bestowing upon it. I was deeply sensible of the responsibility of my position. A new question, involving the rights and happiness of my little patient, and the dearest interests of her parents, was presented to me. I examined the case in all its bearings and relations—moral, physiological, and juridical; I appealed to records of my profession for a precedent, and I sought the counsel of medical friends. The parents were anxious for an operation; they were intelligent, kind, and tender-hearted, and were willing to sacrifice everything for the welfare of their child. Their only object was to save it from future suffering and misfortune. My own mind was made up; but, before I proceeded to take any further steps, I determined to consult my excellent friend and colleague, Professor Miller, in whose judgment and integrity every one who knows him has the utmost confidence. He saw the child and examined her. He viewed the case, as I had previously, in every possible aspect, and his conclusion was, that excision of the testes was not only justifiable but eminently proper under the circumstances; that it would be an act of kindness and of humanity to the poor child, standing as she did towards society in the relation, not of a boy or a girl, but of a neuter, to deprive her of an appendage of so useless a nature; one which might, if allowed to proceed in its development, ultimately lead to the ruin of her character and peace of mind.

Backed by such authority, I no longer hesitated what course to pursue. I performed the operation of castration on the 20th of July 1849, aided by my pupils, Dr D. D. Thomson, of this city, Dr Greenburg R. Henry, of Burlington, Iowa, and Dr William H. Cobb, formerly of Louisville, now of Cincinnati. The little patient being put under the influence of chloroform, I made a perpendicular incision, about two inches in length, into each labium down to the testis, which was then carefully separated from the surrounding structures, and detached by dividing the lower part of the spermatic cord. The arteries of the cord being secured with ligatures, the edges of the wound were brought together with twisted sutures, and the child put to bed. Hardly any blood was lost during the operation. About two hours after, the left labium became greatly distended and discoloured; and, upon removing the sutures, the source of the mischief was found to be a small artery, which was immediately drawn out and tied. No unpleasant symptom of any kind ensued after this, and in a week the little patient was able to be up, being quite well and happy.

The testes were carefully examined after removal, and were found to be perfectly formed in every respect. The spermatic cords were natural.

I have seen this child repeatedly since the operation, as her parents live only a few squares from my office, and have carefully watched her mental and physical development. Her disposition and habits have materially changed, and are now those of a girl; she takes great delight in sewing and housework, and she no longer indulges in riding sticks and other boyish exercises. Her person is well developed, and her mind uncommonly active for a child of her years.



I would fain present this example as a precedent in similar cases. The reasons which induced me to recommend and perform this operation in the instance before me have been already mentioned, and now, after a lapse of three years, I have no cause to regret the undertaking, or to think that I acted harshly and inconsiderately. If the records of surgery and medical jurisprudence are silent upon the subject ; if the learned doctors of the Sorbonne, the fathers of the Royal Academy of Paris, and the Fellows of the Royal College of London, have left us no precepts ; and if the experience of the present day furnishes no examples ; all this, and much more, does not prove that the practice here recommended is not perfectly just and proper, and vindicated upon every principle of science and humanity.

A defective organisation of the external genitals is one of the most dreadful misfortunes that can possibly befall any human being. There is nothing that exerts so baneful an influence over his moral and social feelings, which carries with it such a sense of self-abasement and mental degradation, or which so thoroughly "maketh the heart sick," as the conviction of such an individual that he is for ever debarred from the joys and pleasures of married life, an outcast from society, hated and despised, and reviled and persecuted by the world. Nothing but the most perfect resignation, and a well-founded confidence in the mercy and justice of the Creator, can render the lot of such a being at all supportable.

The subject of doubtful sex is one which has always, in all ages, and in all civilised countries, excited the warmest attention of the physiologist, the philosopher, and the medical jurist. Under the vague and ill-chosen name of hermaphroditism, invented at an early period of the world, was described every imaginable form of malformation of the genital and urinary organs, most dissimilar in character ; and, consequently, were calculated to mystify and mislead the public mind. A class of beings was imagined, combining, it was said, the qualities of the male and female in the same individual, and capable of performing, within itself, the generative functions. The idea that such a union might exist, had its origin, no doubt, in fable. The reader of mythology need not be reminded here of the story of Hermaphroditus and the nymph Salmacis ; how the former so ungallantly resisted the charms and entreaties of the latter, and how, finally, through the interposition of the gods, their bodies were united into one. The ignorance of medical men, the conceit and folly of legislators, and the mercenary conduct of many of the subjects of this variety of congenital malformation, served afterwards, in no small degree, to perpetuate the error thus engendered, and to transmit it, in nearly all its ancient force, down to a comparatively recent period. Modern researches had done much to dissipate these absurdities, when the publication, in 1836, of the great work of Mons. Isidore St Hilaire, entitled "*Histoire des Anomalies de l'Organisation*," set the long agitated question for ever at rest, by demonstrating, in the most undeniable and conclusive manner, that there is no such thing as hermaphroditism, in the vulgar acceptation of the term ; or, in other and more philosophical language, that the union of perfect male and female organs in one and the same individual, is an anatomical and physiological impossibility.

Much prejudice, leading often to the most cruel persecution, existed against this class of individuals among some of the nations of antiquity. The Athenians had a law, providing that all hermaphroditic children should be consigned to the flame ; while the Romans ordained that they should be boxed up, and thrown into the sea. In more recent times, all individuals of this description were excluded from holy orders, and from the office of judges, "because they were ranked with infamous persons, to whom the gates of dignity should not be opened."<sup>1</sup> Much of this prejudice has, fortunately, disappeared, under the benign influences of Christianity and civilisation ; but

<sup>1</sup> Beck's Medical Jurisprudence, vol. i. p. 106, fifth edition, 1835.

much still remains, and must continue in operation, as long as the human mind retains its present organisation. If hermaphrodites are no longer burnt and drowned, stoned and persecuted, and mocked and reviled, they are universally regarded with a degree of prejudice, amounting generally to positive aversion; and as unfit for any offices of dignity, divine, legal, or political. If such be the fact, and no one can doubt it, every suggestion, calculated to ameliorate the condition of this unfortunate class of beings, by depriving them of their only incentives to matrimony, and thereby dooming them to everlasting celibacy, should be hailed as a valuable contribution to the science and humanity of the present age.

[We have willingly given place to the above communication, not only because it is a very interesting and curious one, but also from the respect we entertain for the opinions of its distinguished author; but in doing so we may be allowed to say, that we cannot feel satisfied with the soundness of his argument in the present instance, and that while we will leave the discussion of the subject to those more competent to the task, we may add, that it appears to us the administration of prussic acid to terminate the sufferings of those afflicted with malignant disease, or who have received severe and irremediable injuries, might be justified by the same train of reasoning.—EDITOR.]—*The American Journal of Medical Sciences*, October 1852.

## PHYSIOLOGY.

### *Experimental Researches applied to Physiology and Pathology.*

By E. BROWN-SEQUARD, M.D., of Paris.

#### I.—ON THE SOURCE OF THE VITAL PROPERTIES.

I think that every tissue possesses its vital properties, in consequence of its peculiar organisation, and that in a completely developed animal, nutrition is the source of the vital properties, inasmuch as it is the cause of the maintenance of organisation.

I will try to prove the correctness of my opinion, by the following remarks on some of the vital properties of the spinal cord, the nerves, and the muscles.

##### a.—*Source of the reflex faculty of the spinal cord.*

Notwithstanding the experiments of Redi, Whytt, Prochaska, Unzer, Sénac, Fontana, Caldani, Sir G. Blane, Fray, Legallois, and many other experimenters; and notwithstanding the much more important experiments of Marshall Hall, Müller, Grainger, Volkmann, Kürschner, Pickford, de Martino, Buchner, Mayer, Paton, and Stilling, the existence of the reflex faculty, after the spinal cord has been separated from the encephalon, is not considered by all physiologists as a proof of the independence of the spinal cord. J. W. Arnold and Flourens, still maintain that the medulla oblongata is a centre, giving life to the other parts of the nervous system. The reflex faculty possessed by the spinal cord after it has been separated from the encephalon, is considered by J. W. Arnold as a remainder of something given to the spinal marrow by the encephalon, before their separation.

My experiments prove the incorrectness of that opinion.<sup>1</sup> I have found that after having exhausted the reflex faculty by putting it in action, energetically and frequently, in an animal on whom the spinal cord is separated from the encephalon, it reappears, and becomes soon as energetic as before,

<sup>1</sup> See :—*Recherches et expériences sur la physiologie de la moelle épinière*. Thèse inaugurale. Paris, 3 Janvier, 1846.—*Comptes rendus des séances de l'Académie des Sciences*. Paris, 1847. T. xxiv. p. 840.

provided that the circulation of blood takes place in the cord. Moreover, I have found, that if the reflex faculty is put in action frequently, it is able to produce an immense quantity of action: thus, for instance, it can stimulate sufficiently the muscles of a frog's leg to make them raise, in an hour and in divided portions, about twelve pounds, to the height of about two lines. In a pigeon the reflex faculty is able to stimulate the muscles of a leg, so far as to make them raise fifty pounds, by fractions, in an hour, to the height of more than one inch.<sup>1</sup>

I shall add two other decisive proofs:—1. The reflex faculty is very weak in frogs immediately after the spinal cord has been separated from the medulla oblongata, and it increases afterwards, as R. Whytt and Marshall Hall have discovered. I have stated that it increases so much, that the posterior limbs are able to draw up, by reflex action, more than double the weight the animal could raise up by an action of its will before the division of the cord. 2. After having divided the spinal cord in the dorsal region on a mammal, I kill it by cutting the right carotid artery. A few minutes after the cessation of reflex action I inject blood by the opening made in the carotid. Then life returns, and with it the reflex faculty.

All these facts demonstrate positively that the reflex faculty is a vital property belonging to the spinal cord, and that its source is in the nutrition which maintains the organisation of that nervous centre.

#### b.—*Source of the vital property of the motor nerves.*

The independence of the motor nerves is denied by almost all physiologists. They believe that the nervous centres are the sources of the vital property of these nerves. They base their opinion on this fact, that the motor nerves separated from the nervous centres soon lose their property, as it has been seen by Fontana, Haighton, Astley Cooper, Steinrueck, Müller, Sticker, Günther, Schoen, Kilian, Stannius, Helmholtz, Martin-Magron, and others.

But, *in the first place*, if the motor nerves of the warm-blooded animals lose their vital property after having been separated from the nervous centres, it is not less positive that they retain it during several days. *Secondly*, If the vital property of the motor nerves is exhausted by very energetic action, it re-appears after a short time, although the nerves are separated from the cerebro-rachidian centre, provided that the circulation of blood continues in them. *Thirdly*, If the circulation of blood is stopped in a limb in which the nerves have been divided, it is found that the peripheric portions of the divided nerves lose their vital property before the muscles. After the nerves have been left dead, i.e., deprived of their vital property for a quarter of an hour, half an hour, and even more, blood is allowed to circulate anew in the limb. Then the vital property of the cut nerves returns, and, to produce a muscular contraction, only a slight compression upon them is necessary.<sup>2</sup> If the motor nerves lose their property when they are separated from the nervous centres, it is because they are then badly nourished. Nerves, as well as muscles, must be exercised, in order to be well nourished.

#### c.—*Source of the muscular contractility.*

Although there are some facts which appear strongly to prove that the vital property of the muscular tissue is independent of the nervous system, many physiologists persist in their opposition to Haller's doctrine on this subject. Therefore, I have thought necessary to add new proofs to those already known,

<sup>1</sup> See Gaz. Méd. de Paris. T. 4. 1849. P. 233.

<sup>2</sup> See Comptes rendus de l'Acad. des Sciences. T. xxxii. Séance du 9 Juin, 1851.—Gaz. Médic. de Paris. 1851. T. vi., p. 359.

and I have published many experiments, of which I shall relate here only two of the most decisive.<sup>1</sup>

1. The sciatic and the crural nerves having been resected, for ten or twelve days, on a rabbit or a guinea-pig, I examine if these nerves have completely lost their vital property, and if the muscles are still contractile. When this has been ascertained, I put a ligature around the aorta. Then muscular irritability disappears after a certain time, and cadaveric rigidity appears. Three quarters of an hour, or even an hour, after the complete disappearance of the muscular irritability, and the appearance of the *rigor mortis*, I cut off the ligature, and I find, after ten or fifteen minutes, that the rigidity disappears and the contractility re-appears. I need not say that the nerves do not regain their lost property. This fact clearly proves that the contractility is given to the muscles by blood, i.e., by nutrition, and not by the nervous system.

2. Many experiments have shown to me that muscles paralysed for five days or a little more, in consequence of the division of their nerves, remain much longer contractile after the death of the animal than the non-paralysed muscles. This would hardly be the case if the contractility was given to muscles by the nervous system.

## II.—RESEARCHES ON THE REFLEX FACULTY.

During the last seven years I have published many papers relating to the reflex faculty.<sup>2</sup> Among the facts which I have discovered I will mention the following:—

1. Grainger had found that the act of suckling can be executed by an animal deprived of its brain. I have found that even after the ablation of both the brain and the cerebellum, newly-born rabbits are able to suckle very well; which is a proof that suckling may be executed by reflex action.

2. It is commonly affirmed that the reflex power is much stronger in cold-blooded than in warm-blooded animals. This opinion is correct, so far as regards the contrast between Mammals and Batrachia (the animals usually compared); but it is incorrect if Birds are compared with Reptilia and Fishes. It has been said that the higher an animal is in the scale, the less it has reflex power. If this be true, we should find more and more reflex power from Mammals to Fishes; but the real order, according to my experience, is,—1st, Fishes; 2d, Mammals; 3d, Amphibia and Reptilia; 4th, Birds; so that Birds have more reflex power than all the other animals, and Mammals have more than Fishes. Of course, there are exceptions to this rule in the case of particular species; thus the eel, carp, and tench have as much reflex power as many Mammals possess.

It has also been commonly affirmed that the reflex power diminishes with age, being the greatest in young animals. This statement, also, has been based on a too limited induction. In reptiles and fishes no difference can be detected in this particular. In birds it is decidedly the other way, the reflex power being much the strongest in adults. Among mammals the difference is usually in favour of the young animal; not, however, at the very earliest part of its life, but ten or twelve days after birth. As to man, the reflex power appears to be greater in him than in fishes and mammals; but it is not so energetic as in birds and in amphibia.

I have found that the causes of the differences between different animals, as regards the energy of their reflex power, are to be explained by anatomical differences. There exists a constant relation between the degree of the reflex power and the amount of gray matter in the spinal cord. It appears, also,

<sup>1</sup> See Bulletin de la Soc. Philomat., 1847, p. 74.—Gaz. Méd. de Paris, 1851, t. vi. p. 619, and 1852, t. vii. p. 72.

<sup>2</sup> See my Inaugural Dissertation, Paris, 3 Janvier 1846, 1<sup>re</sup> partie.—Comptes rendus de l'Acad. des Sciences, 1847, t. xxiv. pp. 363 et 859.—Gaz. Méd. de Paris, 1849, t. iv. pp. 430 et 644; et 1850, t. v. pp. 98 et 476.

that the mode of circulation of the blood in the spinal marrow has a great share in the causes of differences amongst different animals.

3. It is not necessary for the existence of the reflex power that the spinal cord should be without alteration. I have found the reflex faculty remaining in pigeons after I had crushed the spinal cord, and produced in it a considerable alteration. This is important to be known by practitioners, to prevent their drawing the conclusion, from the existence of reflex action after a fracture or a luxation of the vertebral column in man, that the spinal cord is healthy.

4. The influence of the nervous system on the secretions, by a reflex action, has been very little studied. I will state two examples of these reflex secretions,—1st, There is on the face, and particularly on the forehead and the nose, an abundant production of sweat when the nerves of the taste are strongly excited, as they are, for instance, by common salt, pepper, sugar, etc. In certain persons the quantity of sweat produced in such cases is sometimes, even in the winter, very considerable. 2d, I have observed that it is sufficient to excite the nerves of taste, in order to produce the secretion of gastric juice, bile, and pancreatic juice.

### III.—RESEARCHES ON THE INFLUENCE OF THE NERVOUS SYSTEM UPON THE FUNCTIONS OF ORGANIC LIFE.

My experiments have convinced me that if it is certain that the nervous system is able to act, and frequently does act, on the functions of organic life, it is not the less certain that the action of the nervous system on these functions is not necessary. I hope this will be sufficiently demonstrated by the numerous facts I have to relate.

#### a.—*Influence of the section of nerves on nutrition and secretion.*

1. The frequent occurrence of certain pathological changes after section of the sciatic nerve in mammals, has been cited as a proof of the dependence of the nutritive operations upon nervous agency. I think the following experiments give evidence against that doctrine. I have divided the sciatic nerve in a number of rabbits and guinea-pigs, and placed some of them at liberty in a room with a paved floor, whilst I confined others in a box, the bottom of which was thickly covered with bran, hay, and old clothes. In a fortnight, the former set exhibited an obviously disordered action in the paralysed limbs; the claws were entirely lost; the extremities of the feet were swollen, and the exposed tissues were red, engorged, and covered with fleshy granulations. At the end of a month, these alterations were more decided, and necrosis had supervened in the denuded bones. On the other hand, in the animals confined in the boxes, no such injuries had accrued; and although some of them have been kept living for four, five, and even six months after the division of the sciatic nerve, no alteration whatever has appeared in the palsied limbs except atrophy. In these cases a portion of the nerve had been cut off, so that reunion was nearly impossible, and did not take place.

Experiments made on pigeons have given the same results.

It is obvious, from these experiments, that the pathological changes which occur after the section of the sciatic nerve, do not proceed directly from the absence of nervous action, but that they are consequent upon the friction and continual compression to which the paralysed limbs are subject, against a hard soil, owing to the inability of the animal to feel or avoid it.

In similar experiments made on frogs, I found that no alteration took place, except when water penetrated through the wound, under the skin, and between the muscles.<sup>1</sup>

<sup>1</sup> See Gaz. Méd. de Paris, 1849, t. 4, p. 880.



2. With the help of an eminent micrographer (Dr Lebert), I have made researches on the influences produced on the capillary circulation, in consequence of the section of all the nerves (sympathetic and cerebro-spinal nerves) in the legs of a number of frogs. We have found no appearance of trouble in the capillary circulation, neither in an hour, nor in three or four days after the division of the nerves.

3. When resection of a long portion of one of the sciatic and the crural nerves is made on a very young rabbit, guinea-pig, or pigeon, the palsied limb continues to grow in length, but it grows only very little, if at all, in thickness. When the experiment is made on all the nerves of the wing in a very young pigeon, it is also found that the wing grows in length, but very little in breadth or in thickness. The secretion of quills takes place equally as well in the palsied limb as in the other.

The difference in all these cases between the length of the sound and that of the palsied limb or wing is never very considerable; nevertheless, the length of the healthy parts is greater than that of the paralysed parts.

4. I have found that burns, wounds, and ulcerations existing in parts palsied, in consequence of the section of their cerebro-spinal nerves, are cured as quickly and as well as those in sound parts.

5. Atrophy is a constant consequence of the section of the nerves of a limb. I have found that it supervenes not only in the muscles and the bones, as J. Reid has discovered, but also in the skin, which becomes evidently thinner.

6. Krimer asserts, that after the section of the nerves of a limb in mammals, the venous blood is of a bright red colour, like the arterial blood. (*Physiologische Untersuchungen*, Leipzig, 1820, p. 138, exp. 1, and p. 151, exp. 9.)

Long before the publication of Krimer, Arnemann had declared that the blood appeared darker than usual in a limb on which all the nerves had been cut. (*Versuche uber die Regeneration an lebenden thieren*, Gottingen, 1786, t. i. p. 48.)

Longet (*Traité de Physiologie*, Paris, 1850, t. ii., B. p. 92,) says that he has seen the venous blood retaining its ordinary colour even three days after the section of the nerves of the anterior limb in dogs.

Who is right—Krimer, Arnemann, or Longet? Neither of them is perfectly right. The assertion of Arnemann is entirely incorrect. By experiments made on dogs, rabbits, guinea-pigs, and pigeons, I have found that the venous blood in palsied limbs is evidently less black than it is in sound limbs. But it is not true to say that venous and arterial blood in paralysed limbs have the same colour. It is always very easy to distinguish the one from the other.

The transformation of the arterial blood into venous is not so complete in the palsied as in the sound limb, but it always takes place even in a great measure. There is a good proof of this in the result of my experiments on the hand and fore-arms of two decapitated men. I injected blood in the arteries of these parts, thirteen or fourteen hours after death, and when cadaveric rigidity existed. Surely there was in that case no nervous action whatever, and nevertheless the blood, which was of a bright red colour when injected, came out nearly black from the veins!

From all these facts I shall conclude:—

1st, That the nervous action (that of the sympathetic as well as that of the cerebro-spinal nerves) is not necessary for the change of colour of the blood in the capillaries.

2d, That the nervous system of animal life has an influence upon nutrition by which it takes a share in the transformation of arterial into venous blood.—*Philadelphia Medical Examiner*, August 1852.

(To be continued.)



## PRACTICE OF PHYSIC AND PATHOLOGY.

OBLITERATION OF THE RENAL VEINS IN SOME DISEASES OF THE KIDNEYS, AND  
IN ALBUMINOUS NEPHRITIS IN PARTICULAR. BM. E. LEUDET.

Obliteration of the renal veins has been little attendend to in almost all the modern works on diseases of the kidneys, and most frequently the description of the state of the vessels is omitted in the history of affections of this nature. M. Rayer (*Mal. des Reins* III. 330), was the first to give a complete history of it; since the publication of that work, some observers have published similar facts, but isolated and few in number. Two cases of obliteration of the renal veins in albuminous nephritis, directed the attention of the author towards this subject. Wishing to know what was the relation of cause and effect between obliteration of the emulgent veins and alteration of the texture of the kidney, he compared his own cases with those already on record. It was important, in order to obtain some data as to the importance of this lesion, to know if it was peculiarly connected with the alteration of the texture of the kidney, or if it was common to several affections of that organ.

Anemia of the kidney, without other change of texture, is rarely observed in obstruction to the course of the venous blood by obliteration of the renal vein. M. Rayer (*loc. cit.*, p. 594), quotes a case of this nature.

Hypertrophy of the kidney is much more frequently met with under such circumstances. Rayer (*loc. cit.*, 591, 592), has made known two examples of this form of lesion. In the first, it is said, that the left kidney was large, the cortical substance exteriorly of a very marked red, as in the early stage of acute nephritis; the tubular portion was itself of a violet red. In the second case, the kidneys, both very large, presented an appearance, which approached to that of Bright's disease.

Nephritis has been observed twice in connection with coagulation of blood in the renal veins. One of these cases is recorded by Dance (*Archives Gén. de Méd.* xxx. 24), the other by Rayer (*loc. cit.*, 593).

Albuminous nephritis coincides most frequently with this state of the veins. M. Rayer says, that he has frequently seen the veins filled with fibrous concretions, and with their walls thickened. In 1842, Dr Stokes showed to the Pathological Society of Dublin (*Dublin Journal*, xxxi. 144), two kidneys affected with Bright's disease, where there was obliteration of the emulgent veins by bloody clots. At a subsequent meeting, the same physician showed to the Society a similar lesion; but the clot was in this case less decolorised, and less firm than in the others; and, in a third specimen, from a patient who had presented the symptoms of albuminuria, the coagulum was dense and firm. Dr Peacock (*Med. Chirurg. Trans.*), in reference to obstruction of the inferior vena cava, mentions a case of obliteration of the right renal vein. In this case the two kidneys, especially the right, presented the lobulated and granular appearance characteristic of Bright's disease. M. Delaruelle (*Bull. de la Soc. Anat. de Paris*, 1846), has recorded a case which M. Leudet includes in this category, although the author, wrongly in his opinion, separates it carefully from the albuminous nephritis. To these cases M. Leudet adds the two which follow.

Obstruction of the renal veins sometimes has for its cause propagation of inflammation from the pelvic veins after delivery. M. Rayer records one of this kind (*loc. cit.*, 596), and cites others from Dugès, Velpeau, and Robert Lee. M. Cruveilhier (*Atlas d'Anat. Path.* Liv. 2, xxvi. pl. 5), mentions a similar instance observed by him, in a woman who died of puerperal fever. The

phlebitis was entirely limited to the renal vein, and did not extend beyond its opening into the cava. The clot was adherent throughout all the trunk of the renal vein, but not adherent in its divisions. In the centre of the clot there was some coherent pus.

Obliteration of the renal veins is, therefore, not by any means a lesion peculiar to Bright's disease; yet it results from the facts quoted above, that this state of the veins seems to coincide with this more than with any other affection of the kidney. Still, this coincidence is rare; for during several years that the author's attention has been devoted to this subject, he has met with only two examples of it. The nature of the clot—its mode of adherence to the walls of the vessels,—the state of these membranes, are described in an incomplete manner in most of the recorded cases. In a case of M. Rayer (*loc. cit.*, vol. ii. 272), the clot formed of superimposed concentric layers, had a little canal in it. In another kidney where the disease was less advanced (*ibid.* iii. 592), the concretions whitish and fibrous in the interior, were a little red externally, and perforated in their centre by a small canal, along which the blood could flow; the veins were not thickened. In one of Stokes' specimens, the clot was only partially decolorised, adhering at some points to the parietes of the veins, whilst otherwise it was quite unattached. Lastly, in another kidney, described by the same pathologist, the clot was more dense and solid, and the walls of the veins notably thickened. In the first of the author's cases the clot adhered to the wall of the vein; in the second, the connection of the clot with the vessel was not recorded. These details as to the clot, its mode of adherence to the parietes of the vessel, and the state of the vein itself, prove that in more than one case the lesion has not appeared to be of inflammatory origin, *i. e.*, caused by inflammatory action in the vein.

Different alterations of the parenchyma of the kidney accompany these variable degrees of obstruction to the course of the venous blood. But one character which prevails in most of the cases is the augmentation in volume of the organ; it was met with in the cases cited by M. Rayer, and also in the two cases now added by the author. In three cases out of four the yellowish granulation of Bright's disease was very manifest.

Various opinions have been expressed as to whether this is a coincidence, an effect, or a cause of the Bright's disease.

In reference to a case already quoted (vol. iii. p. 552), Rayer says, "It is probable that the augmentation in volume of the kidneys, and the moist state of their tissues, was the *result* of an affection of the renal veins." The same pathologist (ii. 268), in speaking of albuminous nephritis says, "The renal vessels frequently present appearances which seem to be, at least in most cases, an *effect* of the extension of the inflammation of the cortical substance."

Thus, then, according to Rayer, the obliteration of the veins may be, according to circumstances, either the cause or the effect of the renal disease.

Several years after the publication of M. Rayer's work, M. Delaruelle came to the conclusion that the primary lesion was situated in the veins, and was the result of a phlebitis. M. Leudet classes Delaruelle's case with those of albuminous nephritis, although the latter does not so consider it; and M. Leudet, as sufficient evidence in support of this opinion, observes, that in the patient, who died after having albuminous urine, dissection showed the substance of the kidneys to be pale, and presenting, to a certain extent, the colour of fatty liver.

F. T. Frerichs (*Die Bright'sche Nierenkrankheit* Braunschweig, 1851,) considers that the obstruction offered to the course of the blood by obliteration of the renal veins, is the cause of the presence of albumen in the urine, and the source of a more or less rapid disorganisation of the kidneys. In speaking of the etiology (*ibid.*, 160), Frerichs places among the causes of the disease, obliteration of the renal veins by clots of blood, or their compression by tumours. To prove this point, he made experiments on animals, they were repeated ten times on rabbits, one cat, and two young dogs. He tied the renal vein, or compressed

it with pincers; in one case he tightened the ligature incompletely, in another the vena cava was tied above the liver. The urine extracted from the bladder or ureters, from a quarter of an hour to six hours after the operation, always contained a greater or less amount of albumen, mixed in three instances with blood. In these different experiments Frerichs tied only one of the renal veins; the kidney from which the blood could not flow away was always considerably increased in weight; but the experiment was never sufficiently prolonged to produce a deeper alteration of the parenchyma of the kidney. Frerichs, in his work, quotes Robinson, (Med. Chir. Trans., xxvi.), as having made similar experiments, and also H. Meyer, (Zeitschrift für Physiologie, 1844). M. Leudet regrets that the state of the kidneys had not been observed at a period more distant from the operation. He does not dispute that ligature of the renal veins may produce albuminuria, but the question which remains to be decided is, will it produce Bright's disease? M. Leudet draws the following conclusions:—

1. Obliteration of the renal veins occurs in some forms of the disease of the kidney, and even in the state of health. (?)

2. It *appears* to be most frequent in albuminous nephritis.

3. It is accompanied almost constantly with an increase in volume of the kidney.

4. It is difficult to say if the lesion in the veins is the cause or the effect of the albuminous nephritis.

The following are the details of M. Leudet's two cases:—

*Albuminous nephritis—intestinal hemorrhage; death; clots in the right renal vein, the cava, the external iliac and femoral of the same side—coagula of less marked character on the left side.*

CASE I.—Jean Marie Thevenin, laceman, aged 24, with brown hair and eyes, and pale complexion, was admitted into La Pitié, 11th December 1851, under M. Gendrin; has habitually enjoyed good health, and has followed the business of lacemaking for more than ten years; has never been long confined to bed by any severe disease; his food has always been sufficient and of good quality; his residence is not damp; has never suffered from rheumatism or palpitation.

He has observed, without knowing any cause for it, that for seven years, after a hard day's work, his legs have been slightly swollen about the ankles. He has never observed any œdema of the face or inferior extremities. No other morbid phenomenon, connected either with the cerebral, digestive, or urinary systems, has ever attracted his attention till within the two months preceding his admission to the hospital.

Two months ago, without any known cause, he observed that the expulsion of the urine, which took place without any pain, was followed by the discharge of two or three drops of blood. This discharge of blood, which was trifling in amount, and did not recur after each discharge of urine, lasted for seven or eight days, and did not re-appear. He never had previously observed anything similar. At that time he felt in both lumbar regions a heavy, almost constant pain, which has lasted ever since. About two weeks after these first symptoms, without any febrile phenomena, the œdema appeared in the legs, also in the arms and face, but to a much greater extent than formerly, and this œdema went on steadily increasing, and forced him to keep his bed for more than a month. Fifteen days before admission into hospital, gastro-intestinal symptoms appeared, at first diarrhœa, the stools being liquid and mixed, with bloody streaks, nine or ten times daily; the appetite disappeared; and some days afterwards vomiting occurred, the patient rejecting all solid and liquid ingesta. A slight cough, but unattended by pain, supervened at the same time. The organs of special sense were unaffected.

On the 11th December Thevenin was in the following condition:—Skin pale and ghastly; considerable œdema occupies the inferior extremities, the scrotum, the abdominal parietes, and loins; it exists also, but to a less marked

extent, in the arms and in the face, which is puffy. The heart beats feebly; there is heard on a level with the base a feeble bellows murmur extending along the aorta and the two carotids, where it is continued, and in a marked degree augmented.

The urine pale, without sediment, gives an abundant whitish precipitate with nitric acid, and with heat lets fall a deposit which, after 12 hours' rest, occupies one-half of the liquid.

There is cough; there is marked dulness over the inferior third of the right side of the chest, with complete absence of respiratory murmur and œgophonic prolongation of the voice. On the left, at the back, there is coarse subcrepitant râle, both in inspiration and expiration.

There is no headache, no affection of the sight, and the patient has had no epileptiform or nervous attacks of any kind. During the three first days the diarrhoea diminished, and the vomiting disappeared. On 14th December the patient vomited his breakfast, and in the evening complained of general discomfort and headache. The pulse, generally quiet, was at 92, neither full nor strong. There was increased heat of surface. The pain in the lumbar region and the soreness on pressing was not greater.

15th.—A large patch of erysipelas appeared on the loins, extending over the right hip. The skin here is of a pale red colour, and less depressible than elsewhere. The fever continues, the general discomfort is more marked, and he is depressed.

16th.—The erysipelas is extended, and diarrhoea has returned to the extent of ten yellow motions.

17th.—The depression is great; pulse 110, feeble and small; erysipelas continues. During the day the diarrhoea increases; the stools are reddish and clear, mixed with some bloody clots, and numerous blood corpuscles are detected in them by the microscope. The œdema is in the same state; there is effusion into the abdomen. No blood in the urine, but abundance of albumen.

18th.—The bloody stools continue, but the dejections are of a darker red, and do not contain any bloody clots. The erysipelas is much diminished.

20th.—The erysipelatous redness has disappeared; the stools are less frequent; the œdema persists; and during the subsequent days the patient is so exhausted as to be unable to sit up in bed without assistance.

25th.—At two A.M. the patient was seized with rigors and delirium, with violent agitation and foaming at the mouth. This attack, which was imperfectly described by the nurse, lasted for a quarter of an hour, and went on to coma, which proved fatal in two hours.

*Autopsy*, 26th December, 27 hours after death.

Cadaverous rigidity marked both in upper and lower extremities; no appearances of putrefaction.

*Brain*.—No congestion of the meningeal vessels; little subarachnoid effusion; cerebral substance with few red points, and of normal consistence.

*Thorax*.—Larynx and bronchi lined with frothy mucus, otherwise healthy. The right pleural cavity contained about a glassful (*un verre environ*) of serum, containing some fibrinous flocculi; some soft false membranes covered the surface of the lung; no effusion in the left pleura. The two lungs, quite unadherent, of a reddish gray in front, without any tubercular deposit, were slightly engorged at the base; their tissue of a brownish gray, heavy, not very crepitating, depressible, not sinking in water, giving out abundant frothy serosity on being cut into. The pulmonary veins and arteries healthy.

Pericardium contained about a spoonful of yellow transparent serosity; no false membrane on it or on the outside of the heart. The heart, of natural size, without any trace of fatty degeneration, was healthy; on the mitral valve, near the insertion of the tendons of the columnæ carneæ there was observed a single small reddish vegetation, of the size of a lentil, lying under the lining membrane of the heart. Each ventricle contained a little dark fluid blood.

**Abdomen.**—The peritoneum contained about  $2\frac{1}{2}$  pints of clear serum. No appearances of peritonitic inflammation. The stomach, at its larger end, presented a fine punctiform redness, but no vascular arborescence. At this point the mucous membrane was soft; elsewhere it was of natural consistence.

In the small intestines, the mucous membrane of which did not exhibit any softening, there was observed here and there some fine vascular arborescence. In the large intestines, especially in the cœcum, these arborescences were more numerous, in the form of patches, marked with dark ecchymotic spots, with slight softening of the mucous membrane near them, but no ulceration. The contents of the bowels were yellowish and liquid.

The liver was adherent to the abdominal parietes, rather large, and of pale yellow colour, but not fatty. The spleen small, red, and firm. The two kidneys were large.

	Right Kidney.	Left Kidney.
Length, English inches,	6·2	6·2
Breadth,            ,,	2·5	3·3
Thickness,         ,,	1·3	0·4

**Right Kidney.**—The fibrous tunic adhered to the organ more closely than natural. The external surface, smooth before and behind, presented on the convex edge some slight prominences. The whole of the organ was slightly reddish-yellow, scattered over with little reddish stellate arborisations, not forming regular figures. Some little yellow points, very discreet, were seen on the exterior, but much more marked on the interior of the organ. There was a considerable increase in thickness of the cortical substance. It was of a marked yellow, with little yellowish spots scattered throughout it, and few vascular arborisations. The pyramids were atrophied, and also yellowish. In the spaces between the pyramids, and also in the cortical substance, the veins could be traced, filled with clots of whitish decolorised fibrine. The lining membrane of these vessels was healthy. The clots hardly adhered to them, and could be easily detached.

The right renal vein was filled by a decolorised clot of equal firmness, not containing any trace of black blood, not adherent to the walls of the vessel, which were sound. In the inferior cava there existed a whitish clot, obstructing only about a third of the caliber of the vessel, stretching into the right external iliac artery, and terminating about the middle of the femoral vein. At the point of the termination of the two common iliacs in the inferior cava, there existed also a clot, but soft, and less adherent to the lining membrane. These vessels contained little dark blood. Their lining membrane was healthy. The arteries were nowhere obstructed by clots.

The left kidney presented the same appearances as the right. Clots equally fibrinous were found in it as in the right. The renal vein on this side was free. The pelves of the kidneys and the bladder were healthy.

*Albuminous Nephritis of slow progress—Obliteration of Renal Veins—Kidneys much enlarged—covered with yellow Granulations.*

**CASE II.**—A woman, aged 34, who had born no children for eight years, was admitted into La Charité, under M. Gerdy, for a slight burn of the arm. She stated that she had previously enjoyed good health. Towards the end of January, she appeared to be falling into an adynamic state. The lungs were examined, but nothing detected, except some trifling remains of bronchitis. The urine had never been tested.

She was transferred to the medical wards, under the care of M. Rayer. When seen by the physician, she was in a state of coma, and it was hardly



possible to extract from her a reply to questions. She had scarcely any œdema of the lower extremities; the respiration was short and embarrassed; and she expectorated transparent frothy mucus. Pulse very quick and small. Auscultation detected at the base of each lung, abundant subcrepitant râle; there was no dulness behind. The heart showed no signs of disease.

The patient died comatose two days after admission.

*Autopsy.*—Weather cool and dry; no cadaverous rigidity; no putrefaction.

*Head.*—Little congestion of the vessels of the integuments or meninges; subarachnoid effusion slightly greater than natural; membranes of brain not adherent; the cerebral tissue of normal consistence, and not showing any unnatural amount of red points.

*Thorax.*—A little congestion of the back part of the apices of the lungs, which is much more marked at the base; pulmonary tissue of a violet red, not friable, but a little heavier than natural, and emitting, on being cut into, a large amount of 'somewhat frothy sero-sanguinolent liquid. The bronchi contained a small quantity of pinkish frothy liquid.

*Heart* flabby, not fatty; substance and orifices healthy.

*Abdomen.*—Stomach and small intestines natural. In the cœcum there were a few superficial follicular ulcers, extending no deeper than the mucous membrane, and diminishing in number towards the colon.

The *liver* was not deep-coloured nor fatty, and was of natural consistence.

The *spleen* normal in size and structure.

The two *kidneys* were, to a remarkable degree, increased in size. When weighed, after being stripped of cellular tissue and fat, the one weighed 15½ ounces (troy), the other 14½. The proper tissue of the kidney, though it appeared thicker than natural, did not adhere closer than usual to the cortical substance. Externally, the kidneys appeared of a yellowish-red, without any star-like vessels, and exhibited a considerable number of small white granulations—Bright's granulations. On making a section, the cortical substance appeared of a yellow tint, which was rendered more apparent by its contrast with the bright redness of the tubular portion. The kidneys, on being cut into, did not exude any fluid. The Bright's granulations were seen in great numbers on the surface of the section, especially on the parts nearest the surface. The cones of the tubular structure were of a deep red-brown, and the tissue which formed them appeared to be condensed. M. Davaine made a microscopic examination, of which the following are the results:—Examined under a power of 350 diameters, the yellow cortical substance appeared to be composed principally of epithelial cells, more distinct than those observed in a healthy kidney. Most of the cells of these diseased kidneys contained fatty globules in variable quantity, and especially many granules (*globulins*) pressed close together.

Neither the trunk nor branches of the renal artery exhibited any marked change.

The trunk of the renal vein and its branches were filled with yellowish, solid, compact, fibrinous clots. The clot of the main trunk had a channel through it. Fragments of these clots, examined by M. Davaine, exhibited an amorphous layer of molecular granules, or globules resembling, more or less, blood corpuscles. A portion taken from the interior of these clots exhibited stellate crystals, having the appearance of margarine, and further, a great number of the globules resembled blood corpuscles.—*Gazette Médicale de Paris*, 30th October 1852.



## Part Fifth.

### MEDICAL NEWS.

#### EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING X.—*Wednesday, July 7, 1852.*—Dr BEGBIE, P., in the Chair.

*Dr Begbie* read a paper, entitled "Illustrations of Erysipelas," detailing a number of cases of that affection, some of which occurred in series, appearing to originate either in local miasma or in some contagious influence. In regard to treatment, *Dr Begbie* maintained the necessity of a tonic and stimulant treatment in many cases of erysipelas, and bore strong testimony to the value of the remedy proposed by Mr George Hamilton Bell and Dr Charles Bell,—viz., the internal administration of the tincture of muriate of iron. (See September Number of this Journal.)

A long discussion followed the reading of *Dr Begbie's* paper, in the course of which *Drs Seller* and *MacLagan*, and *Professor Miller*, maintained the necessity of active antiphlogistic treatment in many cases of erysipelas. *Mr Miller* preferred in all cases scarification of the part affected, performed early in the disease, to any other mode either of general or local blood-letting; and agreed with the opinion of Mr Liston, that the application of leeches was attended with considerable danger of renewed irritation and extension of the disease. *Dr Seller* thought that the muriate of iron, as a remedy in erysipelas, had justified its claim to investigation, and that, generally, the theoretical ideas formerly prevalent as to the exclusively antiphlogistic treatment required in erysipelas had been to a considerable extent modified by the experience of that disease in later times.

A very general desire having been expressed for additional information as to the therapeutical efficacy of the muriate of iron, especially in erysipelas,

*Mr Benjamin Bell* stated, that he had employed this remedy to a considerable extent both in erysipelas and some other affections of the skin. In one strongly marked case of erysipelas, with much inflammatory fever, the medicine had produced such an effect, that it was spoken of by the patient as a "cooling mixture." In albuminuria occurring after scarlatina he had used the muriate of iron with excellent effect, and had on a former occasion detailed the results of his experience to the Society. In *erythema nodosum* he had also found it useful; and in one case of *herpes zoster* affecting the scalp it had acted like a charm, the painful sensations suddenly subsiding after the first few doses.

*Dr Keiller* and *Dr W. T. Gairdner* also stated some facts bearing on this subject, the former having used the muriate of iron in small-pox, with good results; the latter having found it apparently remarkably effective in restoring the secretion of urea, and removing the tendency to coma, in a very complicated and rapidly advancing case of renal desquamation in Bright's disease.

*Dr Begbie* said that he had, in several of the cases in his paper, had recourse to abstraction of blood, and other antiphlogistic remedies; and he had no doubt of the applicability of this method to some cases of erysipelas. He observed, however, that while scarcely any practitioners maintained that the antiphlogistic treatment could be applied to *all* cases of erysipelas, there were not wanting physicians of high character who treated the disease on the tonic plan *exclusively*.

*Dr Keiller* read the details of a case of *erythema nodosum*, which illustrated the connection between that disease and the rheumatic diathesis.

*Dr J. W. Begbie* communicated the particulars of a case of abscess of the brain, connected with disease of the ethmoid bone. The symptoms were of several years' duration, and the appearance of the abscess on dissection suggested a doubt as to whether the abscess of the brain or the disease of the bone was the primary affection. *Dr Begbie* inclined to the latter opinion, and referred to similar cases of *Andral*, *Bright*, and others.

*Dr Strachan*, of Dollar, read a case of emphysema of the cellular tissue, with hydro-pneumo-thorax, producing rapidly fatal symptoms. The cause of the affection was somewhat obscure, but it was ascribed by *Dr S.* to the escape of air from the trachea into the posterior mediastinum.

### ROYAL MEDICAL SOCIETY.

On Friday, the 19th ult., the Royal Medical Society took possession of their New Hall, No. 7, Melbourne Place, on which occasion there were upwards of 200 members and visitors present, including Professors *Syme*, *Simpson*, *Gregory*, *Bennett*, *Balfour*, and *More*, and a large number of the Fellows of the Royal Colleges of Physicians and Surgeons.

At public business, the Senior President, *Dr Cobbold*,<sup>1</sup> addressed the meeting as follows :—

GENTLEMEN,—Both gratitude and honour prompt us, on an occasion like the present, to draw aside the curtain of forgetfulness, and advert briefly to the history of the past (!)

You are well aware that the establishment of the Royal Medical Society of Edinburgh was first brought about by the energy of a few enlightened individuals. "These youthful aspirants after truth perceived that it was not merely the frigid plodding over books, nor the doctrines and precepts of age and authority, nor the detail of empirical practice, that could inspire that taste and spirit, and give that manly tone to our inquiries which alone can render study agreeable, vigorous, and successful. They perceived that it was in *society* alone, by the mutual communication and reflection of the lights of reason and knowledge, that the intellectual as well as the moral powers of man are exalted and perfected."

During the first few years of the Society's existence, its weekly meetings were held at a tavern in the vicinity of the College; but, by permission of the managers of the Royal Infirmary, two apartments were afterwards provided in that Institution, one room being used exclusively as a library (which had already mustered some thousand volumes), the other employed for purposes of debate. Nearly forty years had thus expired, when the scheme for erecting a Hall was first set on foot by two of the Society's Presidents. This was seconded with liberality and zeal, both by its *then* present and former members, and ultimately led to the erection of the building in Surgeon Square, which at that time was one of the most flourishing districts in the Old Town. The foundation stone was laid by the illustrious *Dr Cullen*, on the 21st day of April 1775.

It would be foreign to our present purpose to enter into minute detail in connection with the history of the Society since that date. Such of you as are not familiar with its subsequent rise and progress, we would at once refer to *Dr Stroud's* history of the Medical Society, and also to the centenary oration delivered by *Dr Carpenter* on the 17th of February 1837.<sup>2</sup>

<sup>1</sup> The junior presidents are, *Dr W. H. Broadbent*, *Dr W. M. Dobie*, and *Dr J. M'Grigor MacLagan*.

<sup>2</sup> From *Dr Stroud's* invaluable treatise, and from *Dr Duncan's* "Addresses" and "Account of the Building of the Medical Society," these introductory statements are gathered.—T. S. C.

As regards the building itself, and the accommodation it was designed to supply, it can scarcely be admitted to have fully answered the calculations of its early projectors, inasmuch as they evidently anticipated that ample space would be found for the accumulating treasures of many successive generations; and little did they think, that ere those "time-honoured walls" had manifested signs of decay, its overgrown library, and the now loathsome state of that once influential and healthy locality, would necessitate our removal to a more eligible site.

In the year 1819, at a period when the Society's funds received great augmentation from the overwhelming demand for "*seats*," it was deemed expedient to make an extension of the premises. Accordingly, a numerous committee, comprising several of the Professors of the University, was appointed to promote and obtain subscriptions; but, before taking any decided steps in this direction, it appeared to them advisable to concert some definite plan, calculated to recommend itself to universal approval. After mature deliberation, they came to the conclusion, that the project of *enlargement*, if practicable, would be, in many respects, preferable to that of re-building, chiefly on the score of comparative expense.

An architect was consulted, who estimated the value of the Hall at L.1200, the expense of a proposed wing (which was to be on the north side of the building) at L.1000, and the erection of a new Hall at L.4000.

During the following recess, the committee presented a petition to the Town Council, requesting, on behalf of the Society, the grant of a considerable portion of ground in the direction we have already indicated. This petition was both strongly recommended and favourably received, but was eventually declined, from what (now, indeed) fully appears to have been a just apprehension, that its admission would occasion an injurious encroachment on the Royal Infirmary, the limits of which Institution have all along been too much circumscribed. After this defeat, the committee recommended the purchase of one of the opposite houses; but this suggestion was fortunately and very wisely abandoned.

Thus, gentlemen, we perceive that the Society, "*from a want of external facilities*," has suffered a temporary loss and interruption, extending over a period of "*more than thirty years!*" for we find that, from that date, there has been an almost constant decrease in the number of "*petitions for seats*" in each succeeding year; and this is mainly attributable to the circumstances here specified, and *not* from any want of zeal on the part of its members, as some have foolishly imagined.

From this state of embarrassment we are happily emerging; and the especial object for which we are this evening met, namely, "THE INAUGURATION OF THE NEW HALL OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH," will ever hereafter be regarded as an important era in its history.

If, gentlemen, the old Hall, shortly after its erection, was pronounced from the Chair to be "a grand phenomenon of medical ardour," what words shall sufficiently characterise the magnificence of the *palace* in which we are now assembled?

We will not further detain you with desultory comments on the suitability of the means now placed at our disposal. It behoves us rather to urge upon you the necessity of exerting your most strenuous efforts in the cause so ably advocated by our predecessors. We have received a thousand pleasures and advantages from their disinterested endeavours, and it is incumbent on us, and on every generation succeeding us, to enhance the value and reputation of this institution to the latest ages.

The grand objects of this Society are:—Mutual improvement and the investigation of truth; the development of the seeds of genius, and the detection of falsehood; the emancipation of the mind from the fetters of prejudice, and the cultivation of true friendship by social and liberal intercourse.

Such were the views and feelings of the founders of this Society; and the happy effects of it upon their character and conduct have been fully proved.

not only by their own testimony, but by that of their distinguished successors, many of whom honour us with their presence this evening ; and we venture to appeal to their experience, and to point to their position, as bearing ample testimony to the justice of these remarks.

We look to your exertions, gentlemen, as the guarantee of future success. Do not be slothful because it is often argued that merit cannot always ensure a corresponding reward. The insufficiency of merit and of honest endeavours to the acquisition of fame and fortune, will doubtless give occasion to the discontented to repine and to censure the economy of human affairs ; but they who are conversant in the investigation of final causes easily perceive that such a dispensation tends to perfect virtue by the exercise of patience. There are those who enter on the practice of our art totally destitute of preparatory instruction, and who make a merit of their defect. Without even those few lights which may be occasionally obtained in the course of a servile apprenticeship, they assume all the importance of sufficiency, and dictate with an oracular confidence. Against those vultures of mankind, against those harpies of society, who scatter pain and death around, under pretence of affording relief, and who, for the sake of supporting an unbecoming parade in life, not only delude, but destroy those who apply to them as friends under the pressure of the heaviest calamities, every honest mind must feel an indignant sentiment.<sup>1</sup>

But, gentlemen, we are not discouraged. Greatly, indeed, is it to be regretted that popular esteem is so often misplaced, and rather tends to encourage bold, presumptuous, and unblushing ignorance, than to raise true merit from the vale of obscurity. Let us persevere in the profession in which honour and usefulness have induced us to embark, and so in the end shall we prove ourselves to have been worthy followers in the footsteps of the immortal founders of the Royal Medical Society.

After the address, Professor Simpson communicated to the Society the results of a series of experiments on living animals, with a view of ascertaining the true nature of uterine contractions in the parturient female. An interesting discussion ensued, in which Dr Bennett, Dr M. Duncan, Dr W. T. Gairdner, Dr Simpson, and several visitors took part ; after which Mr J. F. Macfarlan moved a vote of thanks to Dr Simpson, which being heartily responded to, the Society adjourned.

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#### DR MUSPRATT—RECLAMATION.

Dr Sheridan Muspratt has called upon us to state, that the memoir reviewed at p. 387 of our October Number was the production of Mr Richard Brinsley Knowles, and that Dr Muspratt did not write a line of it.

We can have no hesitation in giving Dr Muspratt the benefit of this explanation.

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#### PROSPECTS OF THE EDINBURGH MEDICAL SCHOOL.

It is once more our pleasing duty to record an increase in the number of medical students attending classes in the Edinburgh University. The numbers of the matriculated on 1st December 1852 were 497 ; being an excess of 73 over 1851. We may add, that for some years past the increase has been progressive, and that, too, in spite of the allurements of the "diggings,"—which are said to have diverted no small number of students from the benches of the metropolitan schools.

<sup>1</sup> Vicesimus Knox.

## OBITUARY.

## DEATH OF SIR WILLIAM NEWBIGGING.

WE have to record the death of this highly respected surgeon, which took place at his residence in Edinburgh, on the 23d of October last. Mr Newbigging was born at Lanark in 1772; and, after pursuing his studies at Edinburgh, became a Fellow of the Royal College of Surgeons of Edinburgh in 1799. He was for many years one of the surgeons of the Royal Infirmary, and was esteemed a dexterous operator. He had much reputation as a lithotomist, and attracted the attention of the profession by a successful case of ligature of the external iliac artery, an operation which has frequently been performed since that time, but which then had only been executed once previously by Mr Abernethy. Mr Newbigging was extensively engaged in private practice. He was distinguished by an urbanity of manner, which secured for him the esteem of his professional brethren, and a cheerfulness of disposition which he retained to the end of his life, and which helped to sustain him under a succession of painful bereavements in his family. He was at all times anxious to maintain the honour and dignity of his profession, and ever took a warm interest in the progress of medical and surgical science, being always ready to avail himself, through others, of those more recent aids of the practitioner, such as the stethoscope and microscope, with which, at his time of life, and amidst his occupation as a practitioner, he had not the opportunity of becoming personally conversant. Mr Newbigging received the honour of knighthood from her present Majesty at her coronation, and as Sir William Newbigging continued actively engaged in his profession till four or five years ago, when his advanced age led him to retire from the active duties of practice. In December 1850 he had the misfortune to fall and fracture the neck of the femur. From this, however, he made an excellent recovery, and enjoyed good health, and the perfect use of all his faculties till a few months ago, when he gradually declined, and died on 23d October.

Sir William was a Fellow of the Royal Society of Edinburgh, was formerly President of the College of Surgeons, and Vice-President of the Wernerian Society. The interest which he took in the cultivation and advancement of useful knowledge led him, when his retirement from practice left him more at leisure, to become a Fellow of the Royal Scottish Society of Arts when he was in his 77th year.

Sir William Newbigging had the privilege of pursuing a longer career of public and private usefulness than falls to the lot of most men. He has left behind him a character for unsullied integrity, and he will long be respected in the memory of those who knew him, as a professional man, a gentleman, and a citizen.

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ALEXANDER BELL, ESQ., OF DUNDEE.

The late Mr Bell, M.R.C.S. London, was born in the year 1775, in the town of Cupar. His professional studies were prosecuted at the University of Edinburgh, and completed at a later period in London, under the tuition of Sir Astley Cooper, and Mr Saunders the oculist.

He served in Ireland, as surgeon and lieutenant in the loyal Tay Fencibles, during the memorable rebellion of 1798, and until the regiment was disbanded in 1802. He commenced private practice in the village of Errol, in Perthshire; and in 1807 removed to Dundee, where his talents and zealous attention

to his professional duties soon laid the foundation for an extensive and lucrative practice, which he enjoyed for the long period of forty-three years. Two years before his death, he was obliged to relinquish practice by an attack of paralysis.

In general society, Mr Bell's frank and manly bearing made him a general favourite; and to his numerous pupils and apprentices he endeared himself no less by the kind interest which he manifested in their professional education and prospects, than by the excellent example which he set before them of what a professional man should be.

As a surgeon, general practitioner, and consulting physician, he successively distinguished himself. For upwards of thirty years he officiated as surgeon to the Dundee Infirmary, and by the inmates and supporters of that institution his services were gratefully received and thankfully acknowledged.

Mr Bell died on the 22d March 1852, in the seventy-sixth year of his age.

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|--|--|
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| <b>On Contamination of Water by the Poison of Lead.</b> By James Bower Harrison, M.R.C.S.L. London: Churchill. 1852.   | <b>The Dictionary of Domestic Medicine.</b> By Spencer Thomson, M.D. Part XI. London: Groombridge & Sons. 1852.  |
| <b>Practical Sketch of the Asiatic Cholera of 1848.</b> By S. F. Statham, Assistant-Surgeon University College Hospital. London: Taylor, Walton, & Maberley. 1852.                           | <b>Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session held in the City of Philadelphia, May 1852.</b> Vol. II. Philadelphia: Collins. 1852. |
| <b>Stricture of the Urethra.</b> By Robert Wade, F.R.C.S. London: Churchill. 1852.   | <b>Observations on the Treatment of Lateral Curvature of the Spine.</b> By Edward F. Lonsdale, F.R.C.S. London: Churchill. 1852.   |
| <b>Introductory Lectures, delivered at the Leeds School of Medicine, at the opening of the Twenty-Second Session, October 4, 1852.</b> By Thomas Meneley, F.R.C.S.E. London: Longmans. 1852. |  |



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